ESSAYS ON PUBLIC ECONOMICS

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This dissertation covers a number of topics in public economics, specifically dealing with program evaluation and the private provision of public goods. The first chapter deals with the evaluation of a prominent federal policy: the Federal Healthy Marriage initiative. This program funded organizations at the community level, enabling the provision of relationship and marriage education in an effort to promote and sustain marriages. I identify the impact of funding on marriage and divorce, considering the possibility of heterogeneous treatment effects across racial, ethnic, and socioeconomic groups. I find that increased funding reduces the likelihood of marriage while also decreasing the likelihood of divorce amongst certain groups. In the second chapter, Randall Walsh and I evaluate a series of high-profile programs known as “Promise scholarships” in which private organizations guarantee money towards the costs of attendance at selected colleges and universities provided that a student has resided and attended school within a particular public school district continuously for at least the four years prior to graduation. Our estimates indicate that K-12 public school enrollments increase in Promise zones relative to their surrounding areas following Promise announcements, schools associated with merit-based programs experience increases in white enrollment and decreases in non-white enrollment. Furthermore, housing prices increase following announcement, with the largest effects in neighborhoods with high quality schools in the upper half of the housing price distribution. These patterns lead us to conclude that such scholarships are primarily affecting the behavior of high-income households and that merit-based versions disproportionately impact white households. Finally, in the third chapter I examine the incentives for private firms, such as those sponsoring Promise scholarships, to participate in the production of public goods, specifically through sponsoring fundraising lotteries and raffles. I develop a model where firms have advertising incentives to contribute to public causes, deriving predictions about public goods production under various plausible advertising allocation mechanisms including exclusive sponsorships. I test these predictions in a laboratory experiment ultimately finding that, while exclusivity is sometimes beneficial in the theoretical environment, non-exclusive arrangements unambiguously generate more revenue for the public good empirically.

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PREFACE

This dissertation was written under the careful and expert guidance of my dissertation committee: John Duffy, Randy Walsh, Lise Vesterlund, Werner Troesken, and Sera Linardi. Since becoming his research assistant in my third year of doctoral studies, I have received an indispensable education— as well as important financial support— from John. His advice, especially on all matters experimental, was always welcome. In addition, his professional guidance made the transition from student to researcher much easier than it could have been. As my co-advisor (and co-author on the second chapter), Randy never failed to provide insightful comments both on this work and on my career, specializing at times in those that may be difficult to hear. I will be forever grateful to him for providing an endless source of motivation and encouragement throughout this process and for providing the resources to make the second chapter of this dissertation possible. Lise receives the lion's share of the credit for inspiring and enabling the third chapter of this dissertation. Not only did the idea for the project spring from my interactions with her both in and out of the classroom, but Lise also provided much of the financial support necessary to conduct experiments and collect data. But beyond that, Lise has provided invaluable feedback on all of my work; without her input, this dissertation would be a shadow of what it is today. Werner and Sera have also provided terrific suggestions for my work in numerous presentations and seminars over the past few years. Their contributions are also visible throughout this collection of essays.

In addition, I would like to thank my parents, Donald and Helen LeGower, for always

encouraging me on this path and providing the environment for me to thrive. Finally, I would also like to thank my wife, Elizabeth DiGregorio. Without the support of a friend like her during the trials of graduate school, I do not know what I would have done.

1.0 ASSESSING THE IMPACT OF FEDERAL HEALTHY MARRIAGE INITIATIVE GRANTS

"And so, ... in my budget, I have $300 million on an annual basis to support education programs and counseling programs ... all aimed at encouraging marriage; all aimed at helping couples to build and sustain healthy marriage in our society." -George W. Bush

1.1 INTRODUCTION

From late 2006 to 2011, the U.S. Department of Health and Human Services issued close to $100 million per year in grants to local and state organizations in order to promote marriage amongst unwed couples with children and foster healthy relationships among currently married couples. Some individual organizations in targeted counties around the country have received awards of $2 million per year for programs developed to provide marriage education in the community. The motivation for this campaign is the expectation that marriage can lift households out of poverty and that stable, two-parent families confer lifelong benefits on children. Research has shown that, on average, married men earn more than unmarried men (Stratton, 2002; Antonovics and Town, 2004) and children raised in single-parent households have worse outcomes than those that grow up with two parents (Amato, 2005; Brown, 2004; McLanahan, 1994). But have these funds made any impact?

The research within investigates the effects of federal Healthy Marriage Initiative (HMI) grants on marriages and divorces in the areas targeted. By utilizing a dataset that combines National Longitudinal Survey of Youth (NLSY97) responses with federal HMI grant data at the county level, I identify the impact of the HMI on targeted populations. Black and low-income respondents residing in the counties receiving more funding are less likely to be married as a result of these funds, while the effect is reversed for non-black/non-Hispanic and high-income individuals. The presence of HMI funded organizations in a county also decreases the likelihood of divorce relative to counties with no such organizations, especially for black and low-income respondents. I conclude that the primary effect of HMI funding is to decrease marriage rates for targeted populations, selecting for stronger marriages that are less vulnerable to divorce.

The following section briefly discusses the background of the federal HMI program, as well as previous research on the effectiveness of marriage education. Section 1.3 addresses the empirical methodology to be used in the analysis and discuss the sources of the data to be used. Section 1.4 presents the results of the analysis. Section 1.5 tests the robustness and sensitivity of these findings to an array of alternative assumptions. Finally, Section 1.6 concludes.

1.2 BACKGROUND

Following the lead of state and local initiatives, in 2002 the Administration for Children and Families (ACF) began funding research and service programs under the banner of the Healthy Marriage Initiative. The federal initiative started small, issuing grants from existing discretionary funds via the various offices of the ACF. A major expansion occurred in October 2006 when the Office of Family Assistance awarded 125 grants to 123 recipients in 33 states, the District of Columbia, and American Samoa. The awards ranged from $132,000 to $2,342,000 per year, most of them for a 5 year period extending into

2011. While some of the funded organizations are implementing broad-based national or statewide media campaigns, the majority are community-based non-profit organizations offering marriage education through workshops and classes.[1]

To provide an example of the type of programs being funded, the Building Strong Families (BSF) project utilized HMI funding to implement programs in eight different sites across the country: Atlanta, GA; Baltimore, MD; Baton Rouge, LA; Orange and Broward Counties, FL; Houston, TX; Allen, Marion, and Lake Counties, IN; Oklahoma City, OK; and San Angelo, TX. All of the sites recruited unmarried parents with the intention of improving relationship quality and encouraging entrance into a healthy marriage. The programs offered several services including group sessions to improve relationship skills, individual support from program staff, and referral to external support services. While each site carried out the mission of the program individually, the central component in all locations was intensive curriculum-based group education aimed at improving relationship quality. Couples were expected to participate in over 30 hours of group sessions over a period of as little as 6 weeks or as long as 5 months, depending on the site (Wood et al., 2012).

The federal HMI provides grants to service providers in order to enable or enhance marriage education programs like those included in the Building Strong Families project. The most recent and comprehensive analysis of the effectiveness of such interventions is Hawkins et al. (2008). In their meta-analysis of experimental and quasi-experimental studies, the authors show that participation in a marriage and relationship education program had positive and statistically significant effects on relationship quality and communication skills. However, the vast majority of participants in these studies are white, middle-class, married couples with no history of significant relationship distress. Only 7 of 117 studies reported more than 25% racial/ethnic diversity and only 2 studies had primarily low-income

[1] For more information regarding the Healthy Marriage Initiative, see "Administration for Children and Families Healthy Marriage Initiative 2002-2009: An Introductory Guide." at http://www.healthymarriageinfo.org/docs/ACFGuideto09.pdf

couples. Many receipients of federal HMI grants, in keeping with the spirit of the initiative, service low-income and minority communities. As a result, HMI-funded programs may not achieve similar results.

In order to address this shortcoming, a recent paper experimentally evaluates the impact of the HMI-funded Building Strong Families project. Wood et al. (2012) assesses the impact of the BSF program across its eight different locations. The conclusions of the study are mixed, but predominantly negative. When the results are averaged across all eight areas, BSF is shown to have no impact on either the probability of couples remaining together or the quality of couples' relationships. In addition, only one program (Oklahoma City) showed consistent positive effects, while another program displayed some troubling negative effects (Baltimore). While the study was not designed to examine the underlying causes for heterogeneous treatment effects across cities, the authors speculate that the disparity between the Oklahoma City and Baltimore programs is partially due to subject characteristics. Specifically, the Baltimore participants were more economically disadvantaged than participants in other BSF programs. Considering that low-income individuals are the primary target of the HMI, any evidence that suggests significant negative treatment effects of marriage education on economically disadvantaged couples is worrisome to say the least.

Apart from the experimental evidence, Birch et al. (2004) and Kickham and Ford (2009) have both conducted observational studies of the impact of state and community marriage initiatives. Birch et al. (2004) examines community-based marriage initiatives in 122 U.S. counties and finds a modest negative effect on divorce rates in these counties when compared to matched control counties. Kickham and Ford (2009) expands on this study to look at state-wide initiatives using Current Population Survey data. They find that the implementation of state marriage initiatives are followed by a decrease in divorce prevalence, especially among states that the researchers deemed high-activity states. It is clear from their analysis that, in these high-activity states, divorce rates decrease following the intervention. However, the researchers fail to contrast this decline with the evolution of divorce rates in a relevant control set of states receiving no such intervention. Further, even if the decline is not experienced by control states, such a result could also be explained by mean reversion if the states select into the high-activity group due to concerns over abnormally low marriage rates or high divorce rates. Further, neither article examines the effects of these initiatives on subgroups or accounts for the significant expansion of these programs that accompanied the federal government's direct involvement.

As a result, it is difficult to forecast the effectiveness of a broad-based initiative such as the federal HMI from the evidence compiled to date. While most experimental studies yield positive results, they tend to focus on a population that is not the primary target of federal policy initiatives such as the HMI. In addition, recent experimental evidence suggests that these interventions are somewhat less successful, possibly even deleterious for the economically disadvantaged populations of interest. Observational studies find some preliminary evidence of improved marital outcomes resulting from policy initiatives, but remain silent with regard to heterogeneous effect across particular subgroups, which the experimental evidence indicates may be important. I address these shortcomings in the research to date by utilizing the large expansion of funding for these programs that accompanied the 2006 expansion of the federal HMI, paying particular attention to differences across races and socio-economic strata. If programs such as these are effective, the wider reach and increased intensity that followed the HMI expansion should allow for the detection of some effect on relationships in areas served, either through an increase in marriages, a decrease in divorce prevalence, or a strengthening of relationship health.

1.3 DATA AND EMPIRICAL METHODOLOGY

I measure the impact of HMI funding on three different outcomes of interest: marriage, divorce, and reported quality of an individual's relationship with his or her spouse or partner. This task requires the comparison of trends in these outcomes across residents of areas receiving significant funding and residents areas receiving little or no funding. As a result, I like the National Longitudinal Survey of Youth 1997 (NLSY97) with HMI funding data available through the Healthy Marriage Initiative website. The survey data provides information on marital status, residential location, and other important characteristics at the individual level, while the funding data allows us to link individual survey respondents to the intensity of HMI funding in their area of residence.

The NLSY97 follows a random sample of respondents and selected siblings who were born between 1980 and 1984. At the time of the first interview in 1997, the cohort consisted of 8,984 individuals between the ages of 12 and 18. As of the most recent round of the survey administered in 2009 and 2010, the remaining respondents in the sample were between the ages of 23 and 30 and many were experiencing the major life events addressed by the HMI, i.e. marriage, parenthood, and in some cases divorce. The NLSY questionnaire includes a wide array of questions on all aspects of the respondents' lives, including their relationships. As a result, I can construct a relationship history for each respondent, indicating marital status as of the interview date as well as the number of years they had reported a particular marital status. Finally, responses to questions regarding relationship quality are available for some years and some respondents. While the sample of respondents is relatively small for an observational study, the longitudinal nature of the survey combined with the age of the survey respondents makes for a good primary data source.[2]

[2]Some self-reported variables had significant issues with missing values. For married and separated/divorced indicators, education level, age, and county in 2006, some observations were interpolated between rounds. County in 2006 was only interpolated if county in 2005 and county in 2007 were non-missing and the same. Of the 116,792 observations (8,984 respondents x 13 years), observations were interpolated for each variable as follows: married - 5,889; separated/divorced - 5,889; education - 6,257; age - 14,734. Except for age, the vast majority of interpolations took place between subsequent identical values. Inter-

In order to link the respondents to HMI funding, I have compiled detailed data on federal HMI grants. The U.S. Department of Health and Human Services has made information available regarding current and past HMI grant recipients dating back to 2004. The data is presented in various lists and grant abstracts through the Healthy Marriage Initiative website. I have assembled a comprehensive database of federal HMI grants by collecting data from these various sources. This database includes the names of the grantees and their projects, the counties being targeted by the funding, the start and end dates of each grant, and the amount of funding awarded on an annual basis. For many grants an abstract was available, which listed the cities or counties being served by the project. For the remainder of the grants, however, the city in which the grant recipient is located was the only available information. In these cases, services were assumed to be limited to the primary county in which the city of the grant recipient is located. For example, if a grant was issued to a recipient located in Milwaukee, Wisconsin, the grant would be considered as serving Milwaukee County.

The impact of funding is then assessed via a difference-in-difference design. Such an approach entails first calculating the difference between average relationship outcomes in counties receiving funding vs. those not receiving funding in the period prior to the intervention. This figure represents time-invariant differences between the populations of these two areas that can not be credited to the intervention itself as it has not yet occurred. Second, the difference between average outcomes in counties receiving funding vs. those not receiving funding in the period following the intervention is calculated. This difference conflates the impact of the intervention with the overall impact of time, however. The difference-in-differences separates the true impact of the treatment from any time-invariant differences between the two groups of counties as well as any secular impact of the passage of time from the pre- to post-intervention period. For this approach to be effective, there

polated observations where each variable changed between subsequent observations are as follows: married - 940; separated/divorced - 194; education - 666. County in 2006 was interpolated between 2005 and 2007 for 114 respondents (1,482 observations).

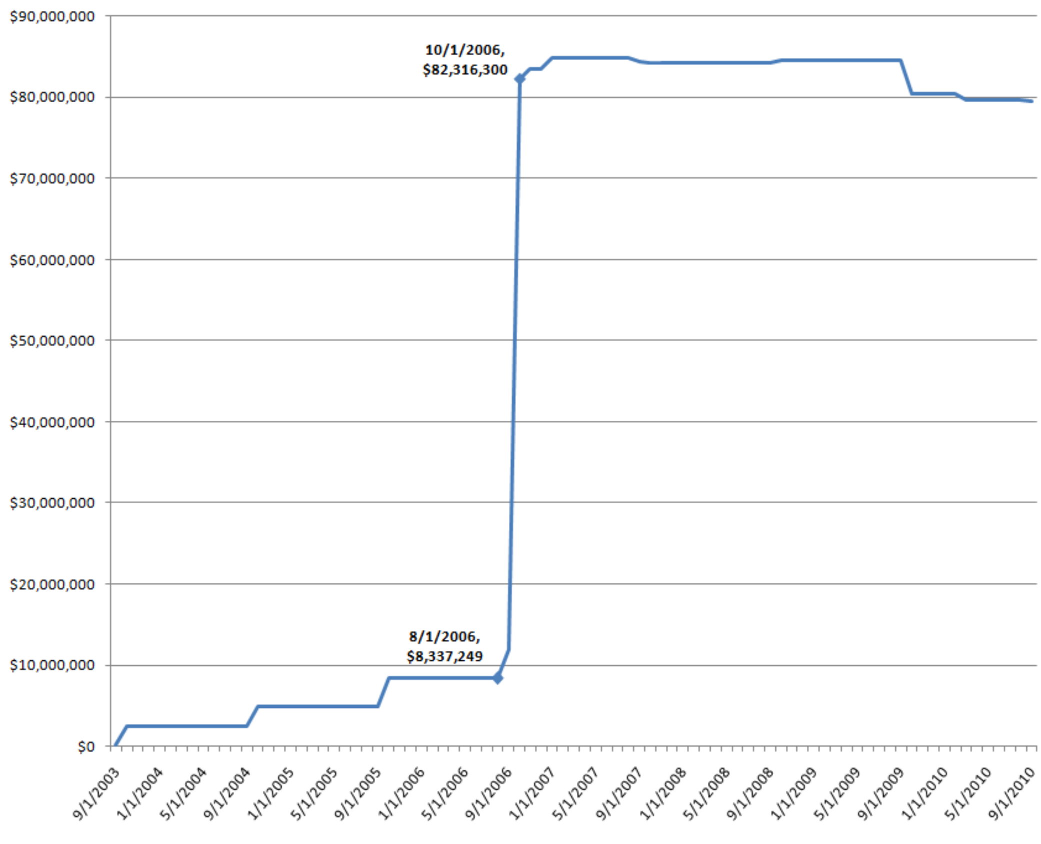

Figure 1.1: Federal HMI Funding per Year

needs to be a sharp delineation between these two time frames. A slow rollout of funding would make any selection of a cutoff date between pre- and post-intervention arbitrary and bias the estimates from any resulting analysis towards zero.

Figure 1.1 suggests that this is not likely to be an issue. Prior to October 2006, funding for the HMI program was less than $10 million per year. As a result of the expansion, however, the amount of funding issued per year for the HMI program increased by an order of magnitude. This increased funding on both the extensive margin, with new counties being brought into the fold, and the intensive margin, with previous recipient counties receiving more funding.

Borrowing from the lexicon of the treatment-control paradigm of clinical trials, I refer to the counties that received funding during this expansion as the treated counties and the individuals living in these counties during the expansion as treated individuals. The control group then consists of all counties (and individuals located in counties) that did not receive funding between September and November of 2006. Note that due to the mobility of survey respondents, not all respondents will "comply" with treatment assignment. To the extent that individuals migrate into or out of these counties over time, this assumption serves to introduce truly treated individuals into my control group and vice versa, attenuating any resulting estimate of the impact of treatment towards zero.[3] With the nomenclature established, I estimate the following model:

$$Y_{icst} = \alpha + \beta Post_t \cdot Treat_c + \gamma \mathbf{X_{it}} + \delta_{st} + \omega_c + \varepsilon_{ict} \tag{1.1}$$

where the dependent variable is either an indicator for married status, an indicator for divorced status, or an index of relationship health for individual i living in state s and county c at time t; $Treat_c$ is a binary variable set to one if a respondent is assigned to the treated group based on his residence in county c as of 2006; $Post_t$ is a binary variable indicating post-2006 waves of the survey; $\mathbf{X_{it}}$ is a number of observed control variables; and δ_{st} and ω_c represent the inclusion of a full set of dummy variables for each county and state-year interaction in the sample. The county and state-year dummies account for any systematic fixed effects in relationship outcomes that exist between counties over time or within a state-year cell across counties. The controls include individual level information that may be correlated with relationship outcomes— age, gender, race/ethnicity, etc.— as well as county-level variables such as urban/rural location and local unemployment rates. As the NLSY samples siblings raised in the same household, some specifications include a

[3]The analysis is performed on a subset of non-movers in Section 1.5.

full set of dummy variables for each sample household. These serve a similar purpose to the county-level fixed effects, accounting for any persistent differences in relationship outcomes resulting from being raised in a particular household. Finally, taking full advantage of the longitudinal nature of the data, some specifications will include individual-specific fixed effects.

With the inclusion of county and year fixed effects, the coefficient β implements the difference-in-difference estimator explained above and measures the impact of the "intention to treat", as we do not observe actual utilization rates in the data. Causal inference relies on assumptions regarding the counterfactual scenario: what would happen to the outcomes of individuals receiving the intervention in the absence of the intervention. In particular, interpretation of the coefficient as representing the causal effect of HMI funding requires that, in the absence of any intervention, outcomes in the treated population would have followed a similar trend to those in the control population. Evidence is provided in support of this assumption in what follows.

Alternatively, I estimate equation 1.1 using per capita funding levels instead of the interaction between $Post_t$ and $Treat_c$ in order to estimate the relationship between outcomes and the *intensity* of the intervention. Using this method, the total annual funding amount received by a given county is divided by a U.S. Census estimate of the county's popuation in that year. Finally, since the policy is targeted at low-income, minority couples, heterogeneous treatment effects are of special interest. In order to determine whether the funding is having a differential impact on black, Hispanic, or low-income individuals, I interact race/ethnicity and income categories with the $Post$ and $Treat$ indicators in the difference-in-difference framework as well as the per capita funding variable. The estimated coefficients on these interaction terms provide a measurement of the marginal effects of HMI grant receipt on those subgroups of interest.

A review of Table 1.1 shows the similarities and differences between the individuals in the treatment and control groups in each sample. Individuals in the treatment group are

Table 1.1: Summary Statistics, NLSY97

	2000 - 2006			2006 - 2009		
	Control	Treatment	t-stat	Control	Treatment	t-stat
Per Capita Funding	0.01	0.06	-23.42	0.04	0.99	-40.53
	(0.11)	(0.36)		(0.26)	(2.57)	
Married	0.12	0.11	4.15	0.28	0.26	2.94
	(0.33)	(0.31)		(0.45)	(0.44)	
Divorced/Separated	0.02	0.01	3.15	0.06	0.04	5.32
	(0.12)	(0.11)		(0.24)	(0.21)	
Age	21.12	21.18	-2.64	25.88	25.92	-1.89
	(2.34)	(2.34)		(1.63)	(1.65)	
Female	0.51	0.51	0.86	0.51	0.50	1.33
	(0.50)	(0.50)		(0.50)	(0.50)	
Non-Black / Non-Hispanic	0.55	0.45	20.84	0.55	0.45	14.91
	(0.50)	(0.50)		(0.50)	(0.50)	
Black	0.28	0.25	7.55	0.28	0.26	4.17
	(0.45)	(0.43)		(0.45)	(0.44)	
Hispanic	0.16	0.29	-34.29	0.16	0.29	-23.02
	(0.36)	(0.45)		(0.36)	(0.45)	
High School or Less	0.91	0.90	6.66	0.72	0.68	5.47
	(0.28)	(0.31)		(0.45)	(0.46)	
Associates / Bachelors	0.08	0.10	-6.34	0.26	0.28	-3.98
	(0.28)	(0.31)		(0.45)	(0.46)	
Urban	0.70	0.87	-44.41	0.71	0.86	-25.89
	(0.46)	(0.33)		(0.45)	(0.35)	
Rural	0.27	0.10	45.70	0.24	0.08	30.42
	(0.45)	(0.30)		(0.43)	(0.27)	
Unemployment Rate	5.32	5.09	14.06	8.15	8.01	3.16
	(1.89)	(1.45)		(3.10)	(2.88)	
Employment Status	0.65	0.63	3.05	0.73	0.73	-0.11
	(0.48)	(0.48)		(0.44)	(0.44)	
Est. Income / 10,000[a]	1.40	1.38	1.49	2.69	2.75	-2.17
	(1.19)	(1.21)		(1.87)	(1.91)	
N	28,026	18,769		12,328	8,313	

Notes: Summary statistics are presented for regression sample. Standard deviations in parentheses. Marital status, education level, and age interpolated between surveys when missing. Est. income provided by respondent either directly or as category (midpoint taken). Employment status = 1 if worked more than 26 weeks over previous year.

[a] Est. income could not be computed for some respondents ($N = \{21,250; 14,015; 10,392; 7,050\}$).

more likely to be Hispanic. This is likely due to the concentration of HMI grant recipients in states like Arizona, New Mexico, Florida, Texas, and California with large Hispanic populations. Individuals in counties receiving funding also live in areas with slightly lower unemployment rates and slightly higher incomes in the later years of the survey. Other statistics are similar across groups. Importantly, for those characteristics that do vary substantially across groups, the differences stay relatively constant over time. As a result, provided that the outcome variables are not diverging prior to the start of treatment, the strategy used in this analysis differences out those static variations in observables, leading to an unbiased estimate of the effect of treatment.

1.4 RESULTS

1.4.1 MARRIAGE

Figure 1.2 displays the trends in marriage amongst treated and control groups in NLSY. In this case, the marriage rate depicted is defined as the ratio of married respondents to total respondents over 18 in each group. Clearly marriage rates in both groups are growing over time. This growth is simply a result of the aging from adolescence into adulthood of a panel of survey respondents. Interestingly though, the treatment group seems to be uniformly less likely to marry than the control. This phenomenon is likely a result of the way treatment is assigned; areas troubled by low marriage rates in the young adult population apply for federal HMI grants at a differential rate, leading to an increase likelihood of treatment for these areas. Still, it appears that the rate of growth is not divergent between the two groups. The difference between marriage rates in treatment and control groups stays relatively constant both before and following the treatment. If the funding were having the desired effect, marriage rates in the treatment group should catch up to and perhaps even surpass the control group.

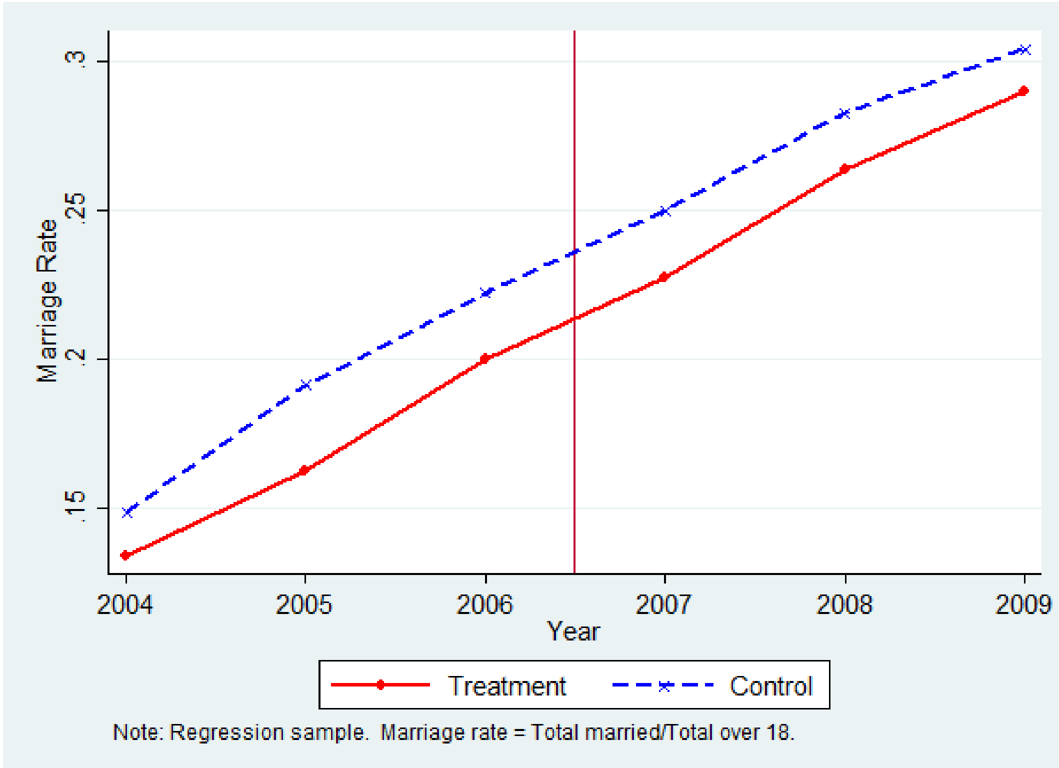

Figure 1.2: Marriage Rate by Group, NLSY97

In order to control for other observable factors that might be affecting relationships at both the individual and county level, I turn to regression analysis. To this end, I estimate Equation 1.1 using status as married as the dependent variable. The results are reported in Table 1.2. The structure of the table is similar to those in the remainder of the section. Column 1 reports the results of a difference-in-differences across groups over time controlling for county and year fixed effects, as well as other covariates. To account for the possibility of heterogeneous treatment effects, I estimate a variation of Equation 1.1 where the post-2006 and treatment indicator variables are interacted with indicators for race and

Table 1.2: OLS Impact of OFA Grants on Marriage, 2000-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Post 2006 × Treatment	-0.0147		-0.0143		-0.0139	
	(0.0104)		(0.0109)		(0.0102)	
× Black		-0.0594***		-0.0658***		-0.0688***
		(0.0149)		(0.0143)		(0.0131)
× Hispanic		-0.0135		-0.0217		-0.0163
		(0.0157)		(0.0157)		(0.0151)
× Mixed Race		-0.0590		-0.0534		-0.0679
		(0.0705)		(0.0686)		(0.0584)
× Non-Black/Non-Hispanic		0.0140		0.0227*		0.0236*
		(0.0137)		(0.0137)		(0.0129)
Observations (Clusters)			67,375 (939)			
R^2	0.203	0.204	0.509	0.510	0.602	0.603
Panel B: By Income Quartile						
Post 2006 × Treatment	-0.0130		-0.0148		-0.0135	
	(0.0114)		(0.0123)		(0.0113)	
× Bottom Quartile		-0.0464***		-0.0482***		-0.0555***
		(0.0162)		(0.0152)		(0.0139)
× Second Quartile		-0.0080		-0.0206		-0.0114
		(0.0162)		(0.0149)		(0.0143)
× Third Quartile		-0.0185		-0.0190		-0.0212
		(0.0200)		(0.0209)		(0.0183)
× Top Quartile		0.0333		0.0437*		0.0485**
		(0.0627)		(0.0685)		(0.0166)
Observations (Clusters)			47,341 (849)			
R^2	0.221	0.222	0.534	0.535	0.614	0.614
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

ethnicity (in Panel A) and respondent's 2006 income quartile (in Panel B).[4] The results of this analysis are reported Column 2. Columns 3 and 4 duplicate the analysis of columns 1 and 2 but include household level fixed effects, while columns 5 and 6 use individual level fixed effects.

Looking at the impact of treatment overall, there appears to be no measurable effect of treatment on likelihood of marriage in the sample. While naturally the predictive power of the model increases dramatically as more effects are included (moving from column 1 to column 6), the impact of treatment is statistically insignificant across the different specifications. However, when estimating the effect on racial subgroups and income quartiles, a pattern emerges. The receipt of an HMI grant dramatically *reduces* the likelihood that black respondents and low-income respondents report being married. There also seems to be a marginally significant, positive effect of grant receipt on non-black/non-Hispanic respondents and high-income respondents. This evidence is consistent with the experimental evaluation of the Building Strong Families program presented in Wood et al. (2012), where the analysis pointed to negative results in sites servicing an economically disadvantaged population and positive results in sites servicing a more affluent, white population.

Turning to the continuous measure of treatment, Table 1.3 reports the results of substituting a per capita funding measure for the interaction indicator used above. When income is omitted, the estimated overall effect of increased funding is insignificant as before. When income is controlled for, the (insignificant) reduction implied by the difference-in-difference estimates is validated; being exposed to increased funding in the form of HMI grants seems to decrease the likelihood of an average NLSY respondent being married in a given year. Specifically, an additional \$2.40 per capita per year of HMI grants leads to a 1% decrease in the likelihood that an individual is married in that year. Amongst the treated counties post-HMI, this amounts to slightly less than one standard deviation (\$2.57).

When looking at heterogeneous effects by race and income, preferred specifications

[4]The reduction in the sample size in Panel B is due to missing data in the income measure. Some respondents in some years do not provide estimates of income.

Table 1.3: OLS Impact of OFA Grant Funding Per Capita on Marriage, 2000-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Per capita funding	0.0013		0.0016		0.0004	
	(0.0024)		(0.0020)		(0.0021)	
× Black		0.0017		-0.0109		-0.0105
		(0.0114)		(0.0173)		(0.0166)
× Hispanic		-0.0256**		-0.0177		-0.0133
		(0.0121)		(0.0114)		(0.0102)
× Mixed Race		-0.0460		-0.0598**		-0.0516**
		(0.0661)		(0.0268)		(0.0247)
× Non-Black/Non-Hispanic		0.0023*		0.0035**		0.0021*
		(0.0012)		(0.0015)		(0.0013)
Observations (Clusters)			67,375 (939)			
R^2	0.203	0.204	0.509	0.510	0.602	0.602
Panel B: By Income Quartile						
Per capita funding	-0.0042**		-0.0031		-0.0041**	
	(0.0020)		(0.0022)		(0.0019)	
× Bottom Quartile		0.0014		0.0078		-0.0047
		(0.0101)		(0.0077)		(0.0074)
× Second Quartile		-0.0041		-0.0104		-0.0048
		(0.0097)		(0.0121)		(0.0100)
× Third Quartile		-0.0064**		-0.0063***		-0.0055***
		(0.0028)		(0.0023)		(0.0016)
× Top Quartile		0.0029		0.0051		0.0060
		(0.0092)		(0.0094)		(0.0097)
Observations (Clusters)			47,341 (849)			
R^2	0.221	0.221	0.534	0.534	0.614	0.614
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), and a dummy for living with both biological parents as of the first round of the survey.
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

(column 6) still indicate a negative impact on black and low-income respondents and a positive impact on high-income respondents, but these effects are rendered statistically insignificant. The positive effect of funding on non-black/non-Hispanic respondents is still present and significant. In addition, the analysis by funding intensity strongly suggests that mixed race individuals and middle-income (3rd quartile) respondents are the most likely to be affected by increased funding. A $2.57 increase in funding per capita per year (one standard deviation in the post-HMI treated counties) leads to a 13% *decrease* in the likelihood that a mixed race individual is married in that year and a 1.4% decrease in the same measure for individuals in the third income quartile. It is worth noting that mixed race individuals comprise less than 1% of the NLSY sample.

These results indicate that funding may in fact be having a perverse effect on young adults if the intended goal is to create more marriages within the youth population. Estimates suggest that increased funding decreases the likelihood that a young person is married, when controlling for income. Furthermore, for some disadvantaged (bottom income quartile) and minority (black) groups targeted the impact is stronger still. Estimates indicate that respondents in these groups are much less likely to be married in years when their counties receive federal HMI grant funding, although the intensity of funding seems to matter little. Still, it is possible that these programs are deterring marginal marriages that would ultimately lead to divorce. If marriage and subsequent divorce would lead to further hardship for these individuals and their children, the program's effect on marriage itself is not an unambiguous indicator of the program's effectiveness. Prevention of divorce, however, should be more closely associated with the intended goals of the policy.

1.4.2 DIVORCE

Figure 1.3 displays trends in divorce rates by group. The divorce rates depicted here are calculated as the ratio of separated/divorced respondents to ever-married respondents in each group. As with marriage rates, it is only reasonable to expect divorce rates in both groups to grow over time as the sample of respondents ages. Importantly, divorce rates in the treatment group track those in the control group fairly well prior to 2006, except for a small, less than 1 percentage point difference in levels. The similarity in trends prior to the onset of treatment indicates that the control individuals should serve as an adequate counterfactual for the treated individuals once pre-existing differences are controlled for. Divorce rates begin to diverge in 2006— the year that grants were issued— and remain roughly 3 percentage points apart through 2009. This decrease in relative divorce rates is precisely the effect we would expect to see given the intent of the policy, although perhaps slightly early given the timing of the grants, which were issued in late 2006. The divergence in 2006 could be due to recipients ramping up activity in anticipation of grant receipt.

While illustrative, Figure 1.3 does not control for other variables that may be influencing relationship outcomes. As above, I estimate Equation 1.1 using status as divorced instead of married as the dependent variable. The results are reported in Table 1.4. The divergence in divorce rates seen in the graph following the issuance of grants is reflected here by the significant and negative coefficient on the Post 2006 x Treatment term. Residing in a county that received a grant during the September 2006 expansion seems to reduce the likelihood that a respondent is divorced in the years following by 1.5% - 2%. This effect is robust to the inclusion of household and individual fixed effects, as well as the inclusion of self-reported income (on the associated sample).

Looking at treatment effects across racial and ethnic groups indicates that the overall decrease in divorce rates is driven primarily by the black and non-black/non-Hispanic respondents. Hispanic respondents also have lower divorce rates subsequent to grant receipt, but the effect is less pronounced. The impact on mixed race individuals is to increase their likelihood of divorce, although estimates are imprecise. All income quartiles seem to be benefitting from the intervention as well through reduced likelihood of divorce. While the effects seems strongest for respondents below the median income, the differences in magnitude are not large. As a result, it seems as through heterogeneity across income groups

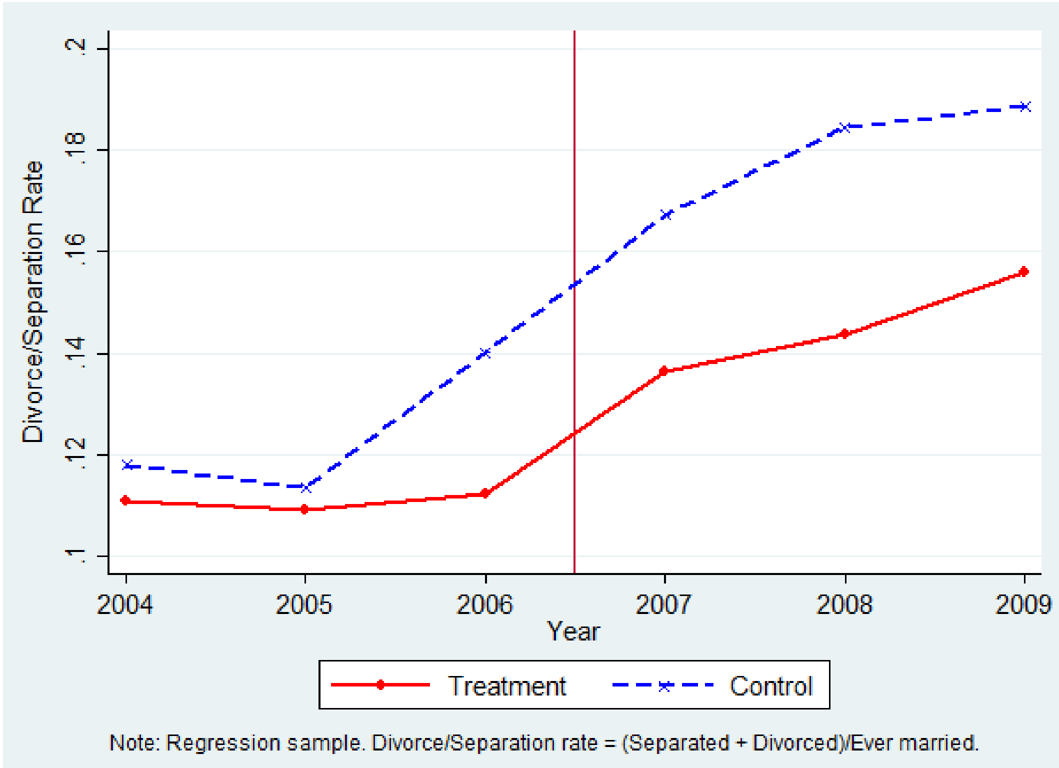

Figure 1.3: Divorce Rate by Group, NLSY97

is not a primary concern when it comes to the impact of the intervention on divorce.

Utilizing the additional information available on the intensity of treatment across counties, Table 1.5 reports the results from replacing the interaction indicator with per capita funding. Across panels and specifications, estimates indicate a negative impact of additional HMI funding on likelihood of divorce overall, although this effect varies quite a bit in magnitude and precision. According to these estimates, reducing likelihood of divorce by 1% would require between \$7 and \$17 of additional funding per capita per year. For treated counties, the mean funding per capita per year in the post-HMI period was roughly

Table 1.4: OLS Impact of OFA Grants on Divorce, 2000-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Post 2006 × Treatment	-0.0159***		-0.0158***		-0.0147***	
	(0.0047)		(0.0048)		(0.0045)	
× Black		-0.0160***		-0.0189***		-0.0247***
		(0.0056)		(0.0060)		(0.0057)
× Hispanic		-0.0125		-0.0100		-0.0131**
		(0.0076)		(0.0067)		(0.0066)
× Mixed Race		0.0363		0.0461		0.0462
		(0.0330)		(0.0297)		(0.0315)
× Non-Black/Non-Hisp		-0.0188***		-0.0184***		-0.0160***
		(0.0064)		(0.0064)		(0.0061)
Observations (Clusters)			67,375 (939)			
R^2	0.172	0.172	0.407	0.407	0.496	0.490
Panel B: By Income Quartile						
Post 2006 × Treatment	-0.0188***		-0.0163***		-0.0156***	
	(0.0053)		(0.0056)		(0.0052)	
× Bottom Quartile		-0.0220***		-0.0158**		-0.0169***
		(0.0070)		(0.0068)		(0.0061)
× Second Quartile		-0.0237***		-0.0189**		-0.0187***
		(0.0078)		(0.0074)		(0.0071)
× Third Quartile		-0.0175*		-0.0113		-0.0110
		(0.0100)		(0.0104)		(0.0089)
× Top Quartile		-0.0210**		-0.0199**		-0.0154*
		(0.0100)		(0.0097)		(0.0086)
Observations (Clusters)			47,341 (849)			
R^2	0.170	0.111	0.439	0.439	0.511	0.511
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence Treatment and county is assigned by residence as of Round 10 of the survey (2006). Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 1.5: OLS Impact of OFA Grant Funding Per Capita on Divorce, 2000-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Per capita funding	-0.0014**		-0.0012		-0.0013*	
	(0.0007)		(0.0008)		(0.0007)	
× Black		-0.0053***		-0.0049**		-0.0052**
		(0.0015)		(0.0022)		(0.0024)
× Hispanic		-0.0033		-0.0024		-0.0029
		(0.0053)		(0.0068)		(0.0050)
× Mixed Race		-0.0147		0.0040		0.0013
		(0.0140)		(0.0129)		(0.0128)
× Non-Black/Non-Hisp		-0.0008		-0.0008		-0.0009
		(0.0007)		(0.0008)		(0.0007)
Observations (Clusters)			67,375 (939)			
R^2	0.172	0.172	0.407	0.407	0.496	0.496
Panel B: By Income Quartile						
Per capita funding	-0.0008		-0.0006		-0.0008	
	(0.0007)		(0.0008)		(0.0008)	
× Bottom Quartile		-0.0025		-0.0024		-0.0027
		(0.0021)		(0.0018)		(0.0017)
× Second Quartile		-0.0026		-0.0025		-0.0029
		(0.0039)		(0.0045)		(0.0045)
× Third Quartile		-0.0002		0.0001		-0.0002
		(0.0009)		(0.0009)		(0.0008)
× Top Quartile		0.0001		-0.0004		0.0001
		(0.0036)		(0.0026)		(0.0025)
Observations (Clusters)			47,341 (849)			
R^2	0.169	0.170	0.439	0.439	0.511	0.511
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

$1.

Looking at the effects specifically on black respondents, however, the estimates are much more dramatic. Reducing the likelihood of divorce by 1% in that group would only require an additional $1.92 in funding per capita per year. When contrasted with the results from the marriage analysis above, it seems as though the intensity of the program matters little for marriage outcomes in the young black population, but has a much stronger effect on divorce outcomes. Analysis by income group is too imprecise to yield significant estimates.

In summary, the estimates of the impact of HMI grants on divorce are somewhat mixed. Difference-in-difference estimates suggest a moderate impact of residing in a treated county, decreasing likelihood of divorce across racial, ethnic, and income groups (albeit with a more pronounced effect on black and low-income respondents). The estimates based on per capita funding intensity at the county level, however, indicate that the effect of additional funding operates almost exclusively through reducing likelihood of divorce amongst black respondents. In combination with the results from the marriage analysis, I conclude that the effect of HMI funding is to decrease overall marriage rates in a county, but the marriages that do occur seem to be stronger, leading to lower likelihood of divorce amongst respondents in those counties. In addition, this effect seems to be most pronounced within the low-income and black populations being targeted by the grants.

1.4.3 MARRIAGE AND RELATIONSHIP HEALTH

The final dependent variable of interest in the analysis is the health of relationships and marriages— the titular focus of the Healthy Marriage Initiative. The questions which provide the basis for this analysis, however, are not always answered and were not asked in the most recent round of the survey, leading to fewer observations than in the analysis above. Figure 1.4 charts the average values of the constructed relationship health index by year and treatment status. Interestingly, the reported quality of relationships appears fairly constant for the young adults in the sample until a dip of about 1.5 points in the

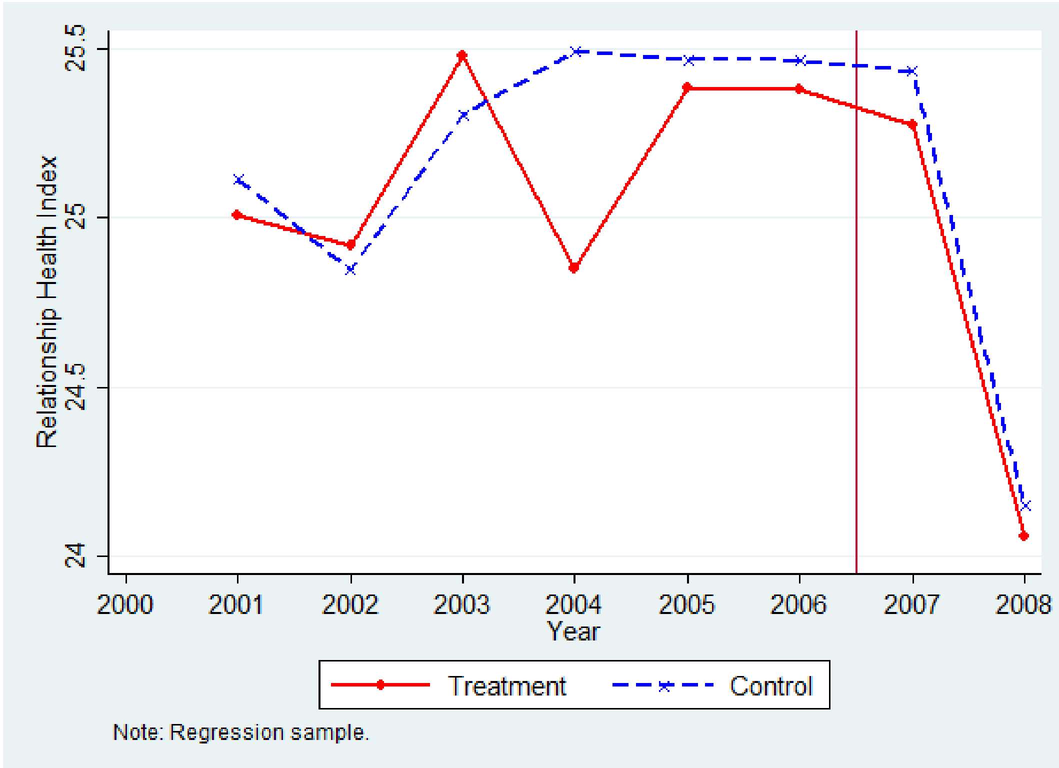

Figure 1.4: Relationship Health Index by Group, NLSY97

most recent year where data is available. Regardless, the data show that the treatment and control groups respond similarly to relationship health questions over time.

The results of estimating Equation 1.1 on this outcome are reported in Table 1.6. Across all specifications, the only notable effect of treatment is on mixed race individuals. These individuals self-report healthier relationships following the receipt of a grant in their county. Repeating the analysis with the additional information provided by per capita funding as in Table 1.7 generates mostly noisy estimates. The only significant effect is a negative effect on black respondents that is small in magnitude and disappears when controlling for

Table 1.6: OLS Impact of OFA Grants on Marriage and Relationship Health, 2004-2008

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Post 2006 × Treatment	-0.0017		0.0410		-0.0260	
	(0.178)		(0.189)		(0.176)	
× Black		-0.417		-0.348		-0.369
		(0.364)		(0.382)		(0.331)
× Hispanic		0.0775		-0.122		-0.169
		(0.263)		(0.262)		(0.242)
× Mixed Race		1.238		2.054**		1.832*
		(0.831)		(1.039)		(0.937)
× Non-Black/Non-Hispanic		0.0841		0.229		0.135
		(0.203)		(0.219)		(0.199)
Observations (Clusters)	16,838 (827)					
R^2	0.162	0.163	0.468	0.468	0.541	0.542
Panel B: By Income Quartile						
Post 2006 × Treatment	0.0525		0.150		0.0745	
	(0.186)		(0.208)		(0.185)	
× Bottom Quartile		-0.311		-0.123		-0.172
		(0.303)		(0.395)		(0.368)
× Second Quartile		0.186		0.163		-0.0026
		(0.272)		(0.305)		(0.260)
× Third Quartile		0.309		0.384		0.350
		(0.287)		(0.306)		(0.278)
× Top Quartile		-0.0996		0.0598		0.0208
		(0.315)		(0.347)		(0.302)
Observations (Clusters)	12,822 (747)					
R^2	0.183	0.183	0.504	0.504	0.566	0.566
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence Treatment and county is assigned by residence as of Round 10 of the survey (2006). Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level. , ** Significant at the 5% level, *** Significant at the 1% level.

Table 1.7: OLS Impact of OFA Grant Funding Per Capita on Marriage and Relationship Health, 2004-2008

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Race/Ethnicity						
Per capita funding	-0.0327		0.0090		0.0262	
	(0.0500)		(0.0369)		(0.0385)	
× Black		-0.213***		-0.441*		-0.232
		(0.0697)		(0.249)		(0.252)
× Hispanic		-0.219		-0.0678		-0.291
		(0.168)		(0.216)		(0.206)
× Mixed Race		0.542		1.648		1.481
		(0.520)		(1.046)		(1.058)
× Non-Black/Non-Hispanic		0.0146		0.0235		0.0497
		(0.0321)		(0.0304)		(0.0304)
Observations (Clusters)	16,836 (827)					
R^2	0.162	0.163	0.468	0.468	0.541	0.542
Panel B: By Income Quartile						
Per capita funding	-0.0196		0.0092		0.0354	
	(0.0383)		(0.0562)		(0.0450)	
× Bottom Quartile		-0.0334		-0.0158		-0.0521
		(0.0496)		(0.0331)		(0.0360)
× Second Quartile		0.101		0.0168		-0.166
		(0.133)		(0.196)		(0.191)
× Third Quartile		0.0006		0.139		0.104
		(0.113)		(0.203)		(0.180)
× Top Quartile		-0.112		0.0383		-0.0112
		(0.165)		(0.205)		(0.206)
Observations (Clusters)	12.822 (747)					
R^2	0.183	0.183	0.504	0.504	0.566	0.566
Controls	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
County Fixed Effects	Y	Y	N	N	N	N
Sibling Fixed Effects	N	N	Y	Y	N	N
Individual Fixed Effects	N	N	N	N	Y	Y

Notes: Standard errors clustered by the respondents' 2006 county of residence Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

individual level effects. Interestingly, a positive (though insignificant) treatment effect is again estimated for mixed race respondents. Again, the mixed race portion of the sample constitutes a very small minority (less than 1% of the NLSY sample), so any effect isolated to that group has limited economic significance.

In short, the impact of HMI funding on the relationship quality of individuals is not measurable with the data currently available. With a larger sample of respondents and a wider range of questions, there may be a discernible impact of funding on relationship quality, but based on this data we cannot reject the hypothesis that there is no effect of HMI funding on self-reported relationship health.

1.5 ROBUSTNESS CHECKS

There are several potential sources of bias in the estimation framework presented in Section 1.3. Selection of counties into treatment or increased intensity of treatment on the basis of pre-existing marriage and divorce rates is the primary concern. Organizations that are unhappy with the marriage and divorce statistics in their counties are likely to apply for funding and receive grants. In some cases, these organizations or others in the same counties had already been receiving grants as early as 2003 under the initial phase of the HMI. The direction of the bias introduced is unclear. Conflating counties receiving low-intensity treatment with counties receiving none would serve to bias estimates towards zero. However, counties with troublesome marriage and divorce outcomes may experience reversion to the mean, which naïve estimates could not distinguish from a treatment effect. In addition, counties with histories of poor marriage outcomes may have previously undertaken state or local initiatives, establishing the infrastructure necessary to make efficient use of federal grant dollars. As such, the impact of funding in these counties can not be interpreted as representative for counties that may not have the necessary infrastructure in place.

For divorce, selection bias at the individual level becomes an important issue. Since these outcomes are only meaningful for individuals who are already married, the interpretation of the estimated treatment effect is altered if individuals are selecting into (or out of) marriage on the basis of treatment, as is indicated by our marriage estimates. If individuals in more tenuous relationships are getting married as a result of the policy intervention, the estimate of the impact of treatment on divorce prevalence might be biased upwards as these couples are more prone to divorce. Alternatively, if the marriages conceived during the intervention are stronger on average due to the deterrence of marriage for less healthy couples, the impact of treatment on divorce prevalence may be biased down.

Tables 1.8 through 1.11 present a series of robustness checks to determine whether the results presented above are significantly influenced by any of these sources of bias in addition to sensitivity analyses prompted by concerns over measurement error. In each, the baseline results from columns 5 and 6 of Tables 1.2 through 1.5 are presented in Column 1, and each subsequent column presents the results from a specification designed to address some potential source of bias or measurement error. As the marriage and relationship health specifications were relatively uninformative, robustness checks against Tables 1.6 and 1.7 are not reported.

First, I revisit the results from the marriage specifications. The first columns of Tables 1.8 and 1.9 present the same estimates as the models from columns 5 and 6 of Tables 1.2 and 1.3. The subsequent columns all present similar models including individual fixed effects, but with variations on the measurement of treatment or sample restrictions. The second column is a replication of the baseline model on the period from 2004-2009 only. This model serves to check whether the results are driven by the earlier years of the survey, when the relatively young respondents would have been marrying at a low rate. The third column only includes individuals who did not move counties subsequent to the 2006 round of the survey in order to address concerns over the measurement error in the treatment variable: county of residence in 2006. The fourth column restricts the sample to respondents whose county of residence in 2006 had not previously received any federal HMI grants. This model is designed to test if counties with longer histories of poor marriage and divorce outcomes and previous experience with the administration of federal grant money are driving the results, as would be the case if there was significant selection into HMI funding. Finally, the fifth column is designed to test for the effect of another type of measurement error in the treatment variable. When a grant abstract mentioned a statewide initiative (usually a media campaign) as part of their program, this is ignored by the baseline model under the assumption that these statewide media campaigns are still likely concentrated around the grant recipient's location and are likely a small part of their service portfolio. Column 5 includes all counties in a state in the treatment group in these situations and dilutes the per capita funding measure accordingly.

Table 1.8: Robustness Checks for Impact of HMI Grants on Marriage

	Baseline	2004-2009	Non-movers	Excl. Previous	Statewide
Panel A: By Race/Ethnicity					
Post 2006 × Treatment	-0.0139	0.0043	-0.0234*	-0.0191*	-0.0071
	(0.0102)	(0.0086)	(0.0129)	(0.0110)	(0.0116)
× Black	-0.0688***	-0.0346***	-0.0769***	-0.0712***	-0.0668***
	(0.0149)	(0.0118)	(0.0156)	(0.0147)	(0.0136)
× Hispanic	-0.0163	-0.0036	-0.0191	-0.0200	-0.0034
	(0.0151)	(0.0126)	(0.0174)	(0.0181)	(0.0148)
× Mixed Race	-0.0679	-0.0592	-0.0050	-0.0007	-0.0668
	(0.0584)	(0.0556)	(0.0953)	(0.0610)	(0.0496)
× Non-Black/Non-Hispanic	0.0236*	0.0350***	0.0145	0.0137	0.0317**
	(0.0129)	(0.0111)	(0.0166)	(0.0133)	(0.0133)
Observations	67,375	41,415	45,454	56,185	67,375
Clusters	939	937	687	857	939
Panel B: By Income Quartile					
Post 2006 × Treatment	-0.0135	0.0076	-0.0179	-0.0172	-0.0036
	(0.113)	(0.0096)	(0.0150)	(0.0128)	(0.0130)
× Bottom Quartile	-0.0555***	-0.0243*	-0.0812***	-0.0636***	-0.0446***
	(0.0139)	(0.0128)	(0.0175)	(0.0163)	(0.0141)
× Second Quartile	-0.0114	0.0112	-0.0072	-0.0065	0.0030
	(0.0143)	(0.0136)	(0.0177)	(0.0170)	(0.0154)
× Third Quartile	-0.0212	0.0007	-0.0169	-0.0224	-0.0056
	(0.0183)	(0.0158)	(0.0220)	(0.0217)	(0.0174)
× Top Quartile	0.0485**	0.0535**	0.0420	0.0368	0.0462**
	(0.0209)	(0.0177)	(0.0277)	(0.0253)	(0.0202)
Observations	47,341	31,079	31,822	39,301	47,341
Clusters	849	846	637	769	849

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), and a dummy for living with both biological parents as of the first round of the survey.
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 1.9: Robustness Checks for Impact of HMI Funding on Marriage

	Baseline	2004-2009	Non-movers	Excl. Previous	Statewide
Panel A: By Race/Ethnicity					
Per capita funding	0.0004	0.0020	0.0094	0.0011	-0.0152**
	(0.0021)	(0.0025)	(0.0064)	(0.0018)	(0.0073)
× Black	-0.0105	0.0033	-0.0032	-0.0032	-0.0707***
	(0.0166)	(0.0116)	(0.0166)	(0.0164)	(0.0111)
× Hispanic	-0.0133	0.0018	-0.0066	-0.0356*	-0.0174
	(0.0102)	(0.0102)	(0.0114)	(0.0182)	(0.0171)
× Mixed Race	-0.0516**	-0.0479*	-0.0321	-0.0159	-0.0652*
	(0.0247)	(0.0245)	(0.0481)	(0.0347)	(0.0352)
× Non-Black/Non-Hispanic	0.0021*	0.0019	0.0158***	0.0021**	0.0115
	(0.0013)	(0.0018)	(0.0041)	(0.0009)	(0.0089)
Observations	67,375	41,415	45,454	56,185	67,375
Clusters	939	937	687	857	939
Panel B: By Income Quartile					
Per capita funding	-0.0041**	-0.0016	-0.0006	-0.0030**	-0.0245***
	(0.0019)	(0.0015)	(0.0088)	(0.0014)	(0.0079)
× Bottom Quartile	-0.0047	-0.0056	-0.0389***	-0.00311	-0.0389***
	(0.0074)	(0.0041)	(0.0136)	(0.0078)	(0.0128)
× Second Quartile	-0.0048	0.0045	0.0051	0.0098	-0.0302***
	(0.0100)	(0.0092)	(0.0094)	(0.0086)	(0.0108)
× Third Quartile	-0.0055***	-0.0029**	-0.0108	-0.0047***	-0.0339***
	(0.0016)	(0.0011)	(0.0159)	(0.0011)	(0.0131)
× Top Quartile	0.0060	0.0058	0.0462***	0.0001	0.0383
	(0.0097)	(0.0067)	(0.0175)	(0.0072)	(0.0242)
Observations	47,341	31,079	31,822	39,301	47,341
Clusters	849	846	637	769	849

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Examining Table 1.8, it seems that the significant results from the baseline model are robust to the changes to the definition of the treated group. The most precisely estimated coefficients— the negative impact of funding on marriage amongst black and low-income respondents and the positive impact on non-black/non-Hispanic and high-income respondents— retain their sign across models, although some models are less precisely estimated. This consistency indicates that the effects are not solely driven by the inclusion of early survey years (when respondents across groups are likely to be similar in relationship outcomes) or the inclusion of respondents in counties with longer histories of poor marriage outcomes or previous experience with federal funding. Columns 3 and 5 also suggest that measurement error in treatment assignment is not substantially affecting the estimates. Interestingly, the negative impact on black respondents and low-income respondents is strengthened by restricting the sample to only non-movers and also by restricting attention to counties that received no previous funding.

Moving on to the per capita funding estimates, Table 1.9 shows a similar pattern. The negative effects on mixed race respondents and the positive effects on the non-black/non-Hispanic group retain their sign across columns, with some variation in magnitude and precision. Ultimately, the lessons learned in the baseline specification do not change substantially when contrasted with Table 1.9.

Tables 1.10 and 1.11 perform all of the same checks as Tables 1.8 and 1.9 using divorce as the dependent variable. In addition, the fourth column replicates the baseline specification, but includes only respondents who were ever married as of 2004. We can be sure that these individuals did not initially select into marriage due to the presence of funding, thus allowing us to separate the effect of funding on marriage from the effect of funding on divorce. Unfortunately, this restriction also reduces the sample considerably as few respondents were married as of 2004.

Table 1.10: Robustness Checks for Impact of HMI Grants on Divorce

	Baseline	2004-2009	Non-movers	Ever Married 2004	Excl. Previous	Statewide
Panel A: By Race/Ethnicity						
Post 2006 × Treatment	-0.0147***	-0.0117***	-0.0086*	-0.0182	-0.0142***	-0.0176***
	(0.0045)	(0.0043)	(0.0051)	(0.0194)	(0.0050)	(0.0052)
× Black	-0.0247***	-0.0158***	-0.0136*	0.0079	-0.0220***	-0.0251***
	(0.0057)	(0.0054)	(0.0071)	(0.0437)	(0.0063)	(0.0063)
× Hispanic	-0.0131**	-0.0098*	0.0016	-0.0183	-0.0067	-0.0127*
	(0.0066)	(0.0056)	(0.0065)	(0.0257)	(0.0075)	(0.0066)
× Mixed Race	0.0462	0.0552*	0.0440	0.205*	0.0435	0.0245
	(0.0315)	(0.0301)	(0.0473)	(0.107)	(0.0315)	(0.0262)
× Non-Black/Non-Hispanic	-0.0160***	-0.0117**	-0.0135*	-0.0316	-0.0151*	-0.0156**
	(0.0061)	(0.0059)	(0.0071)	(0.0264)	(0.0064)	(0.0062)
Observations	67,375	41,415	45,454	10,748	56,185	67,375
Clusters	939	937	687	481	857	939
Panel B: By Income Quartile						
Post 2006 × Treatment	-0.0156***	-0.0133***	-0.0099*	-0.0244	-0.0154***	-0.0197***
	(0.0052)	(0.0049)	(0.0059)	(0.0224)	(0.0059)	(0.0061)
× Bottom Quartile	-0.0169***	-0.0121*	-0.0086	0.0050	-0.0176***	-0.0242***
	(0.0061)	(0.0065)	(0.0075)	(0.0423)	(0.0070)	(0.0067)
× Second Quartile	-0.0187***	-0.0145**	-0.0121	-0.0614*	-0.0213***	-0.0204***
	(0.0071)	(0.0069)	(0.0086)	(0.0362)	(0.0080)	(0.0074)
× Third Quartile	-0.0110	-0.0132*	-0.0114	-0.0102	-0.0074	-0.0130
	(0.0089)	(0.0077)	(0.0098)	(0.0319)	(0.0105)	(0.0089)
× Top Quartile	-0.0154*	-0.0105	-0.0062	-0.0186	-0.0145	-0.0213**
	(0.0086)	(0.0079)	(0.0108)	(0.0307)	(0.0100)	(0.0084)
Observations	47,341	31,079	31,822	7,645	3,9301	47,341
Clusters	849	846	637	427	769	849

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 1.11: Robustness Checks for Impact of HMI Funding on Divorce

	Baseline	2004-2009	Non-movers	Ever Married 2004	Excl. Previous	Statewide
Panel A: By Race/Ethnicity						
Per capita funding	-0.0013*	-0.0011*	-0.0015	-0.0047	-0.0016**	-0.0105***
	(0.0007)	(0.0006)	(0.0017)	(0.0031)	(0.0007)	(0.0034)
× Black	-0.0052**	-0.0034*	-0.0041*	0.0159	-0.0056*	-0.0126***
	(0.0024)	(0.0020)	(0.0024)	(0.0173)	(0.0029)	(0.0046)
× Hispanic	-0.0029	-0.0032	0.0056	-0.0029	0.0011	-0.0068
	(0.0050)	(0.0046)	(0.0056)	(0.0216)	(0.0089)	(0.0077)
× Mixed Race	0.0013	0.0067	0.0173	0.0609	-0.0024	-0.0001
	(0.0128)	(0.0150)	(0.0303)	(0.0455)	(0.0141)	(0.0173)
× Non-Black/Non-Hispanic	-0.0009	-0.0008	-0.0014	-0.0053*	-0.0013**	-0.0110**
	(0.007)	(0.0006)	(0.0022)	(0.0029)	(0.00063)	(0.0044)
Observations	67,375	41,415	45,454	10,748	56,185	47,341
Clusters	939	937	687	481	857	939
Panel B: By Income Quartile						
Per capita funding	-0.0008	-0.0010	-0.0010	-0.0035	-0.0016*	-0.0123***
	(0.0008)	(0.0008)	(0.0036)	(0.0034)	(0.0009)	(0.0040)
× Bottom Quartile	-0.0027	-0.0015	0.0026	-0.0028	-0.0034**	-0.0175***
	(0.0017)	(0.0017)	(0.0072)	(0.0027)	(0.0017)	(0.0047)
× Second Quartile	-0.0029	-0.0018	-0.0026	-0.0139	-0.0075	-0.0162***
	(0.0045)	(0.0047)	(0.0043)	(0.0266)	(0.0049)	(0.0062)
× Third Quartile	-0.0002	-0.0006	-0.0013	-0.0025	-0.0003	-0.0046
	(0.0008)	(0.0007)	(0.0083)	(0.0185)	(0.0007)	(0.0086)
× Top Quartile	0.0001	-0.0013	0.0020	-0.0054	-0.0031	-0.0084
	(0.0025)	(0.0017)	(0.0081)	(0.0213)	(0.0025)	(0.0088)
Observations	47,341	31,079	31,822	7,645	39,301	47,341
Clusters	849	846	637	427	769	849

Notes: Standard errors clustered by the respondents' 2006 county of residence. Treatment and county is assigned by residence as of Round 10 of the survey (2006). Per capita funding is determined at the grant level by dividing grant funding per year by the aggregate population for all core counties served by the grant in that year. These amounts are then aggregated for all grants associated with the respondent's county determined as above. Individual level controls include age, gender, race/ethnicity, education, employment status, urban/rural, local unemployment rates, self-reported estimated income (Panel B), years married since 1997, a dummy for ever married, and a dummy for living with both biological parents as of the first round of the survey.
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

In Table 1.10, we see again that the most precisely estimated effects of grant receipt from the difference-in-difference models are robust to most alternative assumptions regarding treated individuals and sample restrictions. The overall decrease in divorce is replicated across all specifications and both panels, varying between a 1% and 2% decrease in divorce likelihood and losing significance only in the smallest sample (colun 4). The effects on the non-black/non-Hispanic subgroup and the second and third income quartiles are similarly insensitive to alternative assumptions and sample restrictions. For those results, Table 1.8 suggests that the baseline estimates are not significantly biased by including early survey years or respondents in previously funded counties, nor are they plagued substantially by measurement error in treatment assignment.

The decrease in divorce likelihood for black respondents is replicated across most specifications, but reversed in the sample of individuals who had selected into marriage prior to treatment. This reversal, however, is consistent with the interpretation of the results provided earlier. If the program's negative effect on marriage likelihood for black respondents is a result of preventing marginal marriages and promoting only the strongest relationships, that would lead to a decrease in divorce rates for black respondents overall. For the individuals who selected into marriage prior to the program's inception, however, this effect would no longer be present, leading to a null or positive result for those individuals. This mechanism can also explain the amplification of the negative effect on divorce amongst non-black/non-Hispanic respondents who had previously selected into marriage. If the program's positive effect on marriage likelihood for non-black/non-Hispanic respondents is a result of promoting marginal marriages, that would predict that those individuals who had selected into marriage prior to the intervention would have relatively stronger relationships and would be less vulnerable to divorce. This logic is echoed in Panel B across the bottom quartile and top quartile groups.

Finally, in Table 1.11, the baseline results indicating a negative response of divorce likelihood to additional grant funding both overall and amongst the black respondents specifically are confirmed. The overall effects are qualitatively similar across specifications, except for the sample that alters the funding variable to account for statewide campaigns. This result is likely due to the corresponding dilution of the funding variable. By spreading the funding for statewide programs across all of the counties in the state equally, funding in core counties will be reduced under this definition relative to the baseline definition. Since the survey respondents are concentrated in the core counties, the effect of additional funding per dollar is larger.

As above, the decrease in divorce likelihood for black respondents in counties receiving additional funding is replicated across all specifications except that which restricts attention to those married prior to the intervention. If the effect of funding is to discourage people in troubled relationships from getting married, then the estimates of the impact on divorce could be biased downwards through this channel. In this case, then the negative effect on divorce estimated in the baseline model is potentially a spillover from the negative effect on marriage estimated previously. When this spillover effect is eliminated by restricting attention to those married prior, the effect is reversed (and highly imprecise).

1.6 CONCLUSION

The federal Healthy Marriage Initiative is a program designed to encourage marriage, discourage divorce, and foster lasting and healthy relationships, especially amongst disadvantaged low-income and minority populations. Based on estimates from a longitudinal survey, black and low-income respondents residing in the counties receiving more funding are less likely to be married as a result of these funds, while the effect is reversed for non-black/non-Hispanic and high-income individuals. These results are consistent with the previous evaluations where positive results from relationship education curriculum are found primarily in program servicing a more affluent, white population (Hawkins et al.,

2008) and negative results are found in programs servicing economically disadvantaged populations (Wood et al., 2012).

Still, this is an ambiguous signal of the program's effectiveness; perhaps more marriages would arise in the absence of intervention, but that does not necessarily mean healthier relationships or better outcomes for children. It is possible that a benefit of relationship eduction and pre-marriage counseling lies in discouraging the promotion to marriage of marginal and potentially destructive relationships. Consistent with this hypothesis, I find that the same programs that reduce the likelihood of marriage in disadvantaged groups also decrease the likelihood of divorce, but only for individuals who had not selected into marriage prior to the intervention.

Three years may not seem a sufficient horizon over which to analyze these types of programs, yet they are designed primarily to encourage marriage among young, low-income couples with children. In light of mounting evidence that conditions in early life are crucial to childhood development and have lasting impacts on future education and labor market outcomes (Currie and Almond, 2011), we would hope to see some effect over this time frame. Thus far, the evidence is somewhat encouraging. However, while divorce prevalence has lowered in the areas receiving funding, the intensity of the funding does not seem to matter as much. Furthermore, effects seems to be larger for counties that had not previously received funding for such programs. As a result, future policy initiatives may wish to focus on increasing funding on the extensive margin and incorporating new counties and areas, rather than increasing funding on the intensive margin by funneling more money to counties with pre-existing programs and initiatives. In addition, while there is an abundance of evidence that marriages have beneficial effects on average, future research should focus on whether these beneficial effects hold for the marginal couples that are affected by these policy interventions. It may be that the couples targeted by policy interventions encouraging marriage would be better off never marrying. Initiatives such as the Federal HMI clearly can play a role in affecting marriage outcomes on a large scale; clearly, we must consider carefully what are the desired outcomes of such interventions in terms of marriage and divorce rates.

2.0 PROMISE SCHOLARSHIP PROGRAMS AS PLACE-MAKING POLICY: EVIDENCE FROM SCHOOL ENROLLMENT AND HOUSING PRICES (WITH RANDALL WALSH)

2.1 INTRODUCTION

In late 2005, the Kalamazoo Public School District announced a novel scholarship program. Generously funded by anonymous donors, the Kalamazoo Promise offers up to four years of tuition and mandatory fees to all high school graduates from the Kalamazoo Public Schools, provided that they both resided within the school district boundaries and attended public school continuously since at least 9th grade. The Kalamazoo Promise is intended to be a catalyst for development in a flagging region, encouraging human capital investment and offering incentives for households to remain in or relocate to the area. In the first eight years of the Kalamazoo Promise, research has documented a number of encouraging results, including increased public school enrollment, increased academic achievement, reductions in behavioral issues, and increased rates of post-secondary attendance.[1]

Encouraged by these early returns, many organizations have implemented similar programs modeled after the Kalamazoo Promise in urban school districts across the U.S. Still, most programs do not adhere exactly to the Kalamazoo archetype. Each iteration of the

[1]See Bartik et al. (2010); Bartik and Lachowska (2012); Miller-Adams and Timmeney (2013); Miron et al. (2011); Miller (2010); Andrews et al. (2010); Miller-Adams (2009, 2006); Miron and Evergreen (2008a,b); Miron et al. (2008); Miron and Cullen (2008); Jones et al. (2008); Miron et al. (2009); Tornquist et al. (2010) for some evaluations of the impact of the Kalamazoo Promise.

place-based "Promise" model varies in its features, including the restrictiveness of eligibility requirements, the list of eligible colleges and universities, and the generosity of the scholarship award itself. While research has been conducted on the Kalamazoo program and its impact on various outcomes of interest, this extant work only describes one particular intervention. As a result, we still know very little about the impact that such programs have on their communities. With hundreds of millions of dollars being invested in these human capital development initiatives, understanding their true impact is an important task for policy research.

This paper broadens the scope of our understanding of Promise programs by evaluating the impact of a broad cross-section of Promise programs on two targeted development outcomes: K-12 public school enrollment and home prices. In addition to providing the first estimates of the impacts over a set of multiple Promise programs, we document the significant heterogeneity of these effects across different constellations of program features. While the effect of regional policy on both public school populations and housing markets is of interest itself, including housing markets in the analysis allows us to speak to the valuation of this program across different groups by examining the variation in the capitalization effects across different neighborhoods and across the housing price distribution. Such patterns have important implications for the distribution of economic benefits from Promise programs.

First, we find that, on average, the announcement of a Promise program in a school district increases total public school enrollment. When analyzed by grade level, announcement leads to immediate increases in enrollment in primary schools (K-4) in particular. Since it is common in Promise programs to offer escalating benefits for students beginning their continuous enrollment at earlier grade levels, this pattern lends credence to a causal interpretation of our results. Dividing programs along prominent differences in design, we find that programs which offer scholarships usable at a wide range of schools provide the largest immediate boosts in total enrollment. In addition, some features of Promise programs have significant effects on the composition of affected schools. We find that merit requirements have differential effects across white and non-white enrollment decisions, leading to large increases in white enrollment and decreases in non-white enrollment, potentially exacerbating existing racial inequality in educational attainment.

In addition, within 3 years of the announcement of a Promise program residential properties within selected Promise zones experiences a 6% to 12% increase *on average* in housing prices relative to the region immediately surrounding the Promise zone, reflecting capitalization of the scholarship into housing prices.[2] This increase in real estate prices is primarily due to increases in the upper half of the distribution. These results suggest that the value of Promise scholarship programs is greater for higher-income families while simultaneously suggesting that the welfare effects across the distribution are ambiguous. While higher-income households seem to place a higher value on access to these scholarships, they also appear to be paying a higher premium for housing as a result.

Finally, for two Promise programs located in major metropolitan areas— Pittsburgh and Denver— we observe sufficient housing market transactions over the relevant time period to analyze the heterogeneity of housing market effects across schools within the Promise-eligible school districts. After linking housing transactions data to school attendance boundaries, we compare capitalization effects across the distribution of school quality within each city. Appreciation in housing prices is concentrated in Pittsburgh and Denver neighborhoods that feed into high quality schools (as measured by state standardized test scores). Since the previous evidence suggests that the increased demand is driven by high-income households, it is natural that it should be focused on areas with already high-achieving schools. However, this could have the effect of contributing to further inequality in educational outcomes if the high-income households attracted by Promise programs are exclusively attending already high-quality schools.

These results should guide those looking to establish new Promise programs or to

[2] Housing market data were not available for all Promise program locations. A sample of 8 Promise programs were utilized in this analysis.

tailor existing Promise programs. While place-based scholarships certainly can impact regional development, the basic features of the scholarship matter. Allowing students to use scholarships at a wide range of schools seems to be of first-order importance for total enrollment, with more flexible scholarships generating larger increases in total enrollment. The decision to impose merit requirements has important compositional effects on affected schools, leading to larger relative increases of white students in schools with merit-based programs. When combined with the distribution of capitalization effects, the evidence clearly suggests that Promise scholarships are having the largest impact on households in the middle- and upper-class. It is possible, however, that the change in peer composition and the increased tax base that result from increased demand amongst high-income, white households may have significant spillover effects on low-income and minority students in Promise districts. More research is needed to pin down the relative importance of these effects.

The following section will describe the relevant literature as well as the general structure of the Promise programs being analyzed, including the dimensions along which they vary. Section 2.3 will describe the data and the empirical methodology that will be used to estimate the impact of the program on public school enrollment and housing prices. Section 2.4 will be divided in to three subsections, the first of which will present the results of the enrollment analysis on the entire sample of Promise programs. The remainder of section 2.4 will be devoted to housing market analysis, first using a pooled sample of local housing markets in the second subsection and subsequently focusing on two of the larger urban areas in the final subsection. Finally, section 2.5 will discuss the results and conclude.

2.2 BACKGROUND

In addition to their policy ramifications, our findings contribute to two different strands of literature. First is the already substantial body of work regarding the provision of financial aid. There is a large literature addressing the impact of financial aid on postsecondary educational attainment.[3] Surveying contributions too numerous to cite individually, Dynarski (2002) reviews the recent quasi-experimental literature on the topic and concludes that financial aid significantly increases the likelihood that an individual attends college. Her estimates indicate that lowering the costs of college attendance by $1,000 increases attendance by roughly 4 percentage points. She further concludes that the distributional implications of aid are ambiguous. Estimates of the relationship between the impact of aid and income are evenly divided, with half indicating that the impact of aid rises with income. The studies she surveys focus exclusively on how financial aid affects the college attendance decision and choice of college. While our contribution will not address this question directly, we nevertheless provide important results on a recent development in the financial aid landscape. In particular, the implementation of Promise programs may either contribute to or mitigate inequality in educational attainment across racial groups, depending on the program design. We provide preliminary and indirect evidence that merit-based Promise scholarships in particular may favor white students in the distribution of benefits. In addition, our capitalization results suggest that high-income households are willing to pay more for access to Promise scholarships, although the true incidence of the subsidy remains unclear due to the effects of housing price capitalization.

The second strand of literature to which we contribute concerns research into place-based policies. Recently reviewed by Gottlieb and Glaeser (2008), these studies focus on outcomes such as regional employment, wages, population, and housing markets. The authors demonstrate significant agglomeration effects on these outcomes, suggesting the

[3] See Leslie and Brinkman (1988) for a review of early studies.

potential for policies aimed at redistributing population across space to have aggregate welfare implications. The caveat is that if agglomeration elasticity is constant across locations, redistribution can not have any overall effect. Any place-based policy aiming to capitalize on agglomeration externalities must rely on nonlinearities in the externality, otherwise the gains from population increases in one place will simply be offset by the loss of population in another. Indeed, the research on specific place-based interventions such as the Appalachian Regional Commission, Enterprise and Empowerment Zones, the Model Cities program, and urban renewal spending yield primarily negative results. The authors withhold comment on whether these projects were simply underfunded or such policies are ineffective in general, but the picture painted is not optimistic for the efficacy of such programs. Contributing further to this pessimism are Kline and Moretti (2014), who examine one of the more ambitious place-based policies in U.S. history: the Tennesee Valley Authority (TVA). The authors show that the TVA led to large, persistent gains in manufacturing employment which led to welfare gains through long term improvements manufacturing productivity. However, the productivity gains were exclusively the result of huge infrastructure investments; the indirect agglomeration effects of the policy were negligible. The central message is that, while large place-based interventions can bolster one locality at the expense of another, any gains will evaporate with the termination of the policy and persistent net welfare gains are rare. We find that place-based Promise scholarship programs do in fact increase public school populations and housing prices, which is plausibly explained by the scholarship increasing the willingness to pay for housing in these areas. The existing literature suggests that these effects would evaporate upon the withdrawal of the scholarship program from the area, unless the Promise intervention is to human capital what a program like the TVA is to physical capital. In that case, the direct productivity effects of Promise scholarships may have lasting effects, although the indirect agglomeration effects on productivity are likely to be minimal.

The overlap of financial aid and place-based policy did not begin with the Kalamazoo

Promise, but until recently place-based financial aid had been the domain of state education agencies. The Georgia HOPE scholarship has been in place since 1993, awarding scholarships to Georgia high school graduates who satisfy GPA requirements and enroll at a Georgia college or university. Like the Kalamazoo Promise, many states used the HOPE scholarship as a model when introducing statewide merit-based scholarships of their own. Several studies have thoroughly examined the impact of the HOPE scholarship program on outcomes such as student performance in high school (Henry and Rubenstein, 2002), college enrollment (Dynarski, 2000; Cornwell et al., 2006), college persistence (Henry et al., 2004), and degree completion (Dynarski, 2008). To summarize the findings, the HOPE scholarship has led to overall improvements in K-12 education in Georgia as well as reductions in racial disparities. In addition, college enrollments increased among middle- and high-income students, but income inequality in college enrollments widened and college persistence was not necessarily increased. While evaluating place-based policies, it is notable that most of the research on these programs has focused on the outcomes typically associated with the financial aid literature— i.e. impact on college attendance, degree completion, and the impact of merit scholarships on educational inequality. Because of the statewide nature of these programs, outcomes on a smaller spatial scale that would interest place-based policy researchers— i.e. impact on regional development outcomes, population, public school enrollments, and housing markets— have been largely ignored.

The unexpected introduction of place-based Promise scholarship programs in school districts across the U.S. provides a series of natural experiments similar to those provided by statewide scholarships. However, the smaller geographic scale allows us to study local outcomes for the first time, using the immediate geographic vicinity of a Promise school district as a plausible counterfactual. With an ever-expanding sample of Promise programs implemented at different times in different regions, we can now assess the impact of providing place-based scholarships on a number of relevant but hitherto ignored outcomes, as well as how these impacts vary with the design of the program.

2.2.1 PROMISE SCHOLARSHIP PROGRAMS

According to the W.E. Upjohn Institute for Employment Research, a Promise-type scholarship program is a "universal or near-universal, place-based scholarship program." Upjohn has identified a list of 23 such programs (plus the Kalamazoo Promise itself). These programs are listed in Appendix Table A1 along with some other details of the programs themselves.[4,5]

In practice, the place-based nature of these scholarships is dictated by the requirement that a student maintain continuous enrollment in a particular school district (or other small collection of schools) for several years prior to graduation to receive any benefit.[6] Although the continuous enrollment requirement alone constitutes a restriction on residential location for most U.S. households, many programs pair this with an explicit requirement for continuous residence in the district itself.

Although the Kalamazoo Promise was universal within its Promise zone as can be seen in Appendix Table A1, many Promise programs have other eligibility requirements. Minimum GPA requirements, minimum attendance requirements, and community service requirements are common. Previous work has called attention to the variation in eligibility requirements as an important element in program design, but to date no research has empirically investigated the impact of universal vs. merit-based eligibility on program effectiveness in the context of Promise programs. Miller-Adams (2011) documents the successes of the Kalamazoo Program and attributes some results to its universal eligibility. In particular, the Kalamazoo Pubilc Schools experienced increases in enrollment without

[4]The majority of the list of Promise-type scholarship programs was obtained from `http://www.upjohninst.org/Research/SpecialTopics/KalamazooPromise`. Further research revealed an additional Promise program in Buffalo, NY, which has been added to the list. All other information is based on a review of each program's website.

[5]Of the programs detailed in Appendix Table A1, a number are excluded for data availability or other reasons. Of particular interest is the intervention located in Detroit, MI which is excluded from the analysis because the preciptous decline of Detroit in the years surrounding the Promise is likely to overshadow the relatively insignificant intervention, as discussed in detail in the following section.

[6]While not always defined in terms of school districts, we will use the terms "Promise district", "Promise area", and "Promise zone" interchangeably to refer to the geographical boundaries of a Promise program.

significant changes in the ethnic, racial, or socioeconomic composition of its schools. This pattern is attributed to the universality of the Kalamazoo Promise. Without an accompanying analysis of near-universal programs, however, it is unclear whether similar results could be obtained from very different interventions. In addition, some districts' goals may include modifying the demographic composition of area schools. For example, Schwartz (2010) indicates that relocating disadvantaged children to low-poverty schools has large and lasting effects on their educational achievement. The analysis to date provides districts looking to capitalize on such effects with no guidance regarding what program design choices best suit their goals.

Bangs et al. (2011) review existing research on the effects of merit and universal place-based scholarship programs on K-12 enrollment, student achievement, college attainment, and inequality. Relative to merit aid, the universal scholarships they study are more effective at increasing school district enrollment and reducing poverty and racial disparities in educational attainment. However, the authors include only the Kalamazoo Promise and the Pittsburgh Promise from the class of Promise programs. In addition, direct evidence of the impact of the Pittsburgh Promise is scant; most comparisons are made between Kalamazoo and statewide programs such as the Georgia HOPE scholarship. Using data from over 20 Promise-type programs announced to date, many of which include a merit eligibility requirement, we present direct evidence on the contrast between merit-based and universal programs, specifically in the context of place-based Promise scholarship programs.

Eligibility requirements are scarcely the only source of heterogeneity in program design; the scholarship award itself varies across programs. By way of example, the maximum award for the Jackson Legacy scholarship is $600 per year for two years, whereas the Pittsburgh Promise recently increased their maximum scholarship award from $5,000 to $10,000 per year for up to four years. The maximum scholarship duration varies as well from one year (Ventura College Promise) to five years (El Dorado Promise and Denver Scholarship Foundation). However, the exact degree of variation in benefits is obfuscated by two common features of Promise scholarships. First, scholarships are often stated in percentage terms of tuition, which makes the value dependent on the choice of postsecondary institution. Second, many Promise programs award benefits on a sliding scale based on the grade at which the student first enrolled in a Promise zone school. As an example of both, the Kalamazoo Promise benefit ranges from 65% (enrolled grades 9-12) to 100% (enrolled grades K-12) of tuition and mandatory fees at a Michigan public college or university. As a result, the expected benefit of a Promise scholarship varies across locations in a way that is difficult to quantify, but is nevertheless significant.

The last major feature we will address the list of colleges and universities towards which the scholarship applies. Most programs require enrollment at an accredited postsecondary institution located within the same state as the Promise zone. Some limit that further to public institutions, while many scholarships are only usable at a short list of local colleges. This aspect of the program has a substantial impact on both the value of the scholarship in absolute terms and the distribution of its benefits across groups. For instance, some programs have flexible scholarships that allow use at a large list of institutions including trade schools as well as nationally-ranked four-year universities. Naturally, scholarships that allow use at more expensive schools are potentially more valuable to their recipients. In addition, the variation in price points and selectivity within the list of eligible schools makes the scholarship valuable to both low-income and high-income households alike. Programs with inflexible scholarships typically allow use only at local junior and community colleges. This restriction not only caps the benefit of the scholarship to full tuition at one particular school, but also presents less value to high-income graduates focused on four-year programs.

As the oldest program in its class, a considerable amount of research has evaluated the impact of the Kalamazoo Promise on the outcomes of students in the Kalamazoo Public School District.[7] A series of working papers from Western Michigan University's Depart-

[7] We have found sources that indicate Pinal County's "Promise for the Future" program started as early as 2001. It is perhaps more accurate to say that the Kalamazoo Promise is the oldest widely-recognized program in this class.

ment of Education outline the mechanism for community development in principle, with the Promise generating increased attendance in secondary school leading to better classroom performance and graduation rates and ultimately increased college attendance in the region. Their research to date culminated in Miron et al. (2011) which presents quantitative and qualitative evidence documenting a significant improvement in school climate following the announcement of the Promise.[8] In addition, the W.E. Upjohn Institute for Employment Research has taken a leading role in research surrounding the Kalamazoo Promise. Researchers there have determined that the Kalamazoo Promise has successfully increased enrollment (Hershbein, 2013; Bartik et al., 2010), improved academic achievement (Bartik and Lachowska, 2012), and increased college attendance in certain groups (Miller-Adams and Timmeney, 2013). Finally, Miller (2010) confirms the documented positive effects on public school enrollment, achievement, and behavioral issues. She also adds a preliminary analysis of home values, finding that the announcement of the Promise had no impact on home prices in Kalamazoo relative to the surrounding area.

Apart from these studies of the Kalamazoo Promise, however, little research has been conducted on Promise programs in order to generalize the findings. Gonzalez et al. (2011) study the early progress of Pittsburgh's Promise program and find that it stabilized the previously declining public school enrollment in the Pittsburgh public schools. The study also presents survey-based and qualitative evidence that the Pittsburgh Promise's merit-based eligibility requirements motivate students to achieve and that the Promise was influential in the decisions of many parents to move their children to city public schools. Additionally, some programs' websites present internal research intended to promote the program's progress. Importantly, all studies to date have been limited in scope to an individual Promise location. Also, with the exception of some work regarding Kalamazoo, the research has been primarily qualitative or descriptive in nature. In the remainder of the

[8] See Miron and Evergreen (2008a), Miron and Evergreen (2008b), Miron et al. (2008), Miron and Cullen (2008), Jones et al. (2008), Miron et al. (2009), and Tornquist et al. (2010) for more evidence from their evaluation of the Kalamazoo Promise program.

paper, we will present the first research which utilizes data from a broad array of Promise-type programs. We present direct evidence on the effectiveness of Promise scholarships in increasing public school enrollments, as well as document patterns in enrollment across different programs which are clearly related to program details such as eligibility requirements and award amounts. In addition, we present the first analysis confirming the influence of Promise scholarship programs on property values, the results of which also have interesting implications for future program design.

2.3 DATA AND METHODOLOGY

Our estimation strategy for measuring the impact of the Promise hinges on treating the announcement of a Promise program in a region as a natural experiment, relying on the assumption that the announcement in each area was unexpected. To justify this assumption, we conducted substantial research into the timing of program announcements in each area that we study. The date of the announcement that we use in our analysis corresponds to the earliest mention we could find of the program's existence. Typically, this corresponds to the date of a press release announcing the program. In cases where press releases were unavailable, we used the Internet Archive at http://www.archive.org to find the earliest iteration of the program's own home page, using the archival date as the announcement date. We were able to determine the approximate date of announcement for 22 of the 25 known programs in Appendix Table A1; the remaining 3 were excluded from the analysis.[9] It is likely that some of our announcement dates will be subject to measurement error. This problem is mitigated somewhat in the public school analysis, as enrollment data evaluated on an annual basis. In addition, any bias resulting from measurement error should serve to attenuate our estimates of the true effect of these programs.

[9]The excluded programs were the Educate and Grow Scholarship (Blountville, TN), the Muskegon Opportunity Scholarship (Muskegon, MI), and School Counts! (Hopkins County, KY).

In addition to those programs mentioned above, the Detroit College Promise was also excluded from the analysis. The reasons for this exclusion are two-fold. First, the intervention in Detroit was very small. The maximum scholarship attainable under the Detroit Promise is $500 per year, and that only for the initial two cohorts of graduates from a particular high school; most other students are entitled to a maximum award of $500 total.[10] This small award is due to the lack of sponsorship for the Detroit Promise; as of June 13, 2013, there was only one donor to the Detroit Promise that contributed over $50,000. Contrasted with the 35 such donors to the Pittsburgh Promise, it is obvious why the Detroit Promise is not capable of offering larger scholarships to its graduates. Second, we believe the precipitous decline of a city on the verge of bankruptcy is likely to overshadow any small positive impact on house prices that may have been generated by the Detroit Promise. In the year following the announcement of the Detroit Promise, two of the so-called "Big 3" automakers based in and around Detroit filed for bankruptcy, followed by the city itself filing for bankruptcy in 2013. From 2000 to 2010, Detroit experienced a 25% decline in population— the largest percentage decrease in population for a U.S. city aside from the exodus out of New Orleans after Hurricane Katrina in 2005. Because of these non-Promise related factors, we believe Detroit to be non-representative of the typical Promise program and we exclude it from all results below.

There are two main outcomes that we will be interested in studying in relation to Promise Scholarship programs: K-12 public school enrollments and housing prices. Naturally, identifying and estimating the impact of the Promise presents a unique set of empirical challenges for each outcome of interest. We will first present a description of the data and empirical strategy used to analyze the impact of Promise programs on K-12 enrollment, followed by a similar section devoted to the data and methodological concerns related to our housing market analysis.

[10]The exception to this is the graduating class of 2013, who it was recently announced will receive $600 scholarships from the Detroit Promise.

2.3.1 PUBLIC SCHOOL ENROLLMENT

Our data source for public school enrollments is the National Center for Education Statistics' Common Core of Data (CCD). The CCD surveys the universe of public schools in the United States every year. Among the data collected in the survey are the names and locations of all schools, the operational status code as of the survey year, the instructional level of the school (primary, middle, high), student enrollment counts by grade and by race/ethnicity, and staff counts. As all Promise programs were announced after the year 2000, we retrieved CCD records dating from the 1999-2000 survey year up to the most recently available 2010-2011 survey year.[11] This yielded a total of 1.2 million school-year observations. This data was then combined with information on which schools' students were eligible for Promise scholarships and the year the programs were announced.

Ultimately, the goal is to estimate the change in enrollments resulting from the announcement of the 21 Promise programs observed. For causal inference, however, it is not sufficient to compare student counts in Promise districts prior to the announcement with student counts after the announcement. We require an appropriate counterfactual to account for the possibility that similar (or proximate) schools unaffected by the Promise may have also experienced increases or decreases in enrollment as a result of some unobserved common shock. The interpretation of an increase in Promise school enrollment counts changes substantially if similar but unaffected schools experienced increases just as large, for example. As such, we use a difference-in-differences approach to identify the causal impact of Promise program announcement. We estimate variations of the following fixed-effects regression

$$Y_{it} = \alpha + \beta Post_{it} \cdot Promise_i + \mathbf{X'_{it}} \cdot \gamma + \eta_{it} + \delta_i + \varepsilon_{it}, \tag{2.1}$$

[11]Five programs— Say Yes Buffalo (Buffalo, NY), the Sparkman Promise (Sparkman, AR), the Arkadelphia Promise (Arkadelphia, AR), the New Haven Promise (New Haven, CT), and the Great River Promise (Phillips and Arkansas Counties, AR)— were announced recently enough that no post-announcement data is yet available. However, the pre-announcement data for these Promise Zones and their surrounding areas is included in our analysis to help estimate nuisance parameters more precisely. Importantly, the exclusion of these observations does not qualitatively change our estimates.

where Y_{it} is the natural log of enrollment in school i in year t, $Post_{it}$ is an indicator for surveys occurring after the announcement of the Promise program relevant to school i, $Promise_i$ is an indicator for schools located in Promise zones, $\mathbf{X_{it}}$ is a vector of characteristics school i in year t, η_{it} is a vector of region-by-year and urbanicity-by-year fixed effects, and δ_i are school fixed effects. Standard errors in all specifications are clustered at the school level to allow for correlation in ε_{it} within schools over time.

In addition, some results will be presented that modify Equation 2.1 as follows

$$Y_{it} = \alpha + \sum_{J \in \{M,NM\}} \sum_{K \in \{W,NW\}} \beta_{JK} Post_{it} \cdot J_i \cdot K_i + \mathbf{X'_{it}} \cdot \gamma + \eta_{it} + \delta_i + \varepsilon_{it} \tag{2.2}$$

yielding four coefficients— $\beta_{MW}, \beta_{NMW}, \beta_{MNW}$, and β_{NMNW}— where M_i indicates a Promise program with a merit-based eligibility requirement, NM_i indicates a universal Promise program, W_i indicates a Promise program with a broad (more than three) list of eligible postsecondary institutions, and NW_i indicates a Promise program with a narrow (no more than three) list of eligible postsecondary institutions. This specification allows us to answer questions regarding how the impact of Promise programs varies along prominent design dimensions.

The coefficients of interest in the above equation estimate the impact of Promise announcement on school outcomes— or average treatment effect— provided that the chosen control schools act as an appropriate counterfactual for the evolution of K-12 enrollment in the absence of treatment. Our estimation strategy will use geographically proximate schools as our control group for schools located in Promise zones. As a result, we limit our attention to schools that were located in the county or counties surrounding the treated schools. The intuition for this control group is that schools in the same county or neighboring counties will be affected by the same regional shocks to K-12 enrollment as their treated counterparts, such as broad regional migration or demographic patterns. In addition, we only include surveys conducted within 4 years of the announcement date of the Promise program relevant to the school in question. Finally, we only include observations from schools which reported total student counts and student counts by race/ethnicity in every available survey within the estimation window.[12] This restriction results in our baseline estimation sample of 47,600 school-year observations across 74 U.S. counties and 947 school districts. Table 2.1 presents the summary statistics for the sample of treated and untreated schools across all years in the sample.

The schools initiating Promise scholarship programs are statistically different from those in the geographically proximate control group. Schools in Promise zones have fewer students overall and fewer white students as a fraction of the total students (although this difference, while statistically significant, is very small). In addition, the Promise schools are much more likely to be located in urban areas, naturally making the nearby schools in the control group much more likely to be in suburban areas. Differences in the distribution of schools across levels are very similar, although the more urban Promise districts tend to have fewer schools designated as middle schools in the CCD.

Bear in mind, our empirical strategy does not explicitly rely on Promise schools being similar to comparison schools. Provided that Promise schools and non-Promise schools are not becoming more or less dissimilar over the period prior to the Promise announcement, our estimates should identify the causal impact of the Promise announcement. Specifically, identification of the causal effect of the Promise announcement requires that the outcomes of interest would follow parallel trends (conditional on observable covariates) in the absence of any intervention, such that any difference in the period following announcement can be attributed to the treatment itself. Importantly, this assumption can not be explicitly tested as we do not observe the true counterfactual. In the next section, however, we will present graphical evidence in support of this assumption. Specifically, we will demonstrate that the evolution of enrollment in the periods immediately prior to Promise announcement was similar between Promise zone schools and control schools. This requirement also implicitly assumes that no other major changes are occurring in one group and not the other at

[12] Relaxing this restriction only slightly changes the estimated coefficients.

Table 2.1: K-12 Public School Summary Statistics

		Promise Schools	Control Schools	t-stat
Total Enrollment	mean	599.70	745.36	24.71
	(s.d.)	(431.00)	(615.82)	
% White	mean	0.44	0.44	1.84
	(s.d.)	(0.31)	(0.35)	
Primary	mean	0.67	0.68	0.92
	(s.d.)	(0.47)	(0.47)	
Middle	mean	0.17	0.16	1.81
	(s.d.)	(0.38)	(0.37)	
High	mean	0.14	0.14	1.57
	(s.d.)	(0.35)	(0.35)	
City	mean	0.57	0.39	-28.92
	(s.d.)	(0.50)	(0.49)	
Suburb	mean	0.25	0.45	38.82
	(s.d.)	(0.43)	(0.50)	
Town	mean	0.05	0.04	0.80
	(s.d.)	(0.21)	(0.20)	
Rural	mean	0.14	0.12	-4.06
	(s.d.)	(0.35)	(0.32)	
Obs		5,287	42,313	

Notes: T-statistic from a two-sided t-test with unequal variance.

approximately the same time as the treatment is occurring. While we can not rule this out, due to the time variation in the announcements of the geographically diverse set of programs it is unlikely that any shock other than the Promise program announcement would have occurred in all Promise zones at the time of announcements, especially a shock that would differentially impact Promise zones relative to their immediate surroundings.

2.3.2 HOUSING PRICES

Our housing price data comes primarily from DataQuick Information Systems, under a license agreement with the vendor. These data contain transactions histories and characteristics for properties in a large number of U.S. counties. Included in the data collected are sales of newly constructed homes, re-sales, mortgage refinances and other equity transactions, timeshare sales, and subdivision sales. The transaction related data includes the date of the transfer, nominal price of the sale, and whether or not the transaction was arms-length. In addition, every building in the data has characteristics as recorded from the property's most recent tax assessment. These variables include floor area, year built, number of bedrooms, number of bathrooms, and lot size.[13] Finally, the latitude and longitude of each property is also included.

The location of the property is crucial to the analysis. Locating the property within a Census tract allows us to combine property characteristics with neighborhood demographic data from the U.S. Census and also allows us to control for unobserved neighborhood characteristics through the use of fixed effects. We require a fixed geographical definition of a neighborhood for the latter, but Census tract definitions change over time. Fortunately, the Longitudinal Tract Database (LTDB) has developed tools to estimate any tract-level

[13]Note that not all variables are reliably recorded across all jurisdictions. Most jurisdictions reliably record floor area and year built, but other details are often unreliably encoded (i.e. missing values, unrealistic quantities, no variation in codes, etc.). As a result, any analysis that pools data from all markets only includes floor area (in square feet) and a quartic in building age in specifications where structural characteristics are included. These characteristics were the only variables that were reliably recorded across all jurisdictions studied.

data from the 1970 onward for 2010 Census tract definitions. So, properties were allocated to 2010 Census tracts and historical neighborhood demographic data was estimated based on these tools, interpolating between years when necessary. These demographic data include median income, racial composition, age distribution, educational attainment, unemployment rates, fraction in poverty, fraction of family households, and private school attendance. Also, geographical data allows us to match properties to school districts, counties, or Census places using U.S. Census TIGER files. As Promise eligibility is ultimately determined by location within these boundaries, this is crucial for determining which properties are eligible to receive Promise scholarships.

Unfortunately, not all counties that are home to Promise programs are covered by DataQuick. As a result, the housing market analysis necessarily focuses on a subset of eight Promise zones due to data limitations.[14]

As with demand for public schools, there is reason to believe that the announcement of a Promise program will increase demand for housing within the Promise zone. However, unlike with K-12 enrollment data, housing market data gives us an indication of the value of the announcement of the Promise to households. Since we observe the transaction price associated with the residential location decision, we can draw inference on the household's willingness to pay for access to the program. Assuming that housing supply is fixed in the short-run, any increase in the average household's willingness to pay must be capitalized into prices. As a result, by identifying the change in housing prices attributable to the announcement of a Promise program, we will recover the capitalization of program announcement into housing prices, providing a signal of the average household's marginal

[14]For only six of these does the data originate from DataQuick. For two Promise programs— Say Yes Syracuse (Onondaga County, NY) and the Kalamazoo Promise (Kalamazoo County, MI)— real estate transaction and assessment data was pulled from public records on the internet. For Onondaga County, parcel information and transaction histories were obtained from the Office of Real Property Services (ORPS) websites at http://ocfintax.ongov.net/Imate/search.aspx (for Onondaga County) and http://ocfintax.ongov.net/ImateSyr/search.aspx (for City of Syracuse). For Kalamazoo and neighboring Van Buren county, parcel information and transaction histories for each property were gathered from the BS&A Software portal for Kalamazoo and Van Buren Counties at https://is.bsasoftware.com/bsa.is/. In terms of the scope of content, the data acquired in this way is comparable to those supplied by DataQuick.

willingness to pay for access to the program.[15]

In practice, however, identifying the causal impact of a change in a local amenity like access to a Promise scholarship is not trivial. In this paper, we use the hedonic method to model a property's price.[16] In general, the hedonic method expresses the transaction price of a property as a function of the characteristics of that property. The implicit price of a characteristic is then recovered by estimating the hedonic price function via regression. In addition, Parmeter and Pope (2013) demonstrate how combining this technique with quasi-experimental methods allows the researcher to exploit temporal as well as cross-sectional variation in amenity levels. Recent studies have used quasi-experimental hedonic methods to recover the value of school quality (Black, 1999; Barrow and Rouse, 2004; Figlio and Lucas, 2004), air quality (Chay and Greenstone, 2005), airport noise (Pope, 2008a), toxic releases (Bui and Mayer, 2003; Gayer et al., 2000), flood risk reduction (Hallstrom and Smith, 2005; Pope, 2008b), crime reduction (Linden and Rockoff, 2008; Pope, 2008c), and mortgage foreclosures (Cui and Walsh, 2013). We adopt this technique as well in our estimation of the causal impact of Promise programs on housing prices.

As above, our estimation strategy will employ a difference-in-differences approach to identify the causal impact of Promise program announcement, which is fairly standard in the quasi-experimental hedonic valuation literature. Our baseline estimating equation is written as follows:

$$Price_{imdt} = \alpha + \beta Post_{mt} \cdot Promise_d + \mathbf{X'_{it}} \cdot \gamma + \eta_{mt} + \delta_d + \varepsilon_{imdt}, \tag{2.3}$$

where $Price_{imdt}$ is the natural log of the transaction price for property i in market m and school district d at time t, $Post_{mt}$ is an indicator for transactions occurring after the

[15] Kuminoff and Pope (2009) demonstrate that capitalization is equivalent to marginal willingness to pay only if the hedonic price function is constant over time and with respect to the shock being analyzed or if the shock is uncorrelated with remaining housing attributes. Neither condition is likely to be satisfied here and consequently our estimates are not directly interpretable as marginal willingness to pay. However, we present results that identify capitalization from repeat sales data which has been shown in Monte Carlo experiments to drastically reduce so-called "capitalization bias" over pooled OLS (Kuminoff et al., 2010).

[16] For a thorough review of the hedonic method, Bartik and Smith (1987), Taylor (2003), and Palmquist (2005).

announcement of the Promise program relevant to housing market d, $Promise_d$ is an indicator for properties located in Promise zones, $\mathbf{X_{it}}$ is a vector of building and neighborhood characteristics of property i at time t, η_{mt} are market-by-year-by-quarter fixed effects, and δ_d are school district fixed effects. Market-by-year-by-quarter fixed effects account for regional shocks in housing prices in a given period, while district fixed effects control for static differences between neighborhoods over time. We also estimate variations on the above equation, where school district fixed effects are replaced by 2010 Census tract fixed effects and, finally, property fixed effects. The property fixed effects specifications yield our preferred estimates of the treatment effect, identifying the impact of treatment from repeat sales only and thus controlling for any time-invariant unobservables associated with an individual property. Standard errors in property fixed effects regressions are clustered at the property level to allow for correlation in ε_{imdt} for the same property over time; all other specifications cluster standard errors at the 2010 census tract level. Again, β identifies the impact of Promise announcement on housing prices provided that the prices of control properties would have evolved similarly over time in the absence of treatment.

For several reasons, we expect that the value of most Promise programs may increase with household income. Light and Strayer (2000) find that family income and mother's education level increase both the likelihood of college attendance as well as the selectivity of the chosen school, thus making the Promise scholarship more valuable to higher-income, higher-educated households. In addition, many Promise scholarships are "middle-dollar" or "last-dollar" aid, ultimately applied towards unmet need at your institution of choice after the application of federal, state, and institutional aid. Importantly, while Promise aid is typically *not* need-based, these other sources of aid are typically dependent on the expected family contribution (EFC) as calculated by the household's Free Application for Federal Student Aid (FAFSA) form, with lower income families expected to contribute less than higher income families. As a result, for an identical institution, higher income families are likely to receive less aid than lower income families from these other sources, leaving a larger amount of unmet need. For these reasons, the value of the Promise should be greatest for families with higher incomes. As it is reasonable to expect these higher income families to occupy higher priced domiciles, we would like to test this hypothesis by allowing the treatment effect to vary across the housing price distribution. As such, we perform a two-step procedure that first defines where properties lie on the *pre-Promise* distribution of housing prices— even for properties sold after the Promise— and subsequently estimates treatment effects both above and below the median of said distribution via OLS.

The first step is accomplished by restricting attention to the pre-Promise period in each housing market and estimating a standard hedonic price function which includes all observable property-specific characteristics, i.e. structural and neighborhood features, and controls flexibly for time through quarterly fixed effects. The coefficient estimates from this regression are then used to predict the sale price of each property observed in the sample— including those sold after Promise announcement— as if it had been sold in the first quarter of the year prior to the announcement. The resulting number provides a measure of the component of housing value that is unaffected by the treatment by construction. All transactions are then sorted on this statistic and grouped into observations above and below the median. This exercise tells us where a property would have fallen in the housing price distribution for that particular housing market if the transaction had taken place prior to the announcement of the Promise.[17]

The second step simply repeats the DD analysis specified in equation 2.3, but separately for properties above and below the median of the distribution generated by the first step. Each β then estimates the treatment effect of the Promise announcement within each half of the housing price distribution.

It is worthwhile to briefly discuss the functional form assumption implicit in equation

[17]As discussed below, in some specifications the estimation sample will be restricted either geographically or as a function of observable characteristics. A property's rank in this distribution is based on the widest definition of the housing market and will not depend on the estimation sample. As a result, the above and below median sample will not necessarily contain an equal number of observations when estimation samples are restricted in this way.

2.3. The semi-log functional form, with the natural log of price as the dependent variable, is fairly standard in the hedonic literature and has been justified by Monte Carlo simulations performed initially by Cropper et al. (1988) and more recently by Kuminoff et al. (2010). However, we will also present estimates using a fully linear functional form with deflated transactions prices as the dependent variable. As all Promise scholarships are per-student subsidies and not a per-housing-unit subsidies, there is reason to suspect that the causal effect of the program is better interpreted in levels and not logs. For example, consider two identical families each with one child, one moving into a 2 bedroom house and one moving into a 10 bedroom house in the same neighborhood in a Promise zone. Both families will be willing to pay more for the house after the announcement of the Promise as their child will receive the scholarship with some positive probability. Yet, the expected value of the benefit is the same even though the 10 bedroom house is undoubtedly priced higher than the 2 bedroom house. As such, we would not expect both families to be willing to pay the same *percentage premium* after the announcement of the Promise, which is what would be captured by a DD estimate in logs.

Another important consideration in any hedonic model is the spatial definition of the relevant housing market. The trade-off between using a large geographic housing market and a small geographic housing market is one between internal validity of the estimates and the precision with which they are estimated (Parmeter and Pope, 2013). As such, we take a flexible approach by estimating our equation on a number of different samples, each representing a different housing market definition.

After determining the geographic extent of each of the eight Promise programs, two estimation samples were constructed: one representing a relatively large housing market definition and one representing a small housing market definition. The large housing market is constructed by including all transactions within Promise zones as well as all transactions occurring within 10 miles of the geographic boundary of the Promise zone. The small sample is constructed by only using transactions within a 1 mile bandwidth along both

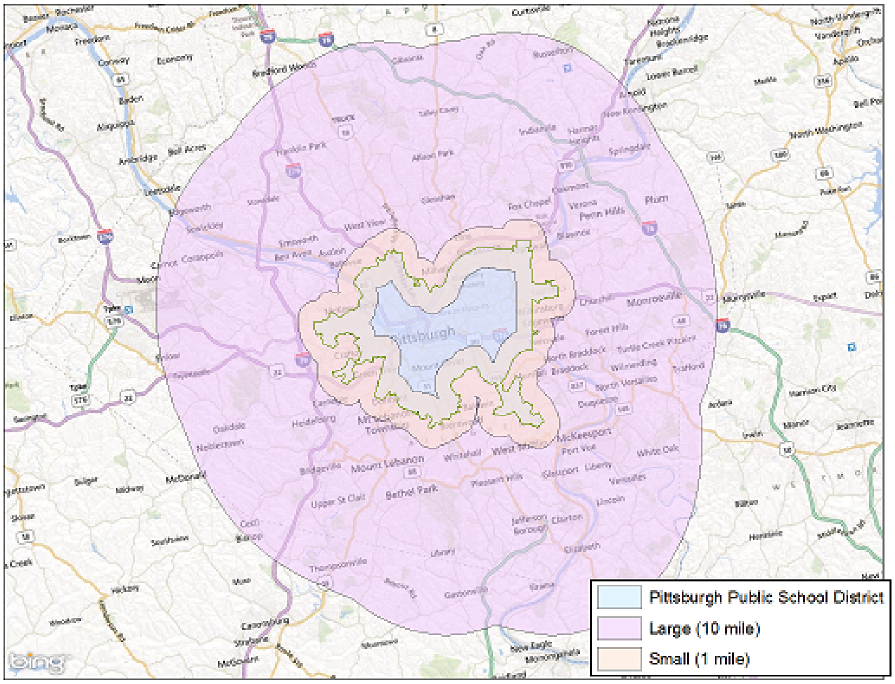

Figure 2.1: Large (10 mile) and Small (1 mile) Housing Markets in Pittsburgh, PA

sides of the Promise zone boundary. Figure 2.1 depicts an example, using the housing markets constructed around the Pittsburgh Promise treatment area.

The large sample affords us many observations of market transactions and thus provides precise estimates. However, the concern in a large sample is that the estimate of the treatment effect will be biased if either the scholarship is not relevant to households in the periphery of the sample or they are simply unaware of the program. The small housing sample mitigates this bias by constructing a sample over which we can be relatively sure that all households will be informed of the scholarship and consider it relevant. The variance of the estimate, however, increases due to the smaller number of observations from which

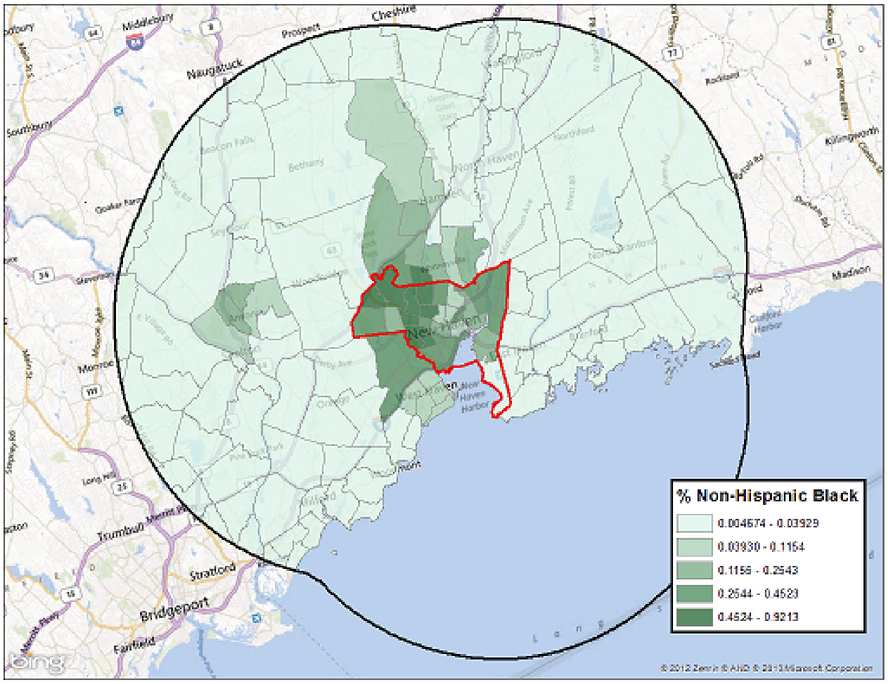

Figure 2.2: New Haven, CT: Percent Non-Hispanic Black (2000) by Census Tract

to draw inference. The goal in estimating our hedonic model on both samples is to evaluate the sensitivity of the measured treatment effect to the choice of housing market definition.

In addition to the two geographically defined markets, we also construct a housing market that, while bounded geographically, is defined in statistical terms. Even in the small housing markets defined above, it is possible that properties on either side of the treatment boundary can vary significantly and discontinuously in terms of observable characteristics, calling into question their use as a counterfactual for houses within the treatment area. By means of example, Figure 2.2 depicts the Promise zone in New Haven, CT (outlined in red) along with its corresponding large housing market (outlined in black). The area is subdivided into census tracts and color coded by racial composition according to the 2000 U.S. Census. As can plainly be seen, neighborhoods vary considerably across the border defining the Promise zone. While this difference in observables can be controlled for econometrically, it raises the question of variation in unobservables and, more importantly, the validity of the parallel trends assumption required for causal interpretation of DD estimates.

In econometric terms, our concern is with limited overlap in observables between treatment and control groups which can cause "substantial bias, large variances, as well as considerable sensitivity to the exact specification of the treatment effect regression functions." (Crump et al., 2009). As such, we would like to define a sample that reduces these concerns by trimming some observations in the non-overlapping region of the support, while simultaneously minimizing the variance inflation that accompanies the reduction in observations.

After pooling all large housing markets defined above, we follow Crump et al. (2009) to define what the authors refer to as the optimal subpopulation. We estimate the following logit model to predict the probability that a transaction occurs within a Promise zone based on pre-Promise property characteristics:

$$\text{Prob}(Promise_d|\mathbf{X_i}) = \frac{1}{1 + e^{\alpha + \mathbf{X_i'} \cdot \gamma}}, \tag{2.4}$$

where $\mathbf{X_i}$ is a vector of time-invariant characteristics of property i including floor area (in sq. feet), a quadratic in building age, and available 2000 U.S. Census demographic information at the tract level.[18] Recovering the associated parameters, we go on calculate the predicted value of $Promise_d$, obtaining propensity scores for all properties in the

[18] As all Promise programs were announced after the year 2000, there is no endogeneity concern introduced by using Census demographics. Building age is similarly unaffected by endogeneity concerns as it is constructed as the difference between year built and year of transaction. Unfortunately, we do not observe variation in other building characteristics, so for each property we do not know whether we observe post-Promise floor area (which could potentially be endogenous to Promise announcement) or pre-Promise floor area (which would necessarily be exogenous to Promise announcement) of each property. However, over our short estimation window, is seems unlikely that floor area would respond to Promise announcement in any systematic or meaningful way.

large housing market sample. We then trim the sample to observations with intermediate propensity scores.[19] Equation 2.3 is then estimated on this sample, producing the Optimal Subpopulation Average Treatment Effect (OSATE).

Finally, we wanted to document any heterogeneity in capitalization effects across the distribution of school quality. It is well-known that the residential location decisions of households with children are heavily influenced by school quality. If the intention of these programs is in part to encourage the migration of households into Promise districts from nearby areas with higher quality schools, it stands to reason that increases in demand for housing should be concentrated in Promise area neighborhoods with access to relatively high quality schools. For two major metropolitan Promise zones— Pittsburgh and Denver— we were also able to obtain school attendance boundaries from the Minnesota Population Center's School Attendance Boundary Information System (SABINS). After matching properties to schools and obtaining standardized test scores at the school level from each state's education agency, we were able generate standardized pre-Promise measures of primary school and high school quality for each property in the Pittsburgh and Denver samples. First, we divide the universe of schools on the basis of the highest tested grade level, with schools testing only 8th graders and lower being labeled primary schools and schools testing any students higher than 8th grade being labeled high schools. Then, we calculate the percentage of tested students scoring proficient or better on standardized tests (math and reading) in the universe of public schools in Colorado and Pennsylvania for the year 2005. Finally, within each state by school level cell we standardize this measure such that the resulting variable is a Z-score distributed with mean zero and unit standard deviation.

Pooling these two markets, we directly estimate how Promise capitalization varies with school quality by estimating variations of the following equation in each market definition:

[19]The optimal bounds of the propensity score distribution were calculated according to Crump et al. (2009). We thank Oscar Mitnik for sharing the code for the procedure on his website.

$$Price_{imdt} = \alpha + \beta Quality_i \cdot Promise_d \cdot Post_{dt} + \mathbf{X'_{it}} \cdot \gamma + \eta_t + \delta_d + \varepsilon_{imdt}, \quad (2.5)$$

where $Quality_{it}$ is one of four standardized pre-Promise measures school quality for property i— primary school math Z-score, primary school reading Z-score, high school math Z-score, or high school reading Z-score. The resulting estimate of β tells us how the capitalization effect of the Promise varies across neighborhoods with access to different quality schools.

For each selected housing market definition, we restrict our attention to transactions occurring within three calendar years of the program announcement date, yielding seven calendar years of transactions for each housing market. We limit transactions to arms length sales or resales of owner-occupied, single-family units. Houses with missing transaction prices, transaction dates, and spatial coordinates are dropped, as were houses with a building age of less than -1. Then, as the coverage and reliability of data varies significantly across jurisdictions, we eliminate outlying observations on a market by market basis. This process typically removed observations with unreasonable (i.e. floor area of 0 square feet) or extreme covariate values (i.e. floor area more than 5,000 square feet, more than 11 bedrooms, more than 10 bathrooms, etc.), taking care that the observations removed constituted a small percentage of observations (1% or less). Finally, we eliminate transactions occurring at prices less than $1,000 or greater than $5,000,000

Table 2.2 presents the summary statistics for the sample of treated and untreated properties for each housing market definition.

Table 2.2: Housing Market Summary Statistics

		Large (10 mile)			Small (1 mile)			Optimal Subpop.		
		Promise	Control	t-stat	Promise	Control	t-stat	Promise	Control	t-stat
Transaction price	mean	220,026	219,754	-0.42	214,049	189,897	-21.55	216,271	189,513	-35.08
	(s.d.)	(190,684)	(143,416)		(190,952)	(161,839)		(191,359)	(136,338)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
Price (1990 dollars)	mean	131,961	134,164	5.68	126,971	114,554	-18.63	130,026	114,016	-35.46
	(s.d.)	(112,909)	(85,987)		(113,360)	(96,472)		(113,213)	(80,906)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
Building age	mean	48.38	26.12	-175.48	45.00	38.72	-31.16	51.65	37.95	-94.43
	(s.d.)	(36.85)	(26.81)		(32.80)	(30.11)		(35.27)	(29.24)	
	Obs.	94,955	401,715		54,867	43,088		77,059	174,262	
Floor area (sq. feet)	mean	1,595.62	1,820.44	87.41	1,573.41	1,598.54	5.56	1,540.07	1,578.04	13.00
	(s.d.)	(710.96)	(750.93)		(723.14)	(693.64)		(689.00)	(642.62)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
% Black	mean	0.14	0.11	-49.37	0.16	0.09	-65.87	0.12	0.15	33.34
	(s.d.)	(0.17)	(0.22)		(0.17)	(0.14)		(0.17)	(0.25)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% under 15	mean	0.20	0.24	128.24	0.21	0.20	-24.57	0.20	0.21	46.59
	(s.d.)	(0.07)	(0.06)		(0.07)	(0.07)		(0.06)	(0.05)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% over 60	mean	0.17	0.16	-29.86	0.16	0.21	65.77	0.19	0.19	3.50
	(s.d.)	(0.11)	(0.11)		(0.09)	(0.15)		(0.10)	(0.10)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% Households with children	mean	0.32	0.40	172.03	0.34	0.32	-22.03	0.31	0.34	75.77
	(s.d.)	(0.13)	(0.11)		(0.13)	(0.12)		(0.11)	(0.10)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% HS diploma	mean	0.40	0.34	-86.73	0.42	0.41	-3.33	0.40	0.43	29.88
	(s.d.)	(0.19)	(0.16)		(0.18)	(0.16)		(0.20)	(0.16)	
	Obs.	95,056	414,961		54,386	42,424		77,053	174,262	
% College	mean	0.34	0.34	4.47	0.32	0.29	-25.87	0.34	0.28	-71.54
	(s.d.)	(0.21)	(0.17)		(0.20)	(0.16)		(0.21)	(0.16)	
	Obs.	95,056	414,961		54,386	42,424		77,053	174,262	

Housing Market Summary Statistics, continued

		Large (10 mile)			Small (1 mile)			Optimal Subpop.		
		Promise	Control	t-stat	Promise	Control	t-stat	Promise	Control	t-stat
% unemployed	mean	0.08	0.06	-87.68	0.08	0.07	-23.53	0.08	0.08	6.73
	(s.d.)	(0.04)	(0.05)		(0.04)	(0.04)		(0.05)	(0.05)	
	Obs.	94,154	414,961		53,484	42,424		77,053	174,262	
% in poverty	mean	0.16	0.08	-215.38	0.15	0.11	-65.11	0.16	0.12	-89.99
	(s.d.)	(0.11)	(0.08)		(0.10)	(0.09)		(0.10)	(0.09)	
	Obs.	94,154	414,961		53,484	42,424		77,053	174,262	
% K-12 private	mean	0.18	0.13	-99.90	0.18	0.14	-51.44	0.18	0.14	-59.60
	(s.d.)	(0.17)	(0.09)		(0.16)	(0.11)		(0.16)	(0.12)	
	Obs.	94,513	413,695		54,381	41,751		76,515	173,715	
Median income	mean	51,491	68,328	207.35	52,493	54,967	16.01	50,615	53,512	34.07
	(s.d.)	(21,829)	(25,221)		(22,956)	(24,406)		(20,398)	(17,851)	
	Obs.	94,152	414,961		53,482	42,424		77,052	174,262	

Notes: Prices were deflated to January 1990 dollars using the "All Urban Consumers-Owner's Equivalent Rent of Primary Residence CPI" from the Bureau of Labor Statistics. T-statistic from a two-sided t-test with unequal variance.

As with public school data, our housing market data reveals that the neighborhoods receiving Promise programs are different from those outside of Promise zones along several dimensions. Using a large housing market definition, the housing stock in Promise zones covered by our housing data smaller in size and typically older than that in the outlying areas. The Promise zones represented in the housing sample— Denver, CO; Kalamazoo, MI; New Haven, CT; Pittsburgh, PA; Peoria, IL; Syracuse, NY; Hammond, IN; and Pinal County, AZ— are mostly urban areas. The exceptions are Hammond and Pinal County, both of which lie very close to urban areas (Chicago and Phoenix, respectively). As such, this could be an artifact of the availability of data through DataQuick, with rural areas being lower priority. This urban differential also reveals itself in the demographic characteristics; Promise neighborhoods typically contain more black residents, fewer children, and fewer college educated individuals. In addition, unemployment and poverty are more prevalent, leading to lower median incomes. Finally, Promise residents are more likely to enroll K-12 children in private schools. Many of these gaps are reduced or even reversed when considering our smaller geographic housing market or our propensity score screened optimal subpopulation, although differences remain significant. It is important to note that neither of the more selective samples dominates the other in terms of matching observables across groups. For example, the floor area of Promise properties matches more closely to the control properties in the small geographic market than in the optimal subpopulation, while the reverse is true for the percentage of black residents in the neighborhood. Due to the way the optimal subpopulation is constructed, the two groups in that sample should be matched closely on the covariates that are important for residential location decisions. In addition, the small geographic market definition yields fewer observations and estimates will be less precise as a result. We present results from both samples in what follows, but we believe the optimal subpopulation represents the best trade off between reducing bias from unbalanced observables and increasing the variance of the resulting estimates.

2.4 RESULTS

We first address the results from the K-12 enrollment data, which apply to a broad sample of Promise scholarship programs. We follow that with evidence of the impact of selected Promise scholarship programs on local housing markets. Finally, we present a more detailed housing market analysis for two large metropolitan Promise zones— Pittsburgh and Denver.

2.4.1 PUBLIC SCHOOL ENROLLMENT ANALYSIS

Figure 2.3 provides graphical evidence, both towards the validity of the parallel trends assumption and of the effect of the Promise on K-12 enrollment. We divide the baseline sample into geographic areas, each composed of one or two Promise zones and the surrounding counties. Within a geographic area, years were normalized such that the year that the relevant Promise was announced was set equal to zero.[20] We then regress log-transformed student counts on a full set of area-by-year fixed effects and plotted the yearly average residuals for treated schools and untreated schools along with a linear fit.

The graph depicts the variation in total student enrollment that is orthogonal to region-wide shocks in the years leading up to and immediately following the announcement of a Promise program. While there are substantial differences in levels between the groups, the trends in enrollment were not substantially different between groups prior to treatment. After the announcement of a Promise program, however, the control group continues on its pre-existing trend, while the Promise schools display a jump in enrollment as well as a sharp upturn in their enrollment trend. We attribute this convergence to increased demand for public schools following the announcement of a Promise program.

Table 2.3 displays the results of our fixed-effects estimates of school-level outcomes from

[20]If two Promise programs were announced in the same year and were located close enough that there was significant overlap in the adjacent counties, they were pooled into one area.

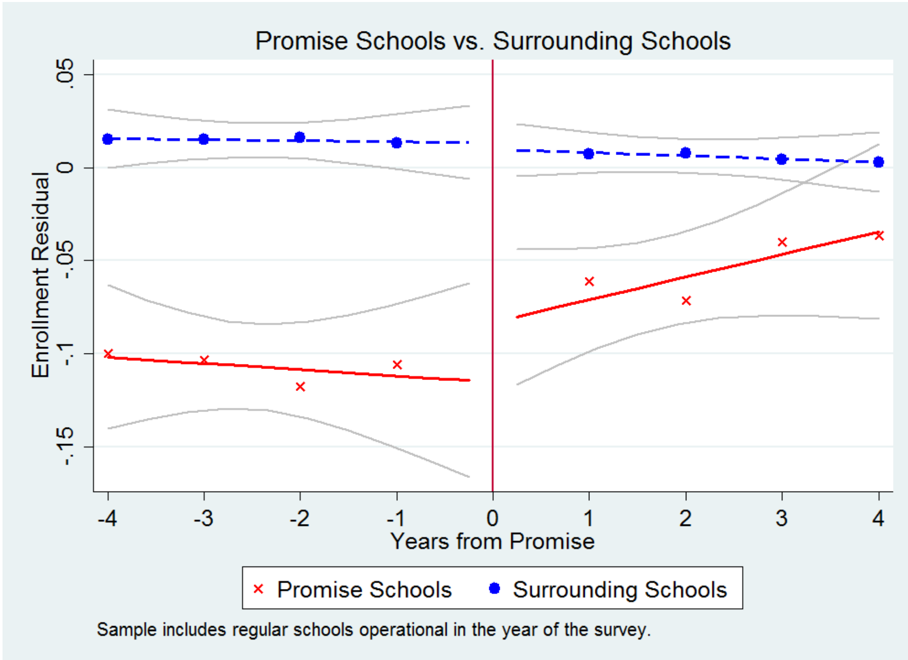

Figure 2.3: Total Enrollment Residuals by Year

equation 2.1 in Panel A and equation 2.2 in Panel B.

As predicted, when enrollment in a particular set of schools gains a student access to a potentially meaningful scholarship award, more students will enroll in those schools. The announcement of a Promise program leads to an increase in overall enrollment of roughly 4%. On average, increases in total enrollment are similar across racial groups, although the effects are not significant when decomposed in this way.

It is typical for Promise programs to scale up scholarship awards with the length of continuous enrollment at graduation. This feature makes the scholarship more valuable to students who begin their enrollment at early grade levels. Also, students who begin their enrollment spell past grade 9 or 10 are excluded from most Promise scholarships. As a

Table 2.3: K-12 Public School Enrollment Effects of Promise Programs

Dependent Variable:	log(Total)	log(White)	log(Non-white)
Panel A: Overall effects			
PromiseXPost	0.037***	0.023	0.021
	(0.007)	(0.016)	(0.012)
Panel B: Effects by type			
No Merit & Wide	0.080***	-0.010	0.001
(117 schools)	(0.023)	(0.042)	(0.038)
Merit & Wide	0.040**	0.110***	-0.039**
(203 schools)	(0.017)	(0.038)	(0.020)
No Merit & No Wide	0.039***	-0.020	0.076***
(327 schools)	(0.009)	(0.019)	(0.017)
Merit & No Wide	-0.031	0.054	-0.129***
(66 schools)	(0.026)	(0.033)	(0.033)
Observations	47,600	47,600	47,600
Clusters (Schools)	6,337	6,337	6,337
R-squared	0.97	0.98	0.98

Notes: Standard errors clustered at the school level in parentheses. Sample includes open, regular schools located in Promise zones and neighboring counties that reported student counts by race in all available surveys conducted within 4 years of the region-relevant Promise announcement. Fixed effects at the region-by-year, locale-by-year, and school level are included in all specifications. Controls include school level (primary, middle, high, other) and locale (city, suburb, town, rural).
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

result, we would expect much of the enrollment increases over the initial years of a Promise program to occur in the earlier grade levels especially in those programs that feature this sliding scale. Figure 2.4 depicts the treatment effect as estimated for each grade level separately.

The estimated increases in enrollment in Promise districts match this pattern almost precisely, with significant increases in enrollment at the lower grade levels (1-4), followed by no detectable changes through most of the higher grades (5-11), and finally decreases in enrollment in grade 12. Furthermore, this pattern is much more pronounced amongst those programs featuring a sliding scale relative to those which lack this feature. This match between the enrollment incentives provided by Promise scholarships and the estimated treatment effects gives us confidence that the identified overall effect is causal.

Turning our attention to the heterogeneity across program features, in panel B of Table 2.3 the effects of Promise programs are decomposed into those generated by programs of different classes. This exercise reveals that estimated overall effect is masking heterogeneity across programs. In addition, the variation is consistent with the expected effect of program features on the scholarship's prospective value. We would expect universal programs that allow use at a wide range of schools should present the most value to the widest range of households. Either imposing a merit requirement or restricting the list of schools should decrease the attractiveness of the program, although which restriction matters more is ambiguous. Finally, offering a merit-based scholarship usable only at a small list of schools should present the least value for the fewest households. Our estimates follow that profile exactly, with universal, wide-list programs generating the largest enrollment increases (8%) followed by merit-based, wide-list programs and universal, narrow-list progams (4%). Programs offering merit-based scholarships usable at a small list of schools seem to have no effect on overall enrollment.

There are also racial disparities in the response to these programs that vary by program feature as indicated by columns 2 and 3 in Panel B. In particular, programs featuring merit

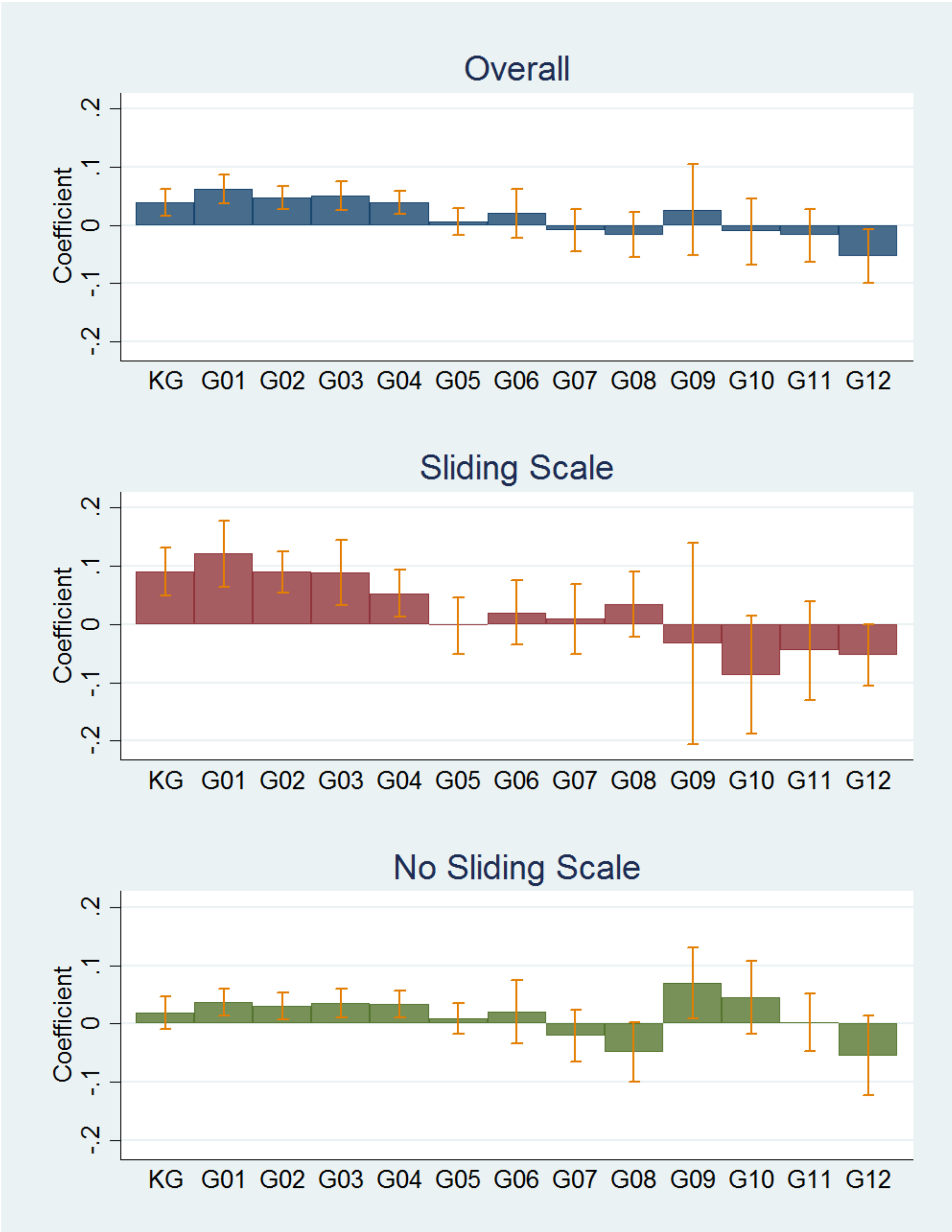

Figure 2.4: Treatment Effect by Grade Level

requirements prompt increases in white enrollment while leading to significant decreases in non-white enrollment. The racial pattern is likely explained by the existing racial achievement gap in U.S. public schools (Heckman and LaFontaine, 2010). As award receipt in these programs is conditioned explicitly on success in high school, the value for the average non-white student is diminished. Universal programs with large lists of eligible schools seem to have no effect on relative enrollment across racial groups, consistent with the analysis of the Kalamazoo Promise which belongs to this class. Finally, the small decrease in total enrollment in schools offering merit-based scholarships usable at a small list of schools is driven by a significant decrease in the enrollment of non-white students. Again, this conforms to our expectations regarding the incentives implied by different scholarship features and how they interact with racial groups.

Overall, offering a Promise scholarship tied to enrollment in a particular public school district is effective in drawing students into that school district, especially if graduates are able to use the scholarship at a wide range of institutions or there are no merit requirements for eligibility. However, Promise programs also have an important impact on the demographic composition of schools. Program administrators should note that scholarships with merit requirements will primarily attract white students and may lead to decreases in non-white enrollment, potentially contributing to racial inequality in educational attainment.

2.4.2 POOLED HOUSING MARKET ANALYSIS

Our enrollment estimates suggest that demand for public schools increases in areas where it is a pre-requisite for Promise scholarship receipt. As public school enrollment is tied to residential location, this would imply an increase in housing demand as well. If we assume that housing supply is fixed in the short run, any increase in housing demand must be capitalized into housing prices. In Figure 2.5, we repeat the graphical exercise conducted on the K-12 enrollment data, but using instead the housing market data and plotting

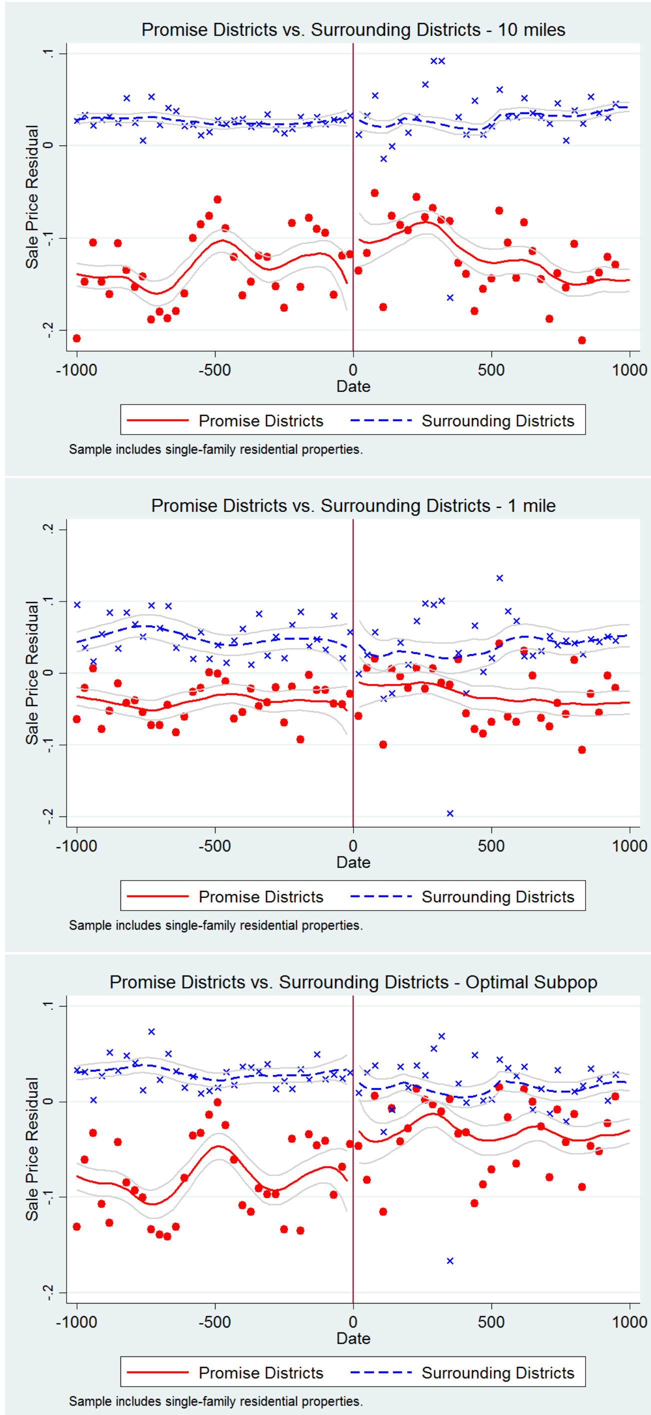

Figure 2.5: Sale Price Residuals by Date

separately for each market definition. Log housing prices for our eight Promise-related housing markets were regressed on a full set of market-by-year-by-quarter fixed effects and the monthly average residuals for treated properties and untreated properties are plotted along with a local linear fit on either side of the announcement date.

Clearly in the context of the large housing market definition, any impact of program announcement on housing prices in Promise areas is hard to detect. While the difference between groups narrows after the program announcement, the series diverge again to pre-Promise levels within about 2 years. As mentioned previously, however, this estimate is subject to significant bias due to the composition of the sample. The large market definition includes properties in the periphery who may not be affected by the Promise as well as properties in the center of the Promise zone that may not be considered by the marginal household when making their residential location decision. Inclusion of both groups biases the estimate of the effect towards zero.

When restricting attention to the smaller geographic housing market definition, the impact of the Promise is more noticeable, but qualitatively similar. There is a convergence between the series immediately after the program announcement, followed by slight divergence after about two years. It is hard to discern from the graph if there was or was not a lasting impact of the Promise announcement on housing prices in the sample. Using the optimal subpopulation yields a different story, however. After the announcement of the Promise, there is a noticeable and discrete increase in prices occurring in Promise zones which persists through the 2.5 years following the announcement.

Table 2.4 presents the results from our estimation of equation 2.3. Each panel corresponds to a different definition of a housing market. The specification in Column 1 includes only school district and market-specific time fixed effects. Of the difference in difference estimators, this specification is the most similar to the graphical analysis and is also subject to the most omitted variables bias, as it identifies the effect through temporal variation of prices at the school district level. Column 2 adds controls for various building and neigh-

Table 2.4: Capitalization Effects of Promise Programs: Log-linear

Dependent Variable: log(Price)	(1)	(2)	(3)
Panel A: Large (10 mile)			
PromiseXPost	-0.003	0.039***	0.083***
	(0.017)	(0.012)	(0.007)
Observations	514,394	487,930	505,604
Clusters	2,055	2,008	393,570
R-squared	0.38	0.69	0.92
Panel B: Small (1 mile)			
PromiseXPost	-0.006	0.045***	0.066***
	(0.022)	(0.017)	(0.013)
Observations	99,212	93,711	94,925
Clusters	607	595	72,656
R-squared	0.41	0.72	0.93
Panel C: Optimal subpopulation			
PromiseXPost	0.032*	0.061***	0.123***
	(0.018)	(0.013)	(0.009)
Observations	251,321	250,229	250,229
Clusters	1,465	1,461	196,877
R-squared	0.38	0.67	0.92
Building Controls	NO	YES	NO
Census Controls	NO	YES	YES
Market-Year-Qtr FE	YES	YES	YES
School District FE	YES	NO	NO
Neighborhood (Tract) FE	NO	YES	NO
Property FE	NO	NO	YES

Notes: Standard errors clustered at the school level (in columns 1 and 2) or the property level (column 3) in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. All controls are interacted with housing market indicators. Building controls in column 2 include square footage and a quadratic in building age. Census controls include the following tract-level statistics interpolated from the 1990, 2000, and 2010 Census full-count data as well as the 2006-2010 American Community Survey: % of pop. black, % of pop. under 15/over 60, % of households with children under 18, % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.075,.925].

* Significant at the 10% level , ** Significant at the 5% level, *** Significant at the 1% level

Table 2.5: Capitalization Effects of Promise Programs: Linear

Dependent Variable: Price ($1990)	(1)	(2)	(3)
Panel A: Large (10 mile)			
PromiseXPost	445.5	7,335***	17,966***
	(2,244)	(1,678)	(1,029)
Observations	514,394	487,930	505,604
Clusters	2,055	2,008	393,570
R-squared	0.25	0.72	0.94
Panel B: Small (1 mile)			
PromiseXPost	-2,451	5,504***	14,244***
	(2,904)	(1,732)	(1,748)
Observations	99,212	93,711	94,925
Clusters	607	595	72,656
R-squared	0.24	0.75	0.94
Panel C: Optimal subpopulation			
PromiseXPost	3,018	8,214***	20,440***
	(2,308)	(1,595)	(1,110)
Observations	251,321	250,229	250,229
Clusters	1,465	1,461	196,877
R-squared	0.27	0.71	0.95
Building Controls	NO	YES	NO
Census Controls	NO	YES	YES
Market-Year-Qtr FE	YES	YES	YES
School District FE	YES	NO	NO
Neighborhood (Tract) FE	NO	YES	NO
Property FE	NO	NO	YES

Notes: Standard errors clustered at the school level (in columns 1 and 2) or the property level (column 3) in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. All controls are interacted with housing market indicators. Building controls in column 2 include square footage and a quadratic in building age. Census controls include the following tract-level statistics interpolated from the 1990, 2000, and 2010 Census full-count data as well as the 2006-2010 American Community Survey: % of pop. black, % of pop. under 15/over 60, % of households with children under 18, % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.075,.925].

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

borhood characteristics of the property and exchanges school district fixed effects for the more spatially explicit Census tract fixed effects. Finally, column 3 includes property fixed effects, identifying the impact of the program from repeat sales of identical properties in Promise zones vs. outside. These same estimates are repeated in Table 2.5 using price in constant 1990 dollars as the dependent variable.

The simplest DD specification yields inconsistent and imprecise capitalization estimates. This may indicate why previous studies using such a specification, but lacking access to rich real estate data across several programs have been unable to uncover a significant treatment effect. After controlling for property covariates and neighborhood fixed effects, the magnitude of estimates increases and the variance decreases across all samples, suggesting capitalization effects on the order of 4% to 6% of home values, or between $5,500 and $8,000. Our preferred specifications use either the small geographic housing market or propensity score screened optimal subpopulation and include property level fixed effects, identifying the effect from repeat sales. These specifications provide very precise treatment effects of between 6% and 12% of home values or $14,000 and $20,500.

Our analysis of public school enrollment suggested that Promise programs have different impacts on different populations, particularly on different racial groups. As such, we would like to document any such heterogeneity in the housing market as well. Our housing market data provides no information on the characteristics of the individuals participating in the transactions. However, we do observe the transaction price of the house, which should be correlated with income and, as a result, race.

To investigate the heterogeneity of the capitalization of Promise scholarships with respect to income, we divide each housing market in half according to the distribution of housing values implied by the pre-announcement hedonic price function. As described in the previous section, we estimate the hedonic price function over the pre-Promise period in each housing market, recover the coefficient estimates, and then use them to predict the sale price of *all* transactions observed in the sample as if each had occurred in the first

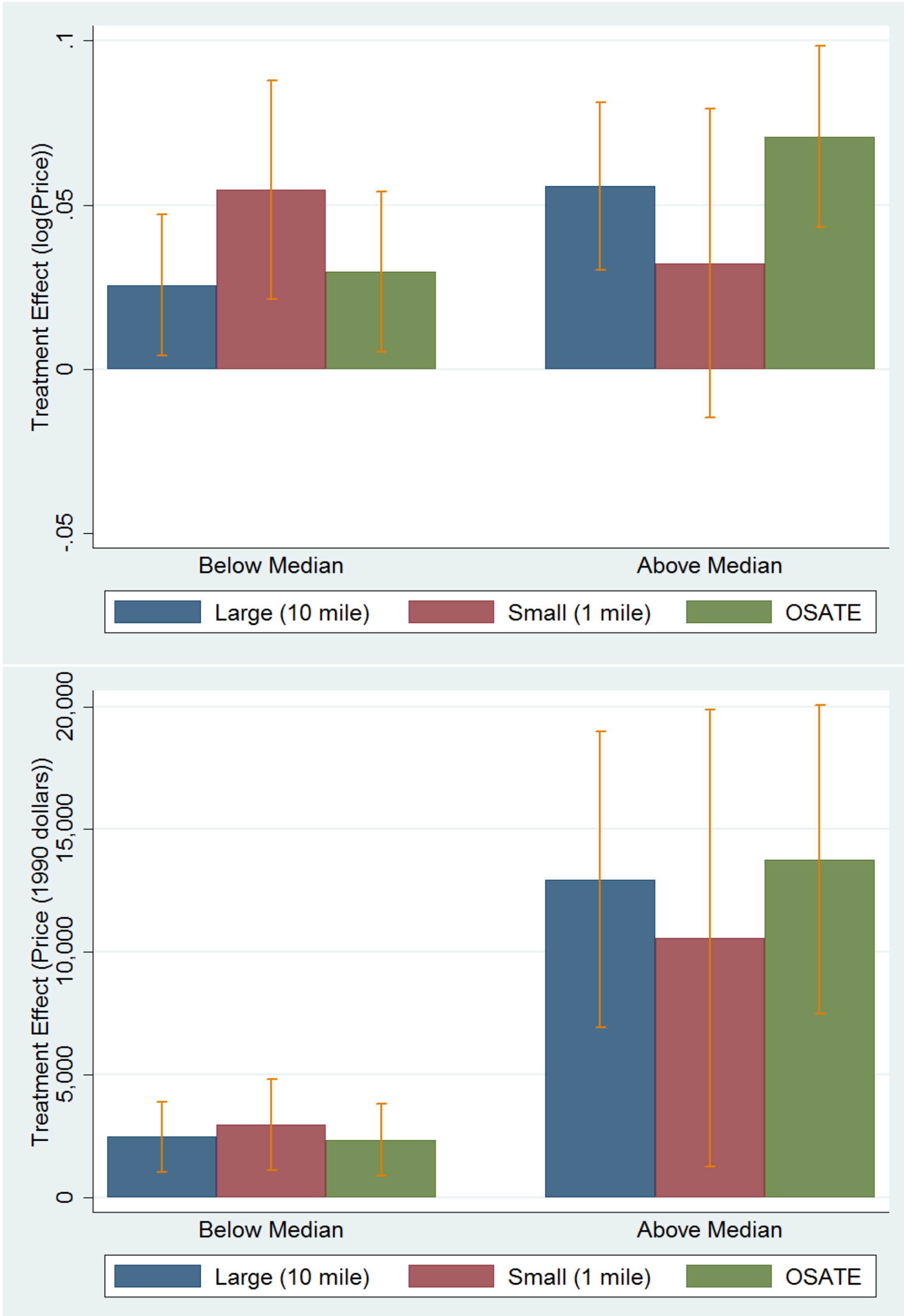

Figure 2.6: Treatment Effect by Above/Below Median

quarter of the year prior to the relevant Promise announcement. We then repeat the DD analysis above, but separately for the samples of properties above the median and below the median of the distribution generated by the first step. We report the estimates from the tract-level fixed effects specification (equivalent to column 2 in Table 2.4) only. The results are depicted in Figure 2.6. Across estimation samples, the capitalization of Promise programs into housing prices increases across the housing price distribution. Capitalization effects in the 1st quintile range from 2.8% to 5.5% compared to capitalization in the top quintile of between 6.8% and 8.9%.

There are several reasons why higher income households may be willing to pay more to gain access to Promise scholarship programs. As mentioned in the previous section, students from higher income households are more likely to attend college and the value of access to Promise scholarships is ultimately conditional on college attendance. Even conditional on college attendance and the quality of the institution, most Promise scholarships only apply to unmet need, which should be greater for high income households due to a larger expected family contribution. As it is reasonable to expect these higher income families to occupy higher priced domiciles, the results from our regressions provide more evidence in support of the claim that higher income households are willing to pay more for access to Promise scholarship programs.

2.4.3 LARGE URBAN HOUSING MARKET ANALYSIS

The pattern of capitalization across the housing distribution suggests that higher-income households place more value on access to Promise scholarships. As a result, one might also expect there to be a similar pattern of capitalization across the distribution of school quality. In order to verify such a pattern, we must link properties to school-level data on performance, such as state standardized test scores. Unfortunately, neither school attendance boundaries nor standardized test performance data is readily available for all of the Promise zones included in our housing market analysis.

For the two Promise programs in our housing market data based in large metropolitan areas— the Pittsburgh Promise and the Denver Scholarship Foundation— we obtained school attendance boundary maps through SABINS. In addition, we acquired school-level data on standardized test scores from the Pennsylvania and Colorado state education agencies. This data allows us to link properties in our housing market data to objective measures of pre-Promise school quality. Before presenting those results, however, we verify that the results from the pooled housing market sample also hold in both Pittsburgh and Denver. Table 2.6 reports estimates of the treatment effect within each market, identifying from repeat-sales as in column 3 of Table 2.4.

Both programs display large treatment effects across all samples, ranging from 15% to 22% in the Pittsburgh market and 5% to 11% in the Denver market. Estimates from specifications using price in constant dollars as the dependent variable are provided for comparison purposes; the implied capitalization amounts are roughly in line with the magnitude of award amounts.

Our final set of results attempts to correlate the capitalization effects of these Promise programs with the quality of schools. Our hypothesis is that capitalization will be concentrated in neighborhoods with higher quality schools. This is because the higher income households on the margin will likely be choosing between higher quality suburban neighborhoods (and no access to Promise aid) and lower quality urban schools (with access to Promise aid). As such, the households that relocate will aim first to minimize the associated loss in school quality.

In order to quantify school quality, we first calculated the percentage of students in each Pennsylvania or Colorado public school that scored "proficient" or better in math and reading standardized tests in 2005, prior to the announcement of either program. Then, we standardize this measure of quality such that within each state by school level cell the distribution has a zero mean and unit standard deviation. Tables 2.7 and 2.8 contain the results from estimating equation 2.5 using high school and primary school

Table 2.6: Capitalization Effects in Large Metropolitan Promise Programs

	Pittsburgh		Denver	
	log(Price)	Price ($1990)	log(Price)	Price ($1990)
Panel A: Large (10 mile)				
PromiseXPost	0.218***	13,508***	0.105***	24,784***
	(0.046)	(2,619)	(0.006)	(1,326)
Observations	52,716	52,716	221,198	221,198
Clusters	46,573	46,573	160,455	160,455
R-squared	0.91	0.95	0.89	0.94
Panel B: Small (1 mile)				
PromiseXPost	0.147**	8,701***	0.069***	17,841***
	(0.075)	(2,763)	(0.013)	(2,363)
Observations	14,474	14,474	49,445	49,445
Clusters	12,762	12,762	34,241	34,241
R-squared	0.90	0.96	0.9	0.93
Panel C: Optimal subpopulation				
PromiseXPost	0.155**	8,096***	0.046***	4,783***
	(0.077)	(2,424)	(0.012)	(1,273)
Observations	13,517	13,517	36,104	36,104
Clusters	11,903	11,903	24,748	24,748
R-squared	0.91	0.97	0.87	0.94
Census Controls	YES	YES	YES	YES
Market-Year-Qtr FE	YES	YES	YES	YES
Property FE	YES	YES	YES	YES

Notes: Standard errors clustered at the property level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. Census controls include the following tract-level statistics interpolated from the 1990, 2000 and 2010 Census full-count data: % of pop. black, % of pop. under 15/over 60, % of households with children under 18. In addition, the following block tract-level statistics are interpolated between the 1990 and 2000 Census sample files and the 2006-2010 American Community Survey: % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Full count statistics interpolated between 1990-2010 with years after 2010 held constant at 2010 values. Sample statistics interpolated between 1990-2006 with years after 2006 held constant at 2006 values. Optimal subpopulation includes sales with propensity scores in the interval [.091,.909] for Pittsburgh and [.076,.924] for Denver. * Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

quality, respectively.

With the exception of the measure of high school quality in the large housing market definition, all of our school quality metrics are associated with larger capitalization effects of Promise program announcement. Across Pittsburgh and Denver, a one standard deviation increase in the quality of the neighborhood high school leads to an increase in the capitalization effect of the Promise of between 1% and 5% (or $2,500 and $6,000). Estimates using primary school quality are uniformly larger; a one standard deviation increase in the quality of the neighborhood primary school leads to an increase in the capitalization effect of the Promise of between 5% and 10% (or $8,800 and $16,000). We expect that the magnitude of the primary school quality effect relative to the high school quality effect is due to a combination of factors. First, the incentives provided by many Promise programs (including the Pittsburgh Promise) are strongest for primary school students as the scholarship amount scales with years of continuous enrollment. As a result, primary school quality should be focal for the households most likely to be influenced by the program. Also, due to the presence of school choice programs in Pittsburgh and Denver, residential location is not always the sole determinant of school quality and the strength of this link varies across grade levels. In Pittsburgh in 2010, 62% of the public elementary school students attended their neighborhood school compared to only 52% of public high school students. The situation in Denver is similar; in 2013, 57% of K-5 public school students attended their neighborhood school compared to 39% of public high school students (9-12). As a result, the quality of the neighborhood high school may be less relevant to the residential location decision than the quality of the neighborhood primary school for which fewer feasible alternatives exist.

Table 2.7: Large Metropolitan Promise Programs by High School Quality

	High School Quality				
	Math			Reading	
	log(Price)	$1990		log(Price)	$1990
Panel A: Large (10 mile)					
Promise x Post x	-0.011*	-5,397***		0.004	-3,081***
Quality	(0.006)	(716.8)		(0.004)	(514.1)
N (Clusters)			195,412 (144,002)		
R-squared	0.91	0.95		0.91	0.95
Panel B: Small (1 mile)					
Promise x Post x	0.027**	4,535***		0.046***	5,859***
Quality	(0.011)	(1,420)		(0.008)	(1,002)
N (Clusters)			52,925 (37,750)		
R-squared	0.93	0.94		0.93	0.94
Panel C: Optimal subpopulation					
Promise x Post x	0.014*	2,579***		0.023***	2,967***
Quality	(0.008)	(919.6)		(0.006)	(642.6)
N (Clusters)			67,663 (47,838)		
R-squared	0.92	0.94		0.92	0.94
Census Controls	YES	YES		YES	YES
City-Year-Qtr	YES	YES		YES	YES
Property FE	YES	YES		YES	YES

Notes: Standard errors clustered at the property level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. School quality in 2005 is measured as the percentage of students that score proficient/advanced on state standardized tests standardized within state-school level cells. All controls are interacted with housing market indicators. Census controls include the following tract-level statistics interpolated from the 1990, 2000 and 2010 Census full-count data: % of pop. black, % of pop. under 15/over 60, % of households with children under 18. In addition, the following block tract-level statistics are interpolated between the 1990 and 2000 Census sample files and the 2006-2010 American Community Survey: % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Full count statistics interpolated between 1990-2010 with years after 2010 held constant at 2010 values. Sample statistics interpolated between 1990-2006 with years after 2006 held constant at 2006 values. Optimal subpopulation includes sales with propensity scores in the interval [.078,.922].
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

Table 2.8: Large Metropolitan Promise Programs by Primary School Quality

	Primary School Quality				
	Math			Reading	
	log(Price)	$1990		log(Price)	$1990
Panel A: Large (10 mile)					
Promise x Post x	0.084***	14,083***		0.068***	10,798***
Quality	(0.005)	(978.6)		(0.005)	-812.6
N (Clusters)			179,567 (131,872)		
R-squared	0.91	0.95		0.91	0.95
Panel B: Small (1 mile)					
Promise x Post x	0.092***	15,867***		0.080***	14,172***
Quality	(0.008)	(1,696)		(0.007)	(1,528)
N (Clusters)			49,749 (35,495)		
R-squared	0.93	0.94		0.93	0.94
Panel C: Optimal subpopulation					
Promise x Post x	0.060***	9,960***		0.052***	8,885***
Quality	(0.007)	(1,306)		(0.006)	(1,110)
N (Clusters)			61,787 (43,451)		
R-squared	0.91	0.95		0.91	0.95
Census Controls	YES	YES		YES	YES
City-Year-Qtr	YES	YES		YES	YES
Property FE	YES	YES		YES	YES

Notes: Standard errors clustered at the property level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. School quality in 2005 is measured as the percentage of students that score proficient/advanced on state standardized tests standardized within state-school level cells. All controls are interacted with housing market indicators. Census controls include the following tract-level statistics interpolated from the 1990, 2000 and 2010 Census full-count data: % of pop. black, % of pop. under 15/over 60, % of households with children under 18. In addition, the following block tract-level statistics are interpolated between the 1990 and 2000 Census sample files and the 2006-2010 American Community Survey: % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Full count statistics interpolated between 1990-2010 with years after 2010 held constant at 2010 values. Sample statistics interpolated between 1990-2006 with years after 2006 held constant at 2006 values. Optimal subpopulation includes sales with propensity scores in the interval [.078,.922].

* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level

Furthermore, in results not presented here we estimated the capitalization effects by individual high school neighborhoods and found that after the announcement of Promise programs in Pittsburgh and Denver, housing prices increased in the neighborhoods associated with top performing high schools in the district (top 3 in Pittsburgh, top 4 in Denver). In addition to these high-performing schools, large capitalization effects are also estimated for the neighborhood associated with the school that ranked at the bottom of each city's high schools— Peabody High School in Pittsburgh and North High School in Denver. Neighborhood level data, however, shows that school attendance rates of resident public school students are among the lowest in each district for these lower-quality schools. On this measure, Peabody ranked 45 out of 48 traditional schools in Pittsburgh in 2010 and North ranked 97 out of 103 traditional schools in Denver in 2013. As a result, some high-income households seem to have located in these Promise-eligible neighborhoods associated with poor quality schools, while utilizing the school choice systems in Pittsburgh and Denver to send children to high quality public secondary schools.[21]

2.5 CONCLUSION

Place-based "Promise" scholarship programs have proliferated in recent years. Typically implemented at the school district level and financed privately, they guarantee financial aid to eligible high school graduates from a particular school district, provided they have continuously resided in the district for a number of years. In this study, we measure the impact of a cross-section of Promise scholarships on a range of policy-relevant outcomes, including public school enrollment and housing prices. In addition, we provide the first direct evidence of how enrollment effects vary with features, such as eligibility requirements and scholarship flexibility.

[21] All data on neighborhood school attendance rates was provided by Pittsburgh Public Schools and Denver Public Schools.

Using a difference-in-differences approach, we conclude that the initiation of a Promise program leads to an increase in public school enrollment in affected schools and an increase in housing prices of between 6% and 12%, with capitalization effects most dramatic amongst Promise zone properties in the upper half of the house price distribution. Even so, there is substantial variation in these effects according to the features of the programs. Scholarships that are usable at a wide range of institutions are effective at increasing total public school enrollment, although this is mitigated by the imposition of merit requirements. However, the effects on school composition vary, with merit requirements providing strong incentives for white enrollment at the expense of non-white enrollment. Furthermore, focusing on Pittsburgh and Denver specifically, the capitalization effect of scholarship programs into housing prices increases with the quality of the neighborhood public school. Taken together, this evidence suggests that these scholarships have important distributional effects that bear further examination.

These results provide strong guidance to future program designers. First and foremost, place-based scholarship programs are capable of having an impact on important regional development outcomes, such as population, school enrollment, and property values. Making the scholarship usable at a wide range of schools is essential in attracting households to the scholarship area. Unfortunately, since minority students are less likely to satisfy them, adding merit requirements could increase educational inequality. Further contributing to inequality, we find that the increase in housing demand resulting from the announcement of the Promise is most pronounced in high-priced neighborhoods with high-quality schools. As a result, the potential for peer effects to play a role in the mitigation of inequality is greatly reduced as the high-quality students attracted by the Promise seem to be settling into already high quality schools.

Still, these same capitalization effects are evidence that high-income households are paying a premium for housing in the wake of a Promise scholarship program, while low-income households do not face the same increase in housing costs to the extent that they own instead of rent. As such, while low-income students will likely utilize these scholarships less often than high-income students, they may benefit more net of this house price effect, although a complementary analysis of rental rates would be necessary to confirm this intuition. In addition, the increase in the tax base that may result from the increase in home values leaves open the potential for more disadvantaged students to benefit. If high-income households are contributing more to Promise school districts in the form of property taxes, low-income students stand to benefit through that channel as well. As a result, the impact of Promise scholarships on educational equity remains somewhat ambiguous and is an area for future research.

There are many other avenues for future research into Promise scholarship programs. Broader real estate transactions data would allow for an extension of the housing market analysis conducted here to the remaining Promise programs. Such research would be important in generalizing the house price effects of Promise programs beyond our sample of eight programs, which offer little variation in program features. We also hope to increase the scope of our evaluation to a wider range of outcomes. Any impact of Promise scholarships on school quality and test scores is important in answering questions related to the effect on educational inequality. Retaining high-income families has the potential to substantially change the composition and performance of urban schools, leading to spillover effects for low-income students.

Extending the analysis to the postsecondary education market would also be fruitful. Some individual Promise programs have studied their effects on college choice and attendance with success. However, typically such studies are conducted through arrangements with school districts, which often have student level records of college applications and enrollments. As a result, data availability is a concern. The same is true for the impact of Promise scholarships on cost of attendance. Recent studies have shown that if students are likely to receive aid from other sources and their chosen college or university can easily quantify the amount of aid, the institution will increase its effective price by reducing the amount of institutional aid provided (Turner, 2012b,a). Knowing that a student comes from a Promise district is a fairly strong signal to a post-secondary institution that the student may be receiving Promise aid. As a result, some of the value of the scholarship may well be captured in the market for post-secondary education. In addition, if the signal is str onger for high-income students than low-income students, perhaps due to uncertainty surrounding additional merit requirements, or demand is more elastic among low-income students, documenting such an effect would have distributional implications as well.

3.0 SPONSORING PUBLIC GOODS LOTTERIES: THEORY AND EVIDENCE

3.1 INTRODUCTION

It has been shown recently that funding public goods through lotteries can secure higher levels of provision and greater social welfare than voluntary contribution mechanisms (Morgan, 2000; Morgan and Sefton, 2000). The driving force behind the theoretical efficacy of competitive mechanisms like lotteries is the offsetting of externalities. While voluntary contributions to a public good are characterized by positive externalities and hence are underprovided, betting in lotteries and bidding in all-pay auctions imposes a negative externality on the other participants, decreasing their likelihood of winning. As a result, combining these two sources of externalities theoretically moves the equilibrium allocation closer (and under certain conditions, arbitrarily close) to the efficient outcome. Many have taken these mechanisms to the lab to muster experimental evidence, where contributions are generally higher than under voluntary mechanisms but welfare improvement is not always achieved (see Morgan and Sefton (2000), Landry et al. (2006), Lange et al. (2007), Orzen (2008), Schram and Onderstal (2009), and Corazzini et al. (2010) for experimental evidence).

In voluntary contribution games, higher gross contributions would necessarily lead to greater public good provision and welfare gains. Under a lottery mechanism, however, public good provision is equal to contributions *net* of the prize amount. Even with higher contributions, production of the public good can be lower in a lottery setting after accounting for the prize. In some extreme cases, the cost of the prize is not covered by the contributions to the lottery, leading to a deficit. In most experiments to date, the lottery would continue even in the face of such a deficit, resulting in negative provision of the public good. Recent experimental work has made the fixed prize provisional on raising sufficient funds, in line with the original theoretical model (Duffy and Matros, 2012), but this practice may be prohibitively costly for actual fundraisers. Instead, prize donations are often secured from businesses before using this strategy, thus ensuring a surplus. Lange (2006) extends the lottery model to include a first stage decision where contributors donate prizes to a second stage lottery, deriving the conditions under which this process can mitigate free-riding incentives. Where the literature is lacking, however, is in its failure to incorporate incentives for firms to provide these prizes.

Charity organizations frequently receive donations of gift certificates and merchandise from local businesses that are subsequently auctioned or raffled off with the proceeds benefitting the charity. The 2008 "Report on the State of Corporate Community Investment" indicates that 77% of businesses surveyed made in-kind donations to local non-profit organizations. The average reported value of these donations was $121,899 over the year. While this is an upper bound on the degree to which firms donate to lotteries in particular, it is suggestive that firms actively participate in these sorts of charitable endeavors. Corporate interest in the public good itself could explain this behavior as in Lange (2006), but further investigation reveals that other incentives may be more salient. According to the survey, 51% of firms indicate that "Reputation and Loyal Customer Base" is a major reason for community investment with an additional 29% indicating that it was either a major or minor goal. In addition, a large percentage (37.2%) of corporations report that the marketing department is responsible for community investment decisions, the highest percentage associated with a single department .

The fundraising community clearly recognizes its importance as an advertising channel as well. A review of several articles on the website eHow.com written by those responsible for securing these donations reveals the belief that advertising is of utmost importance to firms. One such article, straightforwardly titled *How to Ask a Business to Donate Merchandise for a Raffle* recommends "[presenting] your fundraising raffle to businesses as an inexpensive marketing and public relations opportunity," and demonstrating the advertising value by "[listing] all the different ways you plan to promote the raffle event and all the business [sic] that donate."[1] The Junior League of Charlotte, NC (JLC), a non-profit devoted to improving the outcomes of women and children, is a prime example of an organization that uses such a strategy. The JLC holds an annual fundraising raffle where merchandise is provided by local businesses. Their website devotes an entire page to a detailed description of the promotional benefits of sponsorship. Several tiers of sponsorship (delineated by the value of cash or in-kind donations) are associated with varying intensities of advertising, including recognition in organizational publications, mention in event press releases, and prominent display of promotional materials at the event itself.[2]

Clearly, advertising plays a large role in corporate contribution behavior. If this is the case, then exclusive arrangements with non-profit organizations may be desirable for businesses contemplating donations to a fundraising raffle. Non-exclusivity would allow for major competitors to advertise as well, diluting the incentive to donate. If the fundraiser could commit *ex ante* to only accepting the most generous prize donation from amongst a particular group of competing firms, perhaps each firm's incentives to outbid their competitors would be increased enough to compensate for the lack of multiple sponsors. Yet the same articles that stress advertising as the primary incentive for potential donors never mention promises of exclusivity, even within an industry. Some explicitly advocate sending letters to a large list of firms and accepting donations from whoever is willing to give. A review of the promoted sponsors from the Junior League of Charlotte's "March Money Madness" provides more anecdotal evidence; multiple radio stations, caterers, and bou-

[1]See Appendix B for a list of relevant quotes and URLs.
[2]See `http://www.jlcharlotte.org/?nd=springfundraiser` for details.

tiques are represented amongst the raffle sponsors. It is not clear that this approach is optimal in a world where businesses are motivated by advertising.

The goals of this paper are two-fold. First, I will extend the current model of fixed-prize public goods lotteries to include a first-stage in which firms have an incentive to donate prizes for advertising purposes. Second, using this model of sponsored fixed prize lotteries I will explore both theoretically and empirically through a laboratory experiment the revenue implications of several different fundraising strategies available to organizers, varying the exclusivity of the sponsorship arrangement and the allocation of advertising resources. The key finding from the model is that the optimal fundraising strategy is sensitive to the effectiveness of the fundraiser's advertising. When advertising resources are limited or consumers place little value on the fundraiser's endorsement, the optimal solicitation strategy for the fundraiser is non-exclusive, accepting donations from multiple competing firms and allocating advertising benefits according to their relative generosity. However, if the fundraiser has substantial advertising resources at her disposal or the consumers are particularly responsive to the fundraiser's endorsement, the optimal solicitation strategy is exclusive, accepting a donation from and allocating all advertising resources to only the most generous donor. I then replicate the environment in a laboratory setting to test some of the predictions regarding different fundraising strategies.

The results of the experiment confirm some, but not all of the conclusions from the comparative statics of the model. In particular, while theoretical predictions suggest that exclusivity can be beneficial to the fundraiser, the experimental evidence contradicts this claim. Under all parameterizations, non-exclusive arrangements generate larger prizes and therefore greater contributions to the fundraiser. Non-exclusive arrangements that allocate advertising proportionally to corporate sponsors in accordance with their donations are particularly effective, dominating arrangements that only allocate advertising to the more generous donor.

The rest of the paper is organized as follows. In section 3.2, I present a model that incorporates simple advertising incentives into the firm's decision to donate merchandise to a lottery which also raises funds for a public good. With this model in mind, I then consider three possible solicitation and advertising mechanisms for the fundraiser when asking two competing firms for donations: either the fundraiser only accepts the more generous prize donation and advertises accordingly; the fundraiser accepts both prize offers, but rewards the more generous firm with disproportionate advertising; or the fundraiser accepts both prizes and provides advertising in proportion to the generosity of each firm. In section 3.3, I explain the experimental design that is used to replicate these environments in the laboratory. Section 3.4 discusses the results from the experimental data. Finally, section 3.5 concludes.

3.2 MODEL

The firm-sponsored fixed-prize lottery is modeled as a sequential game. In the first stage, two firms choose how many units of output to donate to a prize pool for a subsequent lottery, the revenues from which will fund the provision of a public good. The fundraiser then observes all of the offers and selects from amongst the prize offers to generate the prize that will be available to consumers. She also influences the benefits of sponsorship to the firms in accordance with how she allocates advertising resources between the firms. In the second stage, consumers observe the size of the fixed prize and the advertising generated by the fundraiser. They are then asked to allocate their endowments between buying stakes in the lottery and purchasing private goods from the two firms, under the assumption that some consumers' choices between private good vendors are affected by the advertising of the fundraiser. I focus on symmetric subgame perfect equilibria, first considering the decisions of the consumers.

3.2.1 THE CONSUMERS

There are N identical consumers who have quasi-linear preferences over the private good supplied by two firms as well as the public good. I write their utility function as

$$U_i(x_i, G) = x_i + x_w(g_i, G, B) + u(G) \tag{3.1}$$

where $u(G)$ is their utility from consumption of the public good G, g_i is the individual's contribution to the public good, x_i is the amount in units of total private good purchased, and x_w is the amount in units of the private good won from the lottery. Let $u(G)$ be concave and increasing in G. I will assume a standard lottery contest success function, such that x_w will take the value B (the amount of the fixed prize) with probability g_i/G and 0 otherwise.[3] Each consumer has an endowment w to allocate between purchases of the public good and the private good, with the price of the public good p_G normalized to 1. As a result, a consumer's decision must satisfy the following constraint, assuming a uniform price p for the private good

$$px_i + g \leq w \tag{3.2}$$

Ultimately, two advertising institutions will be considered which impact how I model consumer behavior. In the first, regardless of how the fundraiser chooses to accept (or reject) prize offers from firm donors, the firm making the larger prize donation is named the "sponsor" of the lottery. As a result, the fundraiser devotes the all of her advertising resources to the sponsor, regardless of the difference between the two firms' offers. Under this institution, a fixed proportion λ of private good purchases will be made from the sponsor and the remainder will be made from the other firm (the non-sponsor), given that prices are identical. Environments using this institution are designated "fixed advertising"

[3] I consider only the case of risk-neutral consumers. It has been shown that allowing for risk aversion has an important impact on equilibrium behavior when there are multiple prizes offered (Lange et al., 2007). Maintaining risk neutrality allows me to consider only the case where one large prize is offered, simplifying the decision of the fundraiser but leaving open the question of optimal division of total prize pools in the face of risk averse lottery participants.

environments. Under the second institution, which can only be utilized when both firms' offers are accepted, the fundraiser divides advertising resources in proportion to the generosity of each firm's prize offer. In this scenario, the proportion of private good purchases made from each firm will be given by the relative generosity of the donations. Specifically, given identical prices, firm j will sell $b_j/(b_{-j} + b_j)$ of the total units of private good sold, where b_j is the total amount donated by firm j. Environments using this institution are designated as "proportional advertising" environments.

I make the following assumptions to simplify the analysis. First, I will assume that $\lambda > \frac{1}{2}$ in fixed advertising scenarios such that, given equal prices, more of the sponsor's good is purchased by lottery consumers. In other words, the advertising provided by the fundraiser is beneficial, although not to the extent of providing monopoly rents. While the advertising will be persuasive to some consumers, it seems unreasonable to expect that all consumers, some of whom may have strong brand loyalties or other idiosyncratic attachments to a firm's output, would be persuaded in their consumption decisions by the advertising of the fundraiser. Many factors influence the magnitude of this benefit. The prevalence of preferences for social responsibility in the market of lottery consumers would cause λ to be greater. If the lottery is a fundraiser for a charity or social cause, it is only natural to assume that such preferences would be fairly common. In addition, there is evidence to indicate that consumers do attach additional utility to purchasing from socially responsible firms (Besley and Ghatak, 2007). In addition, the more weight consumers lend to the fundraiser's endorsement of the sponsor the higher will be λ. If a fundraiser's endorsement carries significant weight with the participants, a simple recommendation of one firm over another may be enough to convince a substantial portion of consumers to change their purchasing behavior. Increased fundraising resources would lead to a higher λ, as well. If we assume that the persuasiveness of the advertising is a function of resources devoted to advertising, then more resources would lead to more consumers purchasing from the sponsoring firm. Finally, as with more traditional forms of advertising, higher search costs among consumers would also increase λ. Consider two local businesses one may find in the phone book. For a consumer with high search costs, having the firm's name and contact information emblazoned on a prize in a lottery that the consumer is browsing reduces costly search time when the need arises.

Second, there is a large segment of private good consumers who *do not* participate in the lottery and are not subject to the advertising therein. Both firms sell their output on this market at a price $p > 1$ where 1 is the constant marginal cost of production for both firms. Third, firms can not distinguish lottery participants from other consumers and therefore can not price discriminate in the market for the private good. Finally, suppose that $X_{out}(p)$ is the total demand function for consumers who do not participate in the lottery. As a result, the change in profits from these consumers from a change in price is written $\partial\Pi/\partial p = (p-1)X'_{out}(p) + X_{out}(p)$. I will only consider the situation in which this change in profits satisfies $(p-1)X'_{out}(p) + X_{out}(p) < (p-1)(Nw/p)$. In short, both firms are realizing profits from consumers outside of the lottery and lowering prices in this segment reduces profits by more than can be made from capturing the entire lottery segment. These assumptions together rule out strategic pricing on the part of the firms in the analysis such that firms compete only in the space of prize offers and only for the participants in the lottery. This assumption is obviously an oversimplification. For many fundraising lotteries, however, the segment of consumers subject to the advertising of the fundraiser is relatively small when compared to all potential consumers. While it may be effective to try and obtain a larger share of these lottery consumers through advertising, it may not make sense to compete on price unless the firm can price discriminate against these individuals specifically to counteract a competitor's advertising. Ultimately, these assumptions are made to make the model tractable, eliminating the pricing decision from the firm's problem and reducing the decision to a prize offer.[4] As a result, the price of

[4]If a strategic pricing stage is included before the final stage, the optimal price for the sponsor becomes a non-linear function which in some situations may depend on interactions between the prize offers. This makes the first stage prize offer decision a complex problem.

both private goods is fixed at $p > 1$.

Returning attention to the consumer's problem, provided that $0 < g_i < w$ for all i, the optimal decision will equate the marginal benefit of contributions to the public good—composed of both the direct benefit $u'(G)$ and the increased likelihood of winning the lottery prize— with the marginal cost of contribution expressed in terms of the utility value of the private good forgone:

$$B\frac{G_{-i}}{G^2} + u'(G) = \frac{1}{p} \tag{3.3}$$

where $G_{-i} = G - g_i$ are the donations of individuals other than i. The standard linear voluntary contribution model is nested within; with $u'(G) = \alpha$, $B = 0$, and $p_G = p = 1$, the individual will contribute her entire endowment if $\alpha > 1$ and nothing otherwise.

There exists a unique equilibrium solution $G^*(B)$ to this problem. The right hand side of Equation 3.3 is positive and constant with respect to G while the left-hand side is clearly decreasing in G. As G approaches 0, the left hand side increases without bound, provided that $B > 0$. As a result, if $B > 0$, then there exists some $G^*(B) > 0$ such that the above condition is satisfied. Otherwise, if $B = 0$ then there exists some $G^*(B) \geq 0$ such that the above condition is satisfied. Lemma 1 follows directly from the fact that total contributions are increasing in the prize amount, as established in Morgan (2000) and Lange (2006).

Lemma 1. *Equilibrium public good contributions by consumers in the firm-sponsored fixed-prize lottery are given by $G^*(B) \geq 0$ that satisfies Equation 3.3. Further, $G^*(B) = 0$ only if $B = 0$. Let $X^*(B) = (1/p)(Nw - G^*(B))$ be the total amount of private good sold in this equilibrium, which is decreasing in B ($X^{*'}(B) < 0$).*

The main result of Lemma 1 follows directly from the assumptions of a fixed budget in tandem with the effect of the prize on the incentive to contribute. Obviously, in practice it may be the case that contributions to a particular public good lottery have a minimal impact on the private good budget for the consumer. For example, the amount you bid towards charity lotteries that feature small denomination gift cards or gift baskets may not realistically impact your budget for these consumer items in the future. Shutting down this channel would certainly increase the firms' incentives to donate as the only cost of donation would be the direct cost of the merchandise, eliminating the additional indirect cost of reducing market size. However, some lotteries feature large luxury prizes that could conceivably attract large individual donations which would impact the consumer's budget. When an organization in Missouri called "Gateway to a Cure" advertised prizes such as luxury homes, automobiles, and college scholarships for high-profile raffles benefiting spinal cord research, individuals paid as much as $1,000 apiece for tickets. Certainly, such large donations would have some impact on a consumer's budget. Incidentally and apropos to the motivation for this paper, the organization had to pay $2 million in restitution to raffle participants when it was discovered that they could not furnish the promised prizes to the raffle winners due to a lack of funds.[5] In addition, the fixed budget assumption also drives a wedge between the exclusive and non-exclusive fixed advertising mechanisms discussed below due to the additional externality related to bidding in such an environment. This allows me to derive an additional set of comparative statics to test in the lab. As a result, this first step towards modelling this environment employs the simple assumption of a fixed budget.

I will now turn my attention to the firms in the model. First, I will examine the fixed advertising environments, both with exclusive fundraisers and non-exclusive fundraisers. Subsequently, I will examine the proportional advertising environment when the fundraiser is non-exclusive. Finally, an example with the familiar linear structure will be provided.

3.2.2 EXCLUSIVE FUNDRAISER WITH FIXED ADVERTISING

The private good market is supplied by two *ex ante* identical, profit-maximizing firms indexed by $j \in \{1, 2\}$ with constant unit marginal costs.[6] Prior to the decisions made by

[5] The press release is available at `http://ago.mo.gov/newsreleases/2007/012907.htm`.

[6] I consider firms that place no value on the provision of the public good itself; the alternative can be considered an extension of Lange (2006) with lower costs of prize donations.

consumers in the lottery, the firms are asked to make a prize offer b_i. Consider the scenario where advertising is fixed and the fundraiser only accepts the more generous offer, i.e. $B = \max\{b_1, b_2\}$. In this case, the profit function has a discontinuity at the point where the firm's offered prize exceeds that of its competitor:

$$\pi_j(b_j, b_{-j}) = \begin{cases} \lambda(p-1)X^*(b_j) - b_j & \text{, for } b_j > b_{-j} \\ (1-\lambda)(p-1)X^*(b_{-j}) & \text{, for } b_j < b_{-j} \\ 1/2(p-1)X^*(b_j) - b_j/2 & \text{, otherwise} \end{cases} \tag{3.4}$$

This feature makes the first stage of the game similar in many ways to a symmetric Bertrand duopoly.[7] As in price competition, the discontinuity in the profit function guarantees that firms will increase their prize offers to the point where there are no rents to be gained from winning the sponsorship. The analog in this situation is a symmetric pair of prize offers $(b_1, b_2) = (b^*, b^*)$ that make each firm indifferent between losing the sponsorship and winning the sponsorship given accounting for the cost of the offer:

$$\lambda(p-1)X^*(b^*) - b^* = (1-\lambda)(p-1)X^*(b^*) \tag{3.5}$$

In this case, both firms get the tie payoff $(1/2(p-1)X^*(b^*) - b^*/2)$ which is equivalent to both the left hand side and the right hand side of Equation 3.5. Neither firm has an incentive to offer $b' > b^*$ as it will result in winning the sponsorship for sure and getting $\lambda(p-1)X^*(b') - b'$, which is decreasing in b by Lemma 1. Deviating downwards to $b' < b^*$ will not change the firm's payoff; the firm would get $(1-\lambda)(p-1)X^*(b^*)$ for sure. In addition, neither firm can credibly commit to staying out of the fundraiser altogether, even though the resulting market for the private good would be larger as a result. Consider a situation where neither firm participates, resulting in $b_1 = b_2 = 0$. In the second stage, the firms will split the market down the middle. So for any $\lambda > \frac{1}{2}$, either firm then has an incentive to offer a vanishingly small amount of output to the fundraiser as a prize in

[7] As noted by an anonymous referee, this environment also closely resembles a symmetric first-price pure common value auction with perfect information and a negative externality.

order to secure the larger share resulting from the sponsorship status, provided that X^* is continuous at zero.

Proposition 1. *If $X^*(B)$ as given in Lemma 1 is continuous at zero, there exists a symmetric pair of positive offers $(b_1, b_2) = (b^*, b^*)$ that satisfies Equation 3.5 and, along with $G^*(B)$, characterizes the unique symmetric subgame perfect equilibrium of the firm-sponsored fixed prize lottery when the fundraiser is exclusive and uses fixed advertising.*

3.2.3 NON-EXCLUSIVE FUNDRAISER WITH FIXED ADVERTISING

Now consider the scenario in which advertising is fixed and the fundraiser accepts prizes from *both* firms if offered ($B = b_1 + b_2$). Due to the choice of advertising institution, the more generous firm is still labeled as the "sponsor", receiving a fixed λ share of the private good market. In this case, the profit function changes slightly from the one above:

$$\pi_j(b_j, b_{-j}) = \begin{cases} \lambda(p-1)X^*(b_j + b_{-j}) - b_j & \text{, for } b_j > b_{-j} \\ (1-\lambda)(p-1)X^*(b_j + b_{-j}) - b_j & \text{, for } b_j < b_{-j} \\ 1/2(p-1)X^*(b_j + b_{-j}) - b_j & \text{, otherwise} \end{cases} \tag{3.6}$$

As a result, this scenario is more similar to a common value all-pay auction with perfect information. In this situation, the common value is dependent on the sum of the prize offers. Similarly to the complete information all-pay auction studied extensively in Baye et al. (1996), it is straightforward to show that there is no symmetric pure-strategy equilibrium. For any non-negative pair of prize offers $b_1 = b_2 = b \geq 0$, both firms receive $1/2(p-1)X^*(2b) - b$ for sure. If this payoff is negative, obviously it is profitable to deviate to a prize offer of zero and guarantee a positive payoff. If this payoff is positive and $X^*(B)$ is continuous, then there exists some higher prize offer $b' = b + \varepsilon$ such that

$$\lambda(p-1)X^*(2b+\varepsilon) - (b+\varepsilon) > 1/2(p-1)X^*(2b) - b. \tag{3.7}$$

As a result, there is no symmetric equilibrium in pure strategies.

To find a symmetric equilibrium, I must consider mixed strategies over prize offers. Assume that, in equilibrium, the probability of a firm offering a prize less than b is given by some atomless distribution function $F(b)$ defined over some support $[a, \bar{B}]$. Consider a lower bound of the support $a > 0$. In equilibrium, firms must receive the same payoff in expectation from all pure strategies in the support which must in turn be greater than the payoff from any pure strategy outside of the support. At $b_j = a$, $b_{-j} > b_j$ with probability 1 and the firm receives $(1-\lambda)(p-1)E[X^*(a+b_{-j})] - a$ for sure. It is easy to see that $b_j = 0$ would provide a greater payoff, as X^* is decreasing in the sum of offers by Lemma 1. As a result, the support of $F(b)$ must be bounded from below at zero.

The problem of a firm facing an identical competitor who is using the mixed strategy discussed above is written

$$\max_b \left[\int_0^b \lambda(p-1)X^*(b+\delta)\frac{f(\delta)}{F(b)}d\delta\right]F(b) + \left[\int_b^{\bar{B}}(1-\lambda)(p-1)X^*(b+\delta)\frac{f(\delta)}{1-F(b)}d\delta\right](1-F(b)) - b \tag{3.8}$$

The first order condition of the maximization problem is the following differential equation (with boundary condition $F(0) = 0$ as argued above).

$$(2\lambda-1)(p-1)X^*(2b)f(b) + \int_0^b \lambda(p-1)X^{*'}(b+\delta)f(\delta)d\delta + \int_b^{\bar{B}}(1-\lambda)(p-1)X^{*'}(b+\delta)f(\delta)d\delta = 1 \tag{3.9}$$

This differential equation is problematic to solve, unless the derivative $X^{*'}(b+\delta)$ is a constant h which, over the range of integration, does not depend on δ. In this case, everything but the densities can be pulled out of the integrals to leave us with

$$(2\lambda-1)(p-1)X^*(2b)f(b) + \lambda(p-1)hF(b) + (1-\lambda)(p-1)h(1-F(b)) = 1 \tag{3.10}$$

which is tractable. This linearity will hold in the example below, which will allow me to derive a closed form solution for the equilibrium mixed strategy $F^*(b)$.

Proposition 2. *If $X^*(B)$ as given in Lemma 1 is linear in B, there exists a mixed strategy given by an atomless distribution function $F^*(b)$ that satisfies Equation 3.9 and, along* *with $G^*(B)$, characterizes a symmetric subgame perfect equilibrium of the firm-sponsored fixed prize lottery when the fundraiser is non-exclusive and uses fixed advertising. This equilibrium is unique in the space of atomless distributions.*

3.2.4 NON-EXCLUSIVE FUNDRAISER WITH PROPORTIONAL ADVERTISING

Finally, consider the scenario in which the fundraiser accepts prizes from both firms if offered ($B = b_1 + b_2$), but the advertising is proportional to the prize offers. In this case, the profit function changes again:

$$\pi_j(b_j, b_{-j}) = \begin{cases} \frac{b_j}{b_j+b_{-j}}(p-1)X^*(b_j+b_{-j}) - b_j & \text{, for } b_j + b_{-j} > 0 \\ 1/2(p-1)X^*(0) & \text{, otherwise} \end{cases} \quad (3.11)$$

In particular, profits in this game are completely continuous in b_j for $b_{-j} > 0$. If an interior solution exists, the optimal firm choice of b_j will equate the marginal benefit of increasing its prize offer— the additional market share— with the marginal cost of increasing its prize offer— the actual unit cost plus the decrease in total private good sales resulting from a larger prize pool:

$$\frac{b_{-j}}{(b_j+b_{-j})^2}(p-1)X^*(b_j+b_{-j}) = 1 + \frac{b_j}{b_j+b_{-j}}(p-1)X^{*'}(b_j+b_{-j}) \quad (3.12)$$

The solution of this equation provides us with the best response for firm j to any non-zero prize offer of its competitor.[8] Call this object $f(b_j)$ which maps prize offers by one firm into the optimal prize offer for its competitor. Provided that $f(b_j)$ is a continuous function and that its domain is compact and convex, there will exist a fixed point $f(b^*) = b^*$ which will describe the symmetric equilibrium in pure strategies for this game.

Proposition 3. *If $X^*(B)$ as given in Lemma 1 is continuous at zero and the space of firm offers is bounded from above, there exists exists a symmetric pair of positive offers*

[8] As above, $b_1 = b_2 = 0$ can not be an equilibrium provided that X^* is continuous as either firm would have an incentive to deviate to $\varepsilon > 0$ such that $(p-1)X^*(\varepsilon) - 1/2(p-1)X^*(0) > \varepsilon$. That such an ε exists is given by the continuity of X^*.

$(b_1, b_2) = (b^*, b^*)$ *that satisfies Equation 3.12 and, along with* $G^*(B)$, *characterizes the unique symmetric subgame perfect equilibrium of the firm-sponsored fixed prize lottery when the fundraiser is non-exclusive and uses proportional advertising.*

3.2.5 EXAMPLE

Suppose the utility functions of consumers in this environment are fully linear and assume a marginal utility of the public good of α:

$$U_i(x_i, G) = x_i + x_w(g_i, G, B) + \alpha G. \tag{3.13}$$

In equilibrium, the aggregate demand functions for this set of consumers can be written as follows:

$$\begin{aligned} G^*(B) &= \frac{N-1}{N}\frac{Bp}{1-\alpha p} && (3.14) \\ X^*(B) &= \frac{1}{p}\left(Nw - \frac{N-1}{N}\frac{Bp}{1-\alpha p}\right) && (3.15) \end{aligned}$$

where B is the total amount of the prize. These demand functions induce equilibrium prize offers under each of the proposed environments above. With an exclusive fundraiser and fixed advertising, the equilibrium prize offer for both firms solves Equation 3.5 given the demand function above:

$$B^*_{exc,fixed} = \frac{(2\lambda-1)(p-1)(\alpha p-1)N^2 w}{p[(2\lambda-1)(p-1) + N((1+\alpha)p - 2 - 2\lambda(p-1))]} \tag{3.16}$$

With a non-exclusive fundraiser and fixed advertising, the equilibrium is characterized by a symmetric mixed strategy. As the derivative of private good demand with respect to an individual firm's prize offer is a constant, the simplified differential equation can be used to derive the equilibrium mixed strategy. The distribution $F_{non,fixed}(b)$ describing prize offers in this environment satisfies Equation 3.9:

$$F^*_{non,fixed}(b) = \frac{[(N-1)(p-1)\lambda + Np(\alpha-1) + p - 1]\left[\sqrt{2bp(N-1) + N^2w(\alpha p-1)} - N\sqrt{w(\alpha p-1)}\right]}{(N-1)(2\lambda-1)(p-1)\sqrt{2bp(N-1)+N^2w(\alpha p-1)}} \tag{3.17}$$

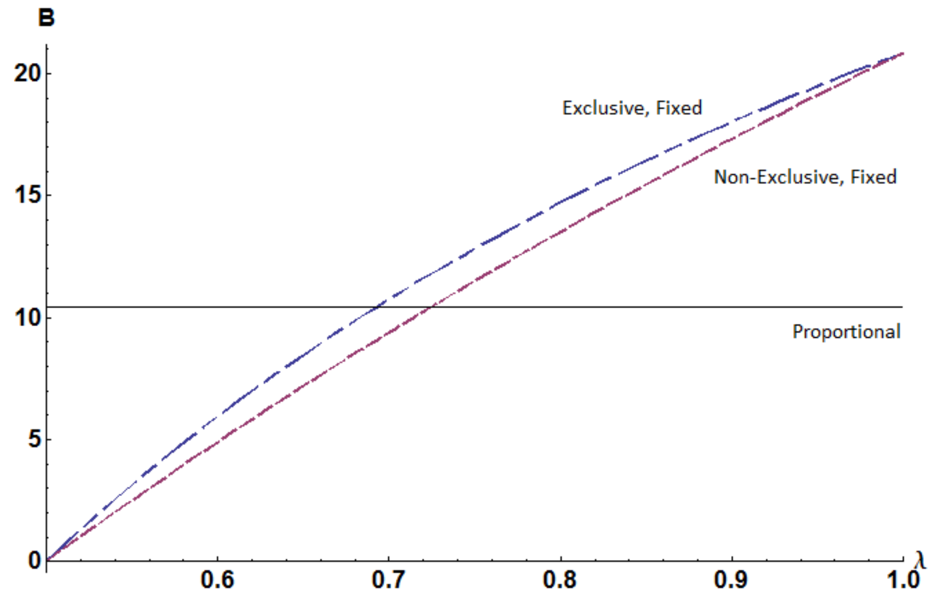

Figure 3.1: Equilibrium Prize Amounts

Finally, with a non-exclusive fundraiser and proportional advertising, the equilibrium prize offer for both firms solves Equation 3.12:

$$b^*_{non,prop} = \frac{(p-1)(\alpha p - 1)N^2 w}{4p[p(1+N(\alpha-1))-1]} \tag{3.18}$$

To fix ideas, assume the following parameter values which will be used in the experimental design: $N = 4$, $\sum w_i = Nw = 100$, $\alpha = .25$, and $p = 1.5$. Under this parameterization, I can characterize the expected value of equilibrium prize amounts under fixed advertising as a function of λ. In addition, I can express the equilibrium prize amount under proportional advertising as a constant. These functions are depicted in Figure 3.1. The upper dashed line depicts $B^*_{exc,fixed}$, the dashed line below it depicts the expected total amount of prize offers under $F^*_{non,fixed}(b)$, and the horizontal black line depicts twice $b^*_{non,prop}$, the total prize amount offered in that equilibrium. Obviously, the revenue maximizing mechanism is sensitive to the value of fundraiser advertising. In regions where this is low, it is best to be non-exclusive and reward firms in proportion to their donations. However, if the endorsement of the fundraiser is very persuasive due to the availability of advertising resources or the preferences of the consumers, it is best to be exclusive and reward the sponsoring firm generously. Also note that when advertising rewards are fixed, the exclusive fundraiser always outperforms the non-exclusive fundraiser. This results from the externalities of bidding higher in the non-exclusive environment. Unlike in the exclusive environment, a higher bid from a firm always increases the prize amount, even in the event that the firm does not secure the sponsorship. The increase in the prize then decreases total private good expenditures, creating an additional downward pressure on bids that is not present in the exclusive environment.

3.3 EXPERIMENTAL DESIGN

It would be ideal to collect data from the field in order to analyze the environment empirically, as there are many charities that use donations from firms as lottery prizes. In addition, the anecdotal evidence presented in section 1 suggests that these charities provide endorsements to the firms choosing to make donations, which would impact the marginal lottery participants' consumption decisions. In these ways, the activities of such charities reflect some of the key features of the model. However, reliable data on these types of raffles is not readily available. Even if the organizations conducting these events kept detailed records, some of the parameters of the model would be impossible to identify. The value of prizes, number of participants, and total contributions in each lottery could be measured, but the neither the total number of individuals impacted through the advertising channel nor the future purchase decisions of those individuals could be observed. Since the reward to the sponsoring firm in terms of market share is an important feature in the model, there would be no way to test the comparative statics of the model via traditional empirical methods.

As a result, to test which of the proposed mechanisms performs best in terms of prizes and contributions, I conduct a laboratory experiment with the features of the environments modeled above. In each of seven sessions, subjects played the variations of the firm-sponsored fixed-prize lottery game outlined above.[9] In all sessions, the subjects were divided into sets of 12 for the purposes of matching. For the length of each session, members of each set of 12 interacted with each other, but never with members of the other set. While some session effects may apply to both set, each sequence of interactions within sets is treated as an independent observation for the purposes of statistical analysis. Each session featured two sequences of 20 rounds. In five of these sessions, participants played one sequence of the exclusive fixed advertising variant and one sequence of the non-exclusive fixed advertising variant.[10] The remaining two sessions featured two sequences of the non-exclusive, proportional environment only. The sessions featuring the fixed advertising technology were split between a more competitive parameterization, featuring a λ of 0.8, and less competitive parameterization, featuring a λ of 0.6. As the experiment focuses on firm decisions, the split of purchases implied by the λ parameter was imposed exogenously, rather than allowing the consumers to decide between private good providers. As a result, contributors chose only how much to contribute to the public good. The remainder of their endowments was used to purchase a private good with the resulting profits split between firms as dictated by λ. The other parameters used in the experiment were the same as those used in the linear example above: $N = 4$, $\sum w_i = Nw = 100$, $\alpha = .25$, and $p = 1.5$.

At the start of each sequence of 20 rounds, subjects were assigned roles that persisted

[9] Six sessions utilized 24 subjects with one additional session containing only 12 subjects.

[10] The order in which the two fixed advertising variants were played was switched between sessions to verify that there were no order effects. A Wilcoxon ranksum test fails to reject the null hypothesis that the difference in average prize amounts, offers, and group contributions between fixed mechanisms were equal across sessions with different orderings of those mechanisms.

through the entire sequence. Each set of 12 was divided into two roles: 4 firms or "first-movers" and 8 contributors or "second-movers". Roles were randomly assigned at the beginning of each sequence, with the qualification that subjects could not play as a first-mover in both sequences in order to eliminate cross-treatment learning effects. In each round of that sequence, subjects were randomly matched into groups consisting of 2 first-movers and 4 second-movers. In the first stage of the round, each first-mover was given an endowment of 20 tokens and asked to offer a prize amount. Following the first-movers' decisions, the final prize amount— either the higher of the two offers or the sum of the two offers, depending on the environment— was revealed to second-movers who were asked to divide an endowment of 25 tokens between the private and public accounts. The total payoff in points for *both* firms was always equal $(p - c)/p$ =1/3 the number of tokens invested by contributors in private account, with the division between firms depending on their relative prize offers and the specific environment:

- In the exclusive fundraiser with fixed advertising treatment, the first-mover making the higher (sponsor) offer received $\lambda \in \{0.6, 0.8\}$ of those points and had to pay his prize offer and the first-mover making the lower offer (non-sponsor) received $(1 - \lambda) \in \{0.4, 0.2\}$ of those points, but incurred no additional cost.
- In the non-exclusive fundraiser with fixed advertising treatment, the sponsor received $\lambda \in \{0.6, 0.8\}$ of those points, the non-sponsor received $(1 - \lambda) \in \{0.4, 0.2\}$ of those points, and both had to pay their prize offers.
- In the non-exclusive fundraiser with proportional advertising treatment, firm j received $b_j/(b_j + b_{-j})$ of those points and both firms had to pay their prize offers.

For second-movers, investing a token in the public account yielded a return of α =1/4 points to each member of the group as well as a chance to win the lottery prize. Investing a token in the private account yielded a return of 2/3 points to the second-mover alone. After both stages completed, the winner of the lottery was randomly chosen from amongst those eligible (as in the model) and payoffs were calculated for the round. At the end of the experiment, each participant was paid for a total of two randomly selected rounds; one from each 20 round sequence. Each treatment environment was featured in at least two sessions, yielding two independent observations per session for a total of at least four observations of each environment.[11] The equilibrium predictions are reproduced in Table 3.1 and suggest the following hypotheses

Hypothesis 1. *With* $\lambda = 0.6$*, prize amounts will be ranked as follows:* $B_{non,prop} > B_{exc,fixed} > B_{non,fixed}$.

Hypothesis 2. *With* $\lambda = 0.8$*, prize amounts will be ranked as follows:* $B_{exc,fixed} > B_{non,fixed} > B_{non,prop}$.

Hypothesis 3. *With* $\lambda = 0.6$*, individual firm prize offers will be ranked as follows:* $b_{exc,fixed} > b_{non,prop} > b_{non,fixed}$.

Hypothesis 4. *With* $\lambda = 0.8$*, individual firm prize offers will be ranked as follows:* $b_{exc,fixed} > b_{non,fixed} > b_{non,prop}$.

Hypothesis 5. *Total contributions will increase with prize amounts. As such, with* $\lambda = 0.6$ *total contributions will be ranked as follows:* $G_{non,prop} > G_{exc,fixed} > G_{non,fixed}$. *With* $\lambda = 0.8$*, total contributions will be ranked as follows:* $G_{exc,fixed} > G_{non,fixed} > G_{non,prop}$.

Subjects were drawn from the Pittsburgh Experimental Economics Lab (PEEL) subject pool, comprised mostly of undergraduate students. As this is a novel setting, no subjects had prior experience in this setting and no subject was allowed to participate in more than one session. Subjects interacted with each other via networked computers and the experimental software was programmed in Python using the Willow package.[12]

[11]The more competitive fixed environments ($\lambda = 0.8$) feature an additional observation from a session containing only 12 subjects

[12]Information on Willow can be found at http://econwillow.sourceforge.net.

Table 3.1: Equilibrium Predictions for the Sponsored Public Goods Lottery

	$\lambda = 0.6$		
	Exclusive	Non-Exclusive	Proportional*
Private Good Purchases	59.52	60.78	54.17
Public Good Contributions	10.71	8.82	18.75
Total Prize Amount	5.95	4.90	10.42
	$\lambda = 0.8$		
	Exclusive	Non-Exclusive	Proportional*
Private Good Purchases	49.02	50.45	54.17
Public Good Contributions	26.47	24.32	18.75
Total Prize Amount	14.71	13.51	10.42

Predictions are for 4 consumers with an aggregate endowment of 100 and 2 firms. Price of the private good is 1.5 and price of the public good is normalized to one. Marginal costs for the firm are equal to unity. Marginal utility of the public good is equal to .25.
* Included in each panel for purposes of comparison; λ is not a parameter in the Proportional mechanism.

3.4 RESULTS

The seven experimental sessions were conducted between April 2012 and September 2012. A total of 156 subjects participated in the experiment, earning an average of \$19.00 with a minimum of \$13.92 and a maximum of \$28.41. First, I will discuss the primary findings as they relate to the main hypotheses regarding prizes, offers, and contribution amounts at the aggregate level. After, I will offer a variety of explanations for the patterns observed in the data generated by first-movers, as theirs is the more novel of the two roles in the experiment. Finally, I will offer evidence consistent with some of the offered explanations.

3.4.1 PRIZES

First-mover behavior is summarized primarily by aggregate prize amounts as well as individual prize offers. Hypothesis 1 states that in fixed advertising environments with $\lambda = 0.6$, prize amounts will be lower than in the non-exclusive, proportional advertising environment. In fixed environments with the larger λ value of 0.8, Hypothesis 2 indicates that prize amounts will be larger than in the non-exclusive, proportional advertising environment. In addition, between the two environments featuring fixed advertising technologies, both hypotheses predict that total prize amounts will be larger when sponsorship is exclusive. A summary of the empirical support for these hypotheses is presented in Findings 1 and 2.

Finding 1. *Average prize amounts are generally higher under the proportional advertising mechanism than under either fixed mechanism, especially when the fixed reward is low* ($\lambda = 0.6$)*.*

Finding 2. *When advertising is fixed, average prize amounts are slightly higher when the fundraiser is non-exclusive, regardless of the value of* λ*, but these differences are statistically insignificant.*

Table 3.2: Average Prize Amounts by Session

	$\lambda = 0.6$		
	Exclusive	Non-Exclusive	Proportional
Session 1	5.63	2.64	14.05
Session 2	3.37	5.51	10.28
Session 3	2.47	1.81	16.45
Session 4	4.48	6.39	10.07
Average	3.99	4.09	12.71
	$\lambda = 0.8$		
	Exclusive	Non-Exclusive	Proportional
Session 1	8.46	15.31	14.05
Session 2	11.53	10.24	10.28
Session 3	8.66	10.08	16.45
Session 4	6.55	7.63	10.07
Session 5*	8.95	11.46	–
Average	8.83	10.94	12.71

* Session 5 was conducted with only 12 participants.

In Table 3.2, the data for sessions where $\lambda = 0.6$ are reported. The proportional mechanism generates larger prizes than either ofthe fixed treatments under this parameterization (p-value < 0.05 for both tests).[13] In fixed treatments, however, average prize amounts are statistically indistinguishable across mechanisms and generally lower than equilibrium predictions, a feature of the data that will persist across parameterizations as well. Still, the variation in prize amounts across treatments in these sessions generally conform to the equilibrium benchmarks.

While observed prize amounts under fixed advertising technology are larger under the more competitive $\lambda = 0.8$ than under $\lambda = 0.6$, the equilibrium benchmarks are far less accurate predictors of behavior when comparing across sessions with the higher λ. In fact, the empirical rankings are the reverse of the comparative static predictions. The exclusive, fixed mechanism, which was revenue dominant in this environment according to the symmetric equilibria derived above, generates the smallest prize amounts empirically. In addition, in contradiction of Hypothesis 2 proportional advertising generates prize amounts larger than either fixed environment, although this difference is only significant when comparing to the sequences with exclusive sponsorship. As in the less competitive environment, prizes under fixed advertising are consistently lower than equilibrium benchmarks. On average, prizes in the non-exclusive, fixed environment are 82% of equilibrium predictions and prizes in the exclusive, fixed environment are 64% of their equilibrium level. In contrast, prizes in the proportional environment are roughly 22% higher than equilibrium benchmarks. It appears that first-movers are relatively more conservative with their prize offers under the exclusive mechanism than under the non-exclusive mechanisms. These data can be visualized in aggregate as in Figure 3.2.

A regression analysis of total prize amounts is also reported in Table 3.3, taking each randomly matched group as the unit of observation. Indicator variables for each treatment environment are included in all specifications. As with the aggregate analysis, we can see

[13]The five main hypotheses are tested via Wilcoxon rank-sum tests comparing session averages of the amounts in question.

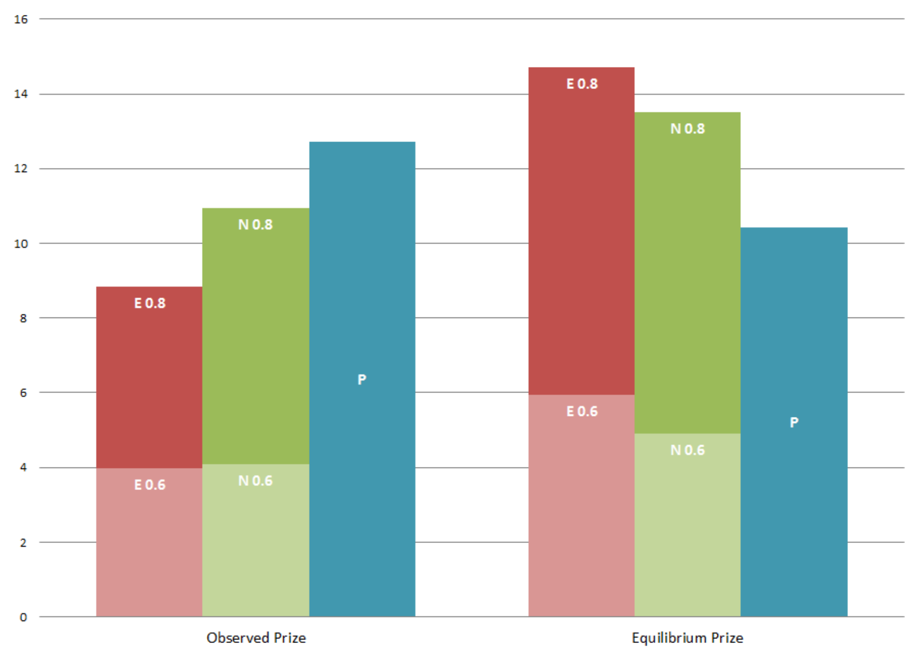

Figure 3.2: Prize Amounts, Observed vs. Predicted

from the regressions that the prize amounts are generally highest under the proportional mechanism (the omitted category) and that prize amounts increase with the λ parameter within the fixed advertising environments. In column 2, sequence and round dummies are added to control flexibly for dynamics. Any trends over the course of a session appear slight; coefficients on the second sequence dummy in column 2 are not statistically significant. Finally, as total prize amounts are decided at the group level, the characteristics of the individual first-movers in the group might explain some of the variation in prizes. Column 3 includes as controls the number of female first movers in the group (0,1, or 2) and the average score of the first-movers in the group on a brief 5 question quiz designed to gauge

Table 3.3: OLS Regressions of Prize Amounts

	(1)	(2)	(3)
Exclusive, $\lambda = 0.6$	-8.723***	-8.723***	-8.425***
	(1.663)	(1.822)	(1.462)
Exclusive, $\lambda = 0.8$	-3.882**	-3.848**	-3.894**
	(1.691)	(1.717)	(1.672)
Non-exclusive, $\lambda = 0.6$	-8.621***	-8.621***	-8.538***
	(1.842)	(1.975)	(1.828)
Non-exclusive, $\lambda = 0.8$	-1.767	-1.801	-1.804
	(1.995)	(2.032)	(2.106)
Second sequence		0.339	0.300
		(1.069)	(1.056)
Female first-movers			-0.307
			(0.670)
Avg. quiz score			-0.834
			(0.788)
Observations	1,040	1,040	1,040
R-squared	0.375	0.386	0.397
Round FE	NO	YES	YES

Notes: Bootstrapped standard errors clustered by session/mechanism in parentheses. Proportional mechanism is the omitted category.

* Significant at the 10% level, ** Significant at the 5% level,

*** Significant at the 1% level.

understanding of basic math.[14] Neither variable enters significantly into the model.

As some mechanisms are exclusive and some are non-exclusive, there are theoretical and empirical differences between prize amounts and individual prize offers. Hypotheses 3 and 4 deal with the ranking of individual firm prize offers under $\lambda = 0.6$ and $\lambda = 0.8$, respectively. Hypothesis 3 states that in fixed advertising environments with $\lambda = 0.6$ prize offers will be higher under the exclusive mechanism than under the non-exclusive mechanism and, further, that offers in the proportional advertising environment will fall somewhere in between. In fixed advertising environments where $\lambda = 0.8$, prize offers are still predicted to be higher under the exclusive mechanism. However, with the more competitive fixed advertising environment, offers in both exclusive and non-exclusive mechanisms are expected to be higher than in the non-exclusive, proportional advertising environment. These hypotheses are tested in the experimental data, and the results are presented in Findings 3 and 4.

Finding 3. *Average prize offers are higher under the proportional advertising mechanism than under either fixed mechanism when the fixed reward is low ($\lambda = 0.6$) and similar otherwise.*

Finding 4. *There is no statistical difference between prize offers across fixed mechanisms, regardless of the value of λ.*

A summary of average prize offers by session and treatment can be seen in Table 3.4. As with prize amounts, observed prize offers are higher in the proportional treatments than in either of the fixed advertising treatments with $\lambda = 0.6$ in contrast with the equilibrium predictions (p-value < 0.05 for both tests). The relative ranking of exclusive vs. non-exclusive arrangements under fixed advertising is consistent with equilibrium predictions, with the exclusive arrangement creating larger incentives for prize offers than the non-exclusive; however, the difference is not significant.

[14]These controls come from a questionnaire that was administered at the end of the session to avoid priming effects and was not incentivized.

Table 3.4: Average Prize Offers by Session

	Exclusive	Non-Exclusive	Proportional
	$\lambda = 0.6$		
	Exclusive	Non-Exclusive	Proportional
Session 1	4.48	1.32	7.02
Session 2	2.36	2.76	5.14
Session 3	3.53	3.20	8.23
Session 4	2.02	0.90	5.03
Average	3.10	2.04	6.36
	$\lambda = 0.8$		
	Exclusive	Non-Exclusive	Proportional
Session 1	7.74	7.65	7.02
Session 2	9.55	5.12	5.14
Session 3	7.19	5.04	8.23
Session 4	5.07	3.82	5.03
Session 5*	7.53	5.73	-
Average	7.42	5.47	6.36

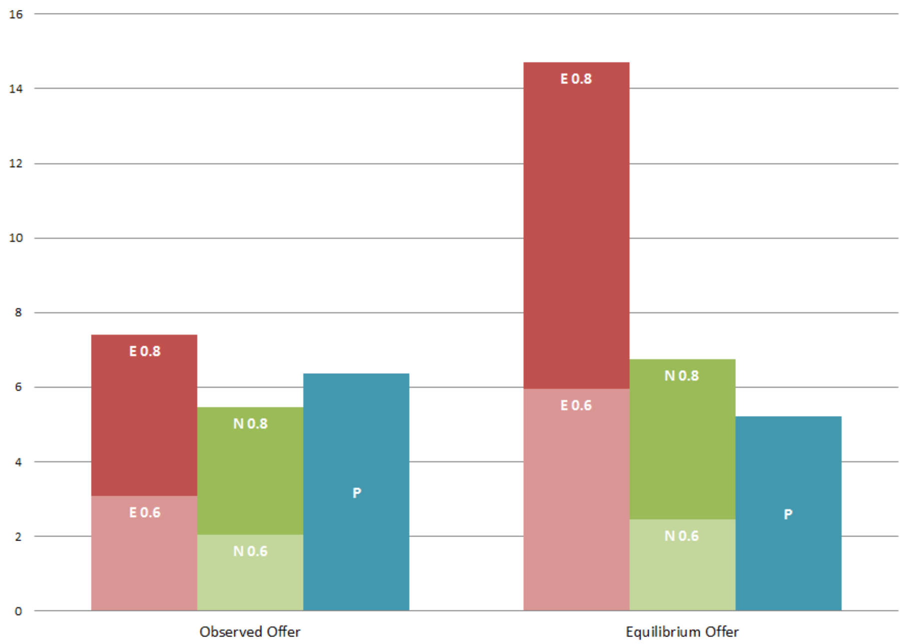

Figure 3.3: Prize Offers, Observed vs. Predicted

With $\lambda = 0.8$ in the fixed advertising setting, offers are again higher in the exclusive treatment than in the non-exclusive treatment, although not at a conventional level of statistical significance. In fact, none of the pairwise comparisons of average prize offers are statistically significant. The average offer in the exclusive treatment of 7.42 is highest, as in the equilibrium benchmarks, but the offers are not nearly as high as the equilibrium would predict. Figure 3.3 shows these averages in comparison with the associated equilibrium calculations.

As opposed to prize amounts which are jointly determined, prize offers represent individual decisions. As such there is potential to explain some of their variation using individual characteristics in a regression analysis. Table 3.5 reports the results of such an

Table 3.5: OLS Regressions of Prize Offers

	(1)	(2)	(3)	(4)
Exclusive, $\lambda = 0.6$	-3.260***	-3.260***	-3.106***	-3.260***
	(0.932)	(1.003)	(0.837)	(0.571)
Exclusive, $\lambda = 0.8$	1.061	1.055	1.027	1.055*
	(1.009)	(1.024)	(1.037)	(0.624)
Non-exclusive, $\lambda = 0.6$	-4.311***	-4.311***	-4.265***	-4.311***
	(0.921)	(0.990)	(0.912)	(0.593)
Non-exclusive, $\lambda = 0.8$	-0.884	-0.877	-0.891	-0.877
	(0.998)	(1.043)	(1.052)	(0.749)
Second sequence		-0.0605	-0.0830	-0.0605
		(0.612)	(0.593)	(0.452)
Female			-0.380	
			(0.513)	
Quiz score			0.0275	
			(0.0531)	
Observations	2,080	2,080	2,080	2,080
R-squared	0.242	0.250	0.366	0.25
Round FE	NO	YES	YES	YES
Individual RE	NO	NO	NO	YES

Notes: Bootstrapped standard errors clustered by session/mechanism in parentheses. Proportional mechanism is the omitted category.

* Significant at the 10% level, ** Significant at the 5% level,

*** Significant at the 1% level.

analysis, using responses from a brief questionnaire as controls. Column 1 reiterates that prize offers are higher under the exclusive mechanism with $\lambda = 0.8$ than under the proportional mechanism, but that the proportional mechanism generates larger offers on average than any of the other environments. Column 2 includes round and sequence dummies, but offers do not seem to systematically increase or decrease over the course of a given sequence or session. Column 3 adds individual level control variables (gender, age, and quiz score) and column 4 includes random effects for each first-mover. Still, individual level variation explains little of first-mover behavior. After controlling for these effects, the coefficients on the treatment indicators retain sign and magnitude.

The predictions of the model fare well when comparing across parameterizations, but fail when comparing across advertising environments. Unsurprisingly, prize amounts increase with the rewards to sponsorship, as predicted by the model. In addition, as hypothesized the proportional mechanism outperforms both fixed mechanisms— exclusive and non-exclusive— in terms of total prize amounts when the sponsor receives a relatively low share. In contrast with the equilibrium predictions, however, prize amounts remain higher under the proportional mechanism than in either of the fixed mechanisms, even when the sponsor in said fixed mechanisms receives a high share. Furthermore, the rankings of offers are not consistent with the comparative static predictions, seemingly due to consistent conservatism amongst first-movers in fixed advertising environments. This pattern of behavior among first-movers will be examined in detail later on. Next, however, I will examine the behavior of the second-movers.

3.4.2 CONTRIBUTIONS

As this environment focuses on the provision of public goods, second-mover behavior will be analyzed in terms of contributions to the public good and their response to the endogenously determined prize amounts offered by first-movers. Hypothesis 5 deals with total contributions; the equilibrium predictions of total contributions mirror those of the prize amounts, with individuals increasing their contributions in response to larger prizes. Across sessions, the empirical findings regarding average contribution behavior are summarized in Finding 5.

Finding 5. *There is weak evidence that sequences with higher average prize amounts experience higher average contributions. However, across all treatments and sessions, contributors that observe higher prize amounts also have higher contributions. The increase in contributions resulting from an increase in prize amounts corresponds closely to the symmetric equilibrium best response.*

As seen in Table 3.6, observed contributions roughly match the pattern observed in prize amounts, with higher prizes generating greater contributions as predicted by the equilibrium. Contributions are higher under the proportional mechanism than either of the fixed mechanisms when $\lambda = 0.6$ (p-value < 0.01) and between the fixed treatments differences in contributions are not statistically significant. When the sponsor's share is fixed at 0.8, the non-exclusive mechanism that generates larger prizes also generates more contributions and contributions are higher still under the proportional mechanism; still, none of the preceding comparisons is statistically significant. So across sessions, there is evidence albeit weak that higher prize amounts generate higher contributions. Figure 3.4 depicts these results visually. Still, comparing contributions across sessions is an indirect test of these hypotheses. Since the treatment environment should only impact second-mover behavior through prize amounts, an interesting question regards whether higher contributions result when prizes are larger, regardless of the environment.

Table 3.6: Average Contribution Amounts by Session

		$\lambda = 0.6$	
	Exclusive	Non-Exclusive	Proportional
Session 1	10.31	16.39	27.50
Session 2	5.92	8.02	26.30
Session 3	5.25	7.40	36.43
Session 4	4.53	4.98	18.64
Average	6.50	9.20	27.22
		$\lambda = 0.8$	
	Exclusive	Non-Exclusive	Proportional
Session 1	33.55	32.94	27.50
Session 2	24.80	31.89	26.30
Session 3	14.56	14.70	36.43
Session 4	9.62	20.21	18.64
Session 5*	28.22	32.67	–
Average	22.15	26.48	27.22

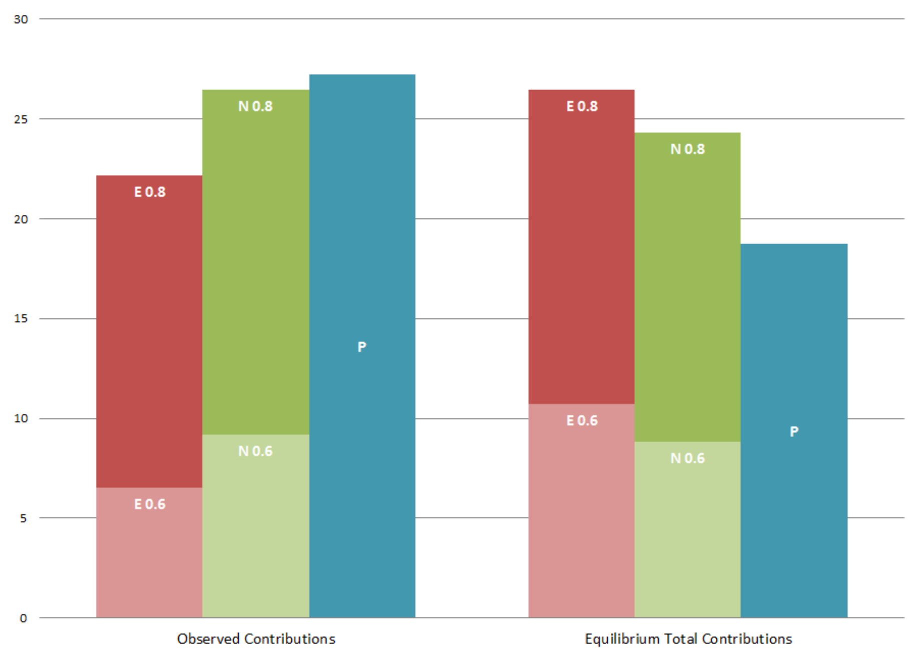

Figure 3.4: Total Contributions, Observed vs. Predicted

Pooling all second-mover decisions across treatments, Table 3.7 reports the results of this analysis. In column 1, the simplest conceivable specification models individual contributions as a constant plus a linear function of the prize amount. Incidentally, the equilibrium response for a risk neutral contributor conforms to this model, with optimal contributions being equal to 0.45 times the amount of the prize offer. The empirical data matches this equilibrium strategy remarkably well. The coefficient on prize amount in column 1 is equal to roughly 0.474 while the constant (unreported) is equal to 0.79. This result can be seen in Figure 3.5, where the equilibrium best response function is overlaid on the linear regression of contributions on prizes from the data. It seems that individuals are generally contributing more than the equilibrium predicts for all levels of the prize and

Table 3.7: OLS Regressions of Individual Contributions

	(1)	(2)	(3)	(4)	(5)
Prize Amt.	0.474***	0.474***	0.489***	0.824***	0.446***
	(0.0429)	(0.0429)	(0.0440)	(0.191)	(0.0412)
Previous first-mover			-1.295*	1.586*	
			(0.731)	(0.933)	
Female			-1.679***	-0.294	
			(0.488)	(0.741)	
Age			0.0544**	0.00302	
			(0.0268)	(0.0371)	
Quiz score			-0.560	0.0356	
			(0.393)	(0.361)	
Previous first-mover X Prize				-0.334***	
				(0.114)	
Female X Prize				-0.143**	
				(0.0727)	
Age x Prize				0.00551*	
				(0.00282)	
Quiz score X Prize				-0.0730*	
				(0.0393)	
Observations	4,160	4,160	4,160	4,160	4,160
R-squared	0.238	0.239	0.242	0.276	0.213
Controls	NO	NO	NO	YES	NO
Sequence FE	NO	YES	YES	YES	YES
Round FE	NO	NO	YES	YES	YES
Individual FE	NO	NO	NO	NO	YES

Notes: Bootstrapped standard errors clustered by session/mechanism in parentheses.
* Significant at the 10% level, ** Significant at the 5% level, *** Significant at the 1% level.

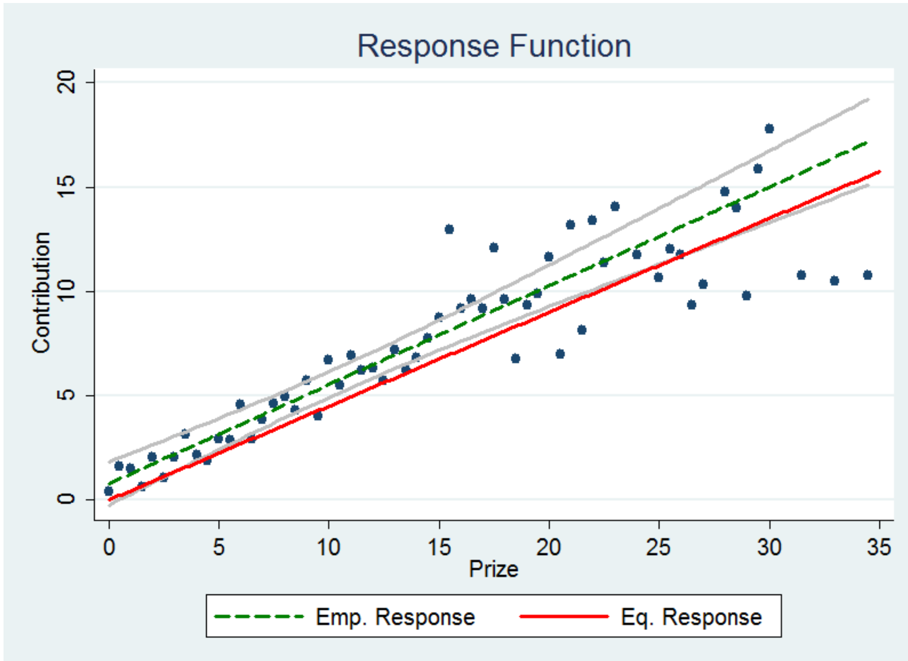

Figure 3.5: Contributions by Prize Amount

are also slightly more responsive to larger prizes. Individual characteristics such as gender, age, quiz results, and an indicator for having previously been a first-mover have some explanatory power. Column 3 reports the coefficients on these characteristics. Females and second-movers who had switched roles from the first to second sequence contribute significantly less across prize amounts. In addition, older subjects contribute slightly more. Column 4 includes interactions of these characteristics with the prize amount, allowing for heterogeneity in responsiveness to the prize amount as well. With this inclusion, the overall impact of these characteristics is diminished; the variation with respect to gender, age, and previous role appears to stem from differential responsiveness to higher prize amounts. Women and previous first-movers are not contributing less for all prize levels, but rather are less responsive to larger prizes than their complementary groups. This is particularly interesting as it pertains to female contributors, as it attests to gender differences in risk and social preferences in the vein of Croson and Gneezy (2009). As we can see in Figure 3.6, for small prize amounts women purchase roughly the same amount of lottery tickets as men. However, as prizes grow large, women increase their purchases of lottery tickets by substantially less than do men. This heterogeneity could be indicative of differential risk aversion as stakes grow large, as there is ample evidence that risk preferences differ by gender (Eckel and Grossman, 2008). In addition, if larger stakes lotteries are thought to be more competitive, women may participate less due to an overall aversion to competition (Niederle and Vesterlund, 2007). Alternatively, this could be an example of the sensitivity of women's social preferences to context. Specifically, previous studies have shown that female contributors intrinsic motivation to contribute to a public good may be crowded out by material incentives as the lottery stakes grow large (Mellström and Johannesson, 2008; Lacetera and Macis, 2010). Unfortunately, it is beyond the scope of this paper to differentiate between those competing explanations in this environment. This important question is left for future experiments designed specifically to answer such questions.

Returning attention to Table 3.7, column 5 includes individual specific fixed effects to account for individual level variation in overall willingness to contribute or buy lottery tickets. Their inclusion reduces the coefficient on prize amounts to almost precisely the coefficient of the equilibrium best response function.[15] As a result, it seems like contributors are responding to larger prizes in accordance with theoretical predictions, but with individual heterogeneity in tastes for lottery participation or public good contribution.

In almost all laboratory public goods experiments, contributions decrease over rounds within a session in spite of a fixed incentive to contribute. In this environment, however, the incentive to contribute is varying across rounds within a session. As such, there are two senses in which we could observe this tendency to contribute less over time: contributors

[15] Results from a random effects model are almost identical.

Figure 3.6: Contributions by Prize Amount by Gender

could become less responsive to prize amounts over time or contributors could reduce the component of contributions unrelated to prize amount in later rounds. To examine each of these notions, I first run a regression of total group contributions on prize amount interacted with time indicators. The coefficients from this regression will reveal any tendency to become less or more responsive to prize amounts over time. Second, I examine the component of contributions unrelated to prize amounts by ploting the residuals from this regression by round. These residuals will reveal any decay in unconditional contributions over rounds within a session. Figure 3.7 depicts the results of this analysis. Panel a) presents the coefficients on prize interacted with the current round out of 40 in the session while panel b) presents the residuals from the regression plotted by round.

Contributors seem to be highly responsive to prize amounts in the first sequence, as depicted in panel a). Over the first 20 rounds, coefficients on prize amounts are frequently above the equilibrium response of 1.8 By the second sequence contributors seem to have settled into the equilibrium response function, with coefficients hovering around 1.8 in most rounds. Panel b) clearly depicts a declining trend in unconditional contribution over the first sequence in a session, followed by a restart effect when the treatment change occurs. The over-emphasis on prize amounts in the first sequence described above makes it even more costly to contribute independent of the effect of the prize, hence the low and declining residuals over the first 20 rounds. After the restart effect, there is some fluctuation around zero, but no clear trend in the residuals, consistent with contributors converging towards the equilibrium best response function. The pattern is consistent with what has been seen in previous public goods experiments, with individuals contributing more than is predicted by equilibrium in the early rounds before settling in to the equilibrium strategy in the later rounds. The difference here is that the equilibrium strategy is not a fixed contribution amount, but rather a fixed response to the observed prize amount. Interestingly, the over-contribution in the early rounds manifests not as an unconditional upward shift in

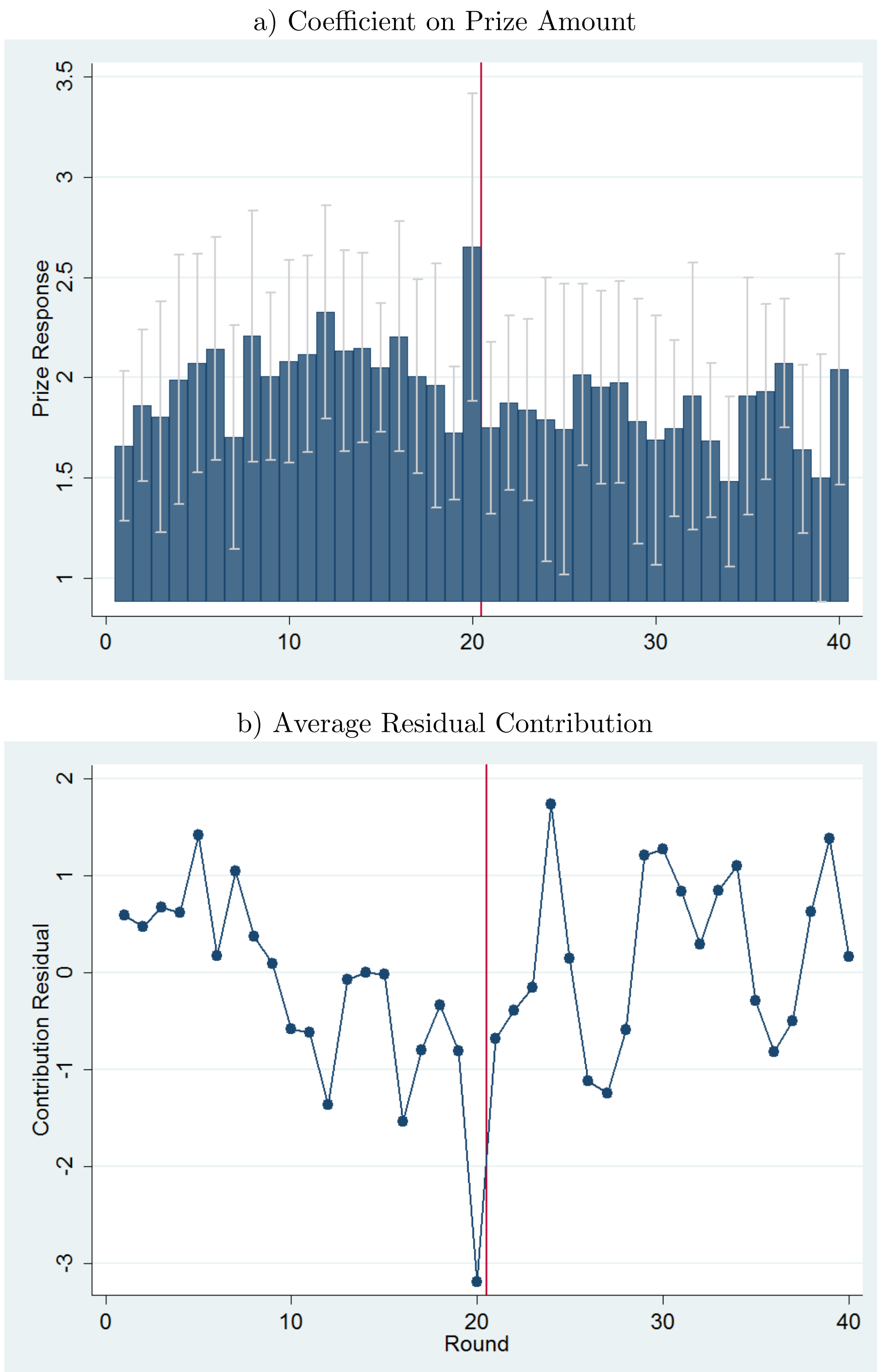

Figure 3.7: Regressions of Total Contributions on Prize Amounts by Round

contributions, but rather as an over-responsiveness to prize amounts.[16]

To sum up, it seems as though contributors in this environment are behaving closely in accordance with the equilibrium predictions. Contributions to the lottery increase with prize amounts offered at roughly the pace predicted by equilibrium. However, there seems to be a baseline preference for contribution that leads to contributions even when lottery prizes are small. This pattern is perhaps unsurprising given the frequent observance of positive contributions in standard voluntary contribution mechanisms. However, it has important implications for first-mover behavior, as I will discuss below.

3.4.3 EXPLAINING FIRST-MOVER BEHAVIOR

The overall results above depict a situation in which first-movers in fixed environments are consistently offering less to prize pools than the equilibrium predicts. This is borne out by an comparison of the distribution of first-mover offers to the distribution implied by the equilibrium under each mechanism. In the exclusive, fixed advertising environment, the equilibria imply a degenerate distribution of offers. However, the non-exclusive, fixed environment has a mixed strategy equilibrium that implies a specific distribution over offers. While it is unreasonable to expect subjects to derive and act in accordance with this function, a comparison of the empirical distribution with the theoretical mixed strategy provides further insight into how subjects are reacting to the various environments.

In the less competitive, fixed advertising environments, the observed distribution of offers is to the left of the equilibrium distribution in both the exclusive case and the non-exclusive case as seen in Figure 3.8, reflective of a general tendency to submit conservative offers in such environments. Neither distribution approaches the corresponding symmetric equilibrium strategy derived in Section 3; a Kolmogorov-Smirnov test rejects the hypotheses that either empirical distribution matches the equilibrium distribution at the 1% level. In both cases, there is a very small mass of offers located outside the support of the equi-

[16]Thanks to an anonymous referee for suggesting this addition.

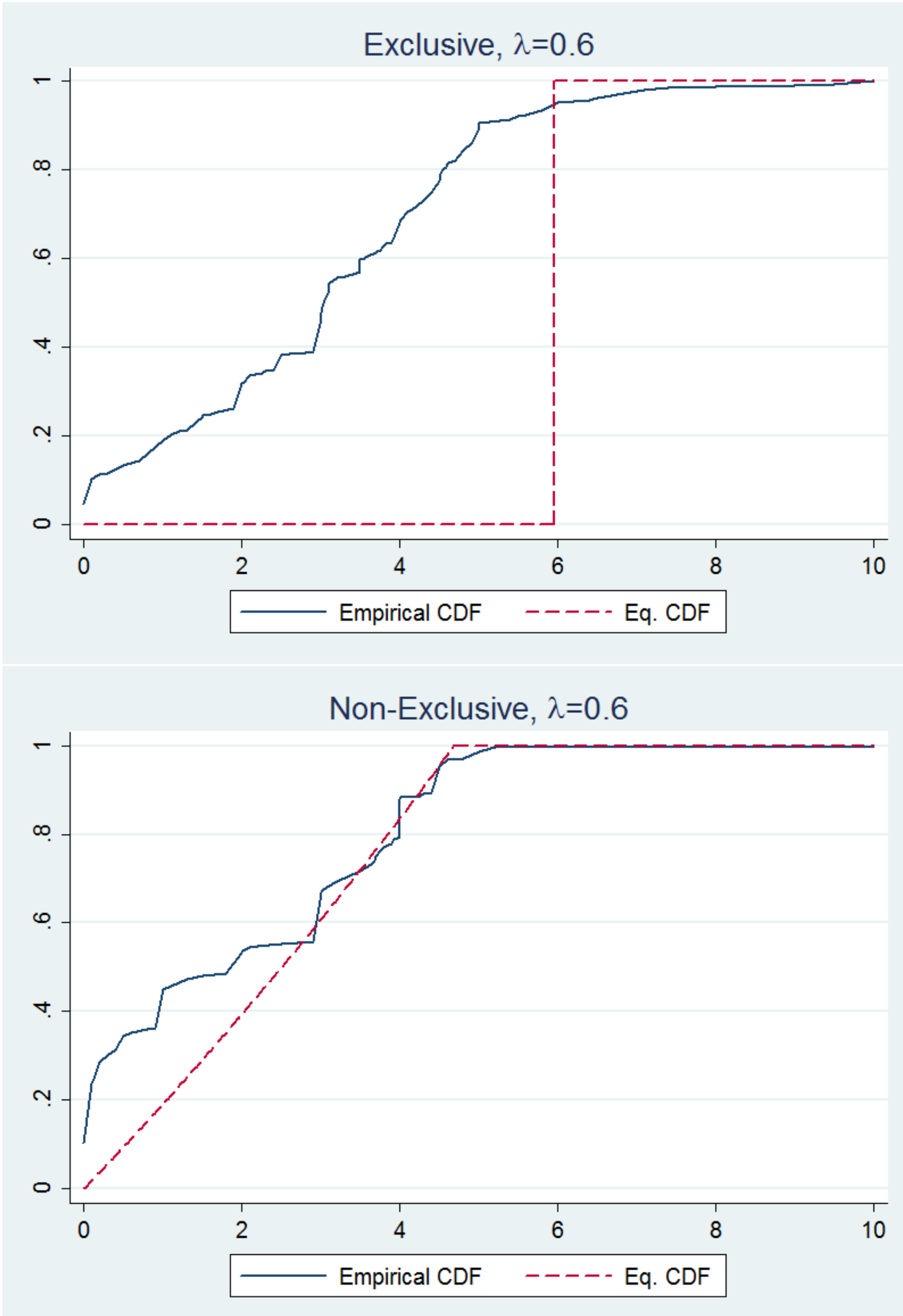

Figure 3.8: CDF of Offers ($\lambda = 0.6$)

librium distribution. Otherwise, the areas where the empirical distribution falls below the theoretical distribution could easily be due to the finite sample, indicating the possibility of first-order stochastic dominance in the population. As with the less competitive environment, first-movers remain conservative relative to equilibrium predictions when the sponsor's share is increased to 0.8, as seen in Figure 3.9. Again, both empirical distributions are clearly to situated to the left of the distributions implied by the symmetric equilibria, almost to the point of first-order stochastic dominance.

Clearly, in environments where the benefits of sponsorship are fixed in terms of share, first-movers are acting conservatively, making lower offers than predicted. This result may be unsurprising for those familiar with the experimental literature on Bertrand competition. Dufwenberg and Gneezy (2000) and Abrams et al. (2000) both observe significant price dispersion in experimental symmetric Bertrand duopoly markets. The environment here that most closely resembles Bertrand competition (exclusive, fixed) features persistent under-offering, which is analogous to price dispersion. This fact is particularly interesting when juxtaposed with the persistent experimental phenomenon of over-bidding in experimental common value all-pay auctions and other contests (Anderson et al., 1998; Gneezy and Smorodinsky, 2006; Lugovskyy et al., 2010; Sheremeta, 2013). The environment that incorporates features of an all-pay auction (non-exclusive, fixed) sees the mitigation of under-offering to an extent.

There are a number of possible explanations for this pattern. One possibility is that these low offers emerge as an empirical best response to observed second-mover actions. In the previous section, it was demonstrated that second-movers are systematically buying more lottery tickets and contributing more to the public good than predicted by equilibrium. This over-contribution would lead to a smaller overall market for the private good, decreasing incentives for firms to make aggressive prize offers.

Suppose that first-movers generate expectations about the contribution level of second-movers based on their previous experiences. In particular, they believe that second-mover

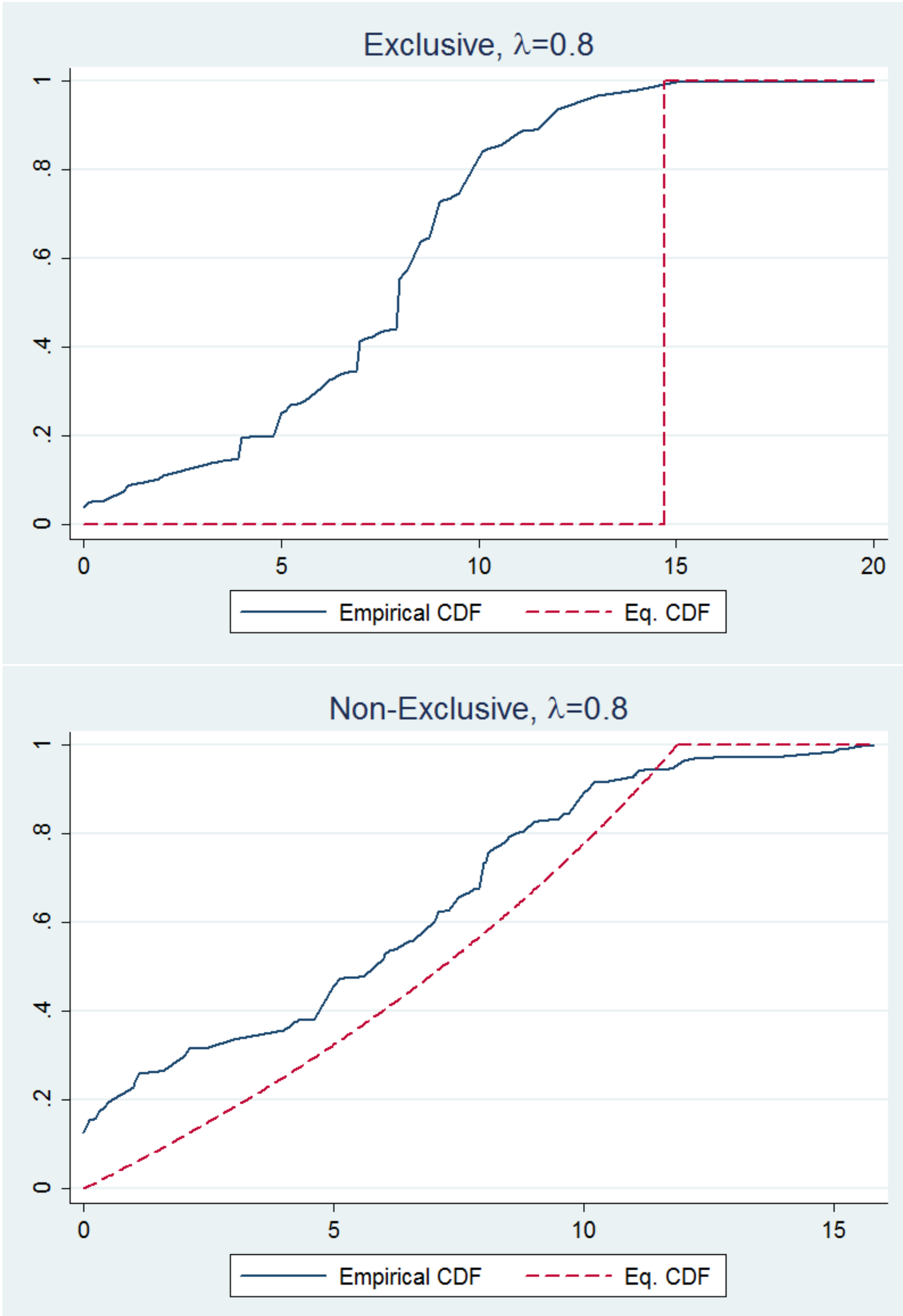

Figure 3.9: CDF of Offers ($\lambda = 0.8$)

contributions follow the simple model $g_i = \beta_0 + \beta_1 B_i$ where β_0 is indicative of some baseline preference for contribution independent of the prize amount, and $\beta_1 \geq 0$ is the individual's responsiveness to larger prizes. The equilibrium model of second-mover contributions dictates $\beta_0 = 0$ and $\beta_1 = 0.45$, and from that is derived the optimal prize offer. For a set of preferences that differs from the model's assumptions, however, these parameters are almost surely different, implying a different optimal offer. As a result, a first-mover draws inference on these parameters based on his past experience observing the joint distribution of contributions and prize amounts to establish his beliefs regarding β_0 and β_1.

This process is mimicked by estimating regressions of total contributions on prize amounts separately for each first-mover based on his or her history of observed outcomes. Taking as our sample decisions made and outcomes observed by the population of subjects who ever acted as first-movers, I run a separate regression for each individual. This regression uses all previously observed combinations of prize amounts and total contributions to estimate β_0 and β_1 as of their final decision making round as a first-mover. Roughly half of these individuals were first-movers in the first sequence, leaving 19 such observations. The remaining half of these subjects were first-movers in the second sequence. For these individuals, the observations of contributions and prizes from their first 20 rounds as second-movers are included in the regression leaving 39 total observations. These parameter estimates are then used to compute their best response to observed behavior in that round, assuming that the other first-mover shares the same beliefs and is behaving symmetrically. I then compare actual offers in the final round to these empirical best responses by means of a t-test. If deviations from equilibrium predictions arise from inaccurate modelling of second-mover responses instead of a failure of first-mover optimization, the difference between these two quantities should be statistically insignificant.

For treatments with fixed advertising, the results of this exercise reject the null hypothesis that actual offers are consistent with empirical best response in favor of the alternative hypothesis that actual offers are lower than empirical best response (p-value < 0.01 for exclusive, p-value < 0.10 for non-exclusive). The exercise is repeated using only second sequence first-movers to give this hypothesis its best chance. There are two reasons to expect that offers should be closer to best response for these individuals. First, the parameters β_0 and β_1 should be more precisely estimated with a larger sample of observed outcomes. Second, they will have had more time to learn the environment, albeit in a different context as a second-mover. Still, in round 40 the offers made by individuals in the exclusive treatments are significantly below best response levels (p-value <0.01). First-movers in non-exclusive treatments, however, are very close to best response given the behavior of second-mover. As a result, it does not appear that optimal response to observed behavior can explain the consistent under-offering of first-movers, at least in exclusive treatments.

Conservative offers could also be due to risk aversion. Subjects may prefer to limit their exposure to the uncertain behavior of second-movers by offering less to the prize and retaining the certain payoff from their endowments. There are two avenues by which I could explore this hypothesis. An ideal approach would be to correlate risk aversion at the individual level with lower offers. Unfortunately, I do not have measures of risk aversion for the participants in the experiment. Another approach is to correlate higher levels of perceived strategic uncertainty with lower offers. Unlike second-movers, who are subject to a truly random process (i.e. the lottery mechanism), if a first-mover can perfectly predict the behavior of her group members *ex ante*, there is no remaining uncertainty regarding her payoff for the round. So from the perspective of a first-mover, as observed behavior becomes less predictable, the variance of the return to making large prize offers increases. As a result, risk aversion should depress prize offers more for first-movers who have observed more variable behavior over the course of a session. I can capture the variability of both first- and second-mover behavior by using the histories of outcomes observed by first-movers. For a given first-mover, I summarize uncertainty with two statistics. The first is the coefficient of variation of the history of their fellow first-movers' prize offers. The second revisits the approach from above, estimating regressions of second-mover contributions on prize amounts and using a measure of the accuracy of the resulting model.[17] These two statistics should correlate well with perceived uncertainty. As a result, if offers are depressed by risk aversion, the statistics should in turn be correlated with lower offers.

Again, I restrict attention to decisions made and outcomes observed by the population of subjects who ever acted as first-movers. First, for the sequence in which they act as a first-mover, the coefficient of variation of the first 10 rounds of partners' offers is calculated. Second, as above I run a separate regression for each individual using all observed combinations of prize amounts and total contributions up to their 10th round acting as a first-mover. Instead of retaining the coefficients, however, we are interested in the accuracy of the linear model. Finally, I use these measures of strategic uncertainty as independent variables in a regression predicting subsequent offers through the last 10 rounds. These regressions, fitted separately for each environment, are displayed in Table 3.8.

For all of the fixed advertising environments, it seems that uncertainty in fellow first-mover behavior depresses offers. However, this effect is not statistically significant when allowing errors to be correlated across subjects' decisions except in the exclusive treatment with $\lambda = 0.6$. Also, in the non-exclusive environments, uncertainty about contributions given prizes seems to lower a first-mover's offer. Again, this strategic uncertainty effect is not statistically significant. As a result, it is difficult to attribute the gap between predicted offers and observed offers solely to risk aversion on the part of first-movers given the relative lack of response to increased risk among first-movers overall. The constants in these models essentially dictate the offers that would be made in an environment with no strategic uncertainty. For the exclusive environments, where prize offers are most conservative, these "no-risk" estimates remain well below the levels predicted by the symmetric equilibria.

Under-offering could also emerge from attempts at collusion. As in any other oligopoly environment, if firms here can commit to not competing on the dimension of prize offers,

[17] Specifically, the coefficient of variation of the root mean squared error of the linear regression.

Table 3.8: The Impact of Strategic Uncertainty on Offers

	Exclusive $\lambda = 0.6$	Exclusive $\lambda = 0.8$	Non-exclusive $\lambda = 0.6$	Non-exclusive $\lambda = 0.8$
	Offers	Offers	Offers	Offers
First-mover risk	-4.556*	-2.140	-1.877	-7.694
	(2.681)	(1.408)	(1.181)	(6.401)
Second-mover risk	1.221	0.827	-1.377	-3.528
	(1.844)	(4.200)	(2.062)	(8.643)
Constant	4.156***	8.691***	3.484***	9.562*
	(0.922)	(1.091)	(0.708)	(5.261)
Observations	160	200	160	200
R-squared	0.124	0.038	0.334	0.044

Notes: Bootstrapped standard errors clustered at the subject level in parentheses.

* Significant at the 10% level, ** Significant at the 5% level,

*** Significant at the 1% level.

they could feasibly maintain and share higher profits. With stable matchings, first-movers could potentially learn to cooperate and keep offers artificially low. However, this explanation is unlikely as the first-movers are randomly rematched every round and change roles after 20 rounds. As a result, there would be little time or opportunity for first-movers to establish the kinds of enforcement strategies that would be required to achieve the collusive outcome.

Future experimentation should focus on obtaining the data necessary to differentiate from amongst these competing explanations for conservative offers, especially in the exclusive environments. Obtaining measures of risk aversion could potentially establish the role of strategic uncertainty in the observed behavior. Removing subjects from the role of second-movers and opting instead for automated contributor behavior would further reduce perceived uncertainty. Finally, automating the behavior of one of the two first-movers in addition would eliminate collusion as an explanation for any persistent gap between observed and predicted offers.

3.5 CONCLUSION

There are any number of ways to go about obtaining donations for a fundraising lottery. A fundraiser must determine from whom to solicit donations and whether or not to promise exclusive sponsorship to these donors. She must also determine how best to reward these generous firms with her available advertising resources. When firms are motivated to donate to lotteries by advertising alone, these considerations are not trivial. In a simple, static environment with profit-maximizing firms, the optimal choice appears to depend on the capacity of the fundraiser to focus attention on one firm. If she can single out the most generous firm and plausibly promise that a large proportion of lottery participants will patronize that firm over a competitor, it seems that exclusivity in the form of accepting donations only from the most generous donor in a market will maximize contributions. If, however, she can not promise such a large advertising benefit, it is optimal to be non-exclusive and provide advertising in proportion to the generosity of each firm's donation.

Experimental evidence thus far indicates that, while contributors seem to increase contributions in response to larger endogenously determined prizes, individuals behaving as firms do not respond to contributor behavior in a way consistent with the equilibrium of a model of firm-sponsored fixed-prize lotteries. In particular, individuals faced with the firm's decision in this game seem to consistently offer less than the amounts predicted by the solution of the model when the benefits from sponsorship are fixed, but offer slightly more than they should when the benefits are proportional to their offer. As a result, prize amounts are generally lower than predicted in the experimental fixed advertising environments and higher than predicted in the proportional advertising environments. There are several possible explanations for this behavior. In the face of preferences that differ from those modeled, firms could simply be optimizing in the face of observed contributor behavior. Additionally, returns to firms could be more uncertain in fixed advertising environments than in environments where benefits are proportional to offers. This uncertainty could lead risk averse subjects to shade offers relative to a risk neutral equilibrium prediction, preferring to keep more from their certain endowment than leave themselves at the mercy of contributors decisions. Evidence from this experiment indicates that firms are not responding optimally to the observed contributor behavior. Further, contributions are not so different from equilibrium predictions as to prompt such a dramatic deviation even for a firm behaving optimally. While there is some evidence of risk aversion in fixed environments, the data collected do not speak directly to this question. As a result, further investigation into this environment is required to attribute the large unexplained gap between equilibrium prize amounts and observed firm prize offers.

In addition, there exist extensions to this environment. Perhaps most obvious is trying to pin down the nature of the rewards to sponsorship. Looking at a similar experimental environment, what happens when contributors are allowed to choose how to allocate their private good purchases between the two first-movers from which offers were solicited? More importantly, how does this response change with the information provided about the relative offers of the first-movers? If contributors are only told which first-mover made the higher offer, and not the amounts of each offer, what inferences are drawn about the relative sizes of the offers and how does that impact the private good purchase decision? Also, if there are multiple fundraisers with different causes and customers, how should each fundraiser go about soliciting donations from firms? Adding dynamics would be another logical step. There are many previous works about public goods with fixed cost components, or so-called provision point mechanisms, where sequential donations are important in securing socially beneficial outcomes. Examining a similar environment in a more generic fundraising campaign with a fixed cost component might shed light on how instrumental firms can be in such settings. This work is an initial step towards describing the environment in which firms and fundraisers interact. The ultimate goal is the development of a plausible model that can be used to inform the decisions of fundraisers in how to conduct their operations in such a way as to maximize their effectiveness.

APPENDIX A

PROMISE PROGRAMS

Table A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Arkadelphia Promise	Arkadelphia, AR	2010	• Graduate from Arkadelphia HS • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.
Baldwin Promise	Baldwin, MI	2010	• Reside within Baldwin Community SD • Graduate from any HS within zone • Continuous residency since 9th grade.	Sliding scale; $500 to $5,000 per year	Any accredited PSI in the Michigan

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Bay Commitment	Bay, MI	2006	• Graduate from Bay County HS • Continuous enrollment since 9th grade • Continuous residency for 6 years • First-generation college student	$2,000 per year	Delta College or Saginaw Valley State University

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
College Bound Scholarship Program	Hammond, IN	2006	• Continuous residency within Hammond City for 3 years • Graduate from any HS in Hammond City • 3.0 GPA OR • 2.5 GPA with 1000 SAT (math and verbal) OR • 2.5 GPA with 1400 SAT	Sliding scale; 60% to 100% of unmet need per year; Max: tuition at Indiana Univ. Bloomington.	Any accredited PSI in Indiana

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Denver Scholarship Foundation	Denver, CO	2006	• Graduate from Denver Public HS • Continuous enrollment since 9th grade • 2.0 GPA • Demonstrate financial need (EFC $<$ 2x Pell limit)	\$250 to \$3,400 per year depending on PSI and EFC	39 PSIs in Colorado
Detroit College Promise	Detroit, MI	2008	• Graduate from traditional Detroit Public HS • Continuous enrollment since 9th grade • Continuous residency since 9th grade	\$150 to \$600 for one semester	43 public PSIs in Michigan

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Educate and Grow Scholarship	Blountville, TN	Unknown	• Continuous residency within selected counties for 12 mos. prior to graduation • Graduate from any HS	Full tuition (4 semesters)	Northeast State Community College
El Dorado Promise	El Dorado, AR	2007	• Graduate from El Dorado Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade.	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Great River Promise	Phillips County, AR	2010	• Graduate from Arkansas or Phillips County HS • Continuous enrollment since 9th grade • Achieve high school attendance requirements.	Full tuition (4 semesters)	Phillips Community College of the University of Arkansas
Hopkinsville Rotary Scholars	Hopkinsville, KY	2005	• Graduate from selected high schools • 2.5 GPA • 95% attendance	Full tuition (4 semesters)	Hopkinsville Community College

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Jackson Legacy	Jackson County, MI	2006	• Graduate from Jackson County HS • Continuous enrollment since 10th grade • Community service	Sliding scale; $150 to $600 per year for two years	Jackson Community College, Spring Arbor University, Baker College of Jackson
Kalamazoo Promise	Kalamazoo, MI	2005	• Graduate from Kalamazoo Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade.	Sliding scale; 65% to 100% of tuition per year	Any public PSI in Michigan

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Legacy Scholars	Battle Creek, MI	2005	• Graduate from Battle Creek or Lakeview SD • Continuous enrollment since 10th grade	Sliding scale; 31 to 62 credit hours	Kellogg Community College
Leopard Challenge	Norphlet, AR	2007	• Graduate from Norphlet HS • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.25 GPA	Sliding scale; $2,600 to $4,000 per year	Any accredited PSI in the U.S.
Muskegon Opportunity	Muskegon, MI	2009[a]	TBD	TBD	TBD

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
New Haven Promise	New Haven, CT	2010	• Graduate from New Haven Public Schools • Reside in New Haven • 3.0 GPA • 90% attendance • Community service	Sliding scale; 65% to 100% of unmet need per year at public; Up to $2,500 at private	Any accredited PSI in Connecticut
Northport Promise	Northport, MI	2007	• Graduate from Northport HS • Continuous enrollment since 9th grade • Participate in fundraising activities	Sliding scale; Amount determined each year	Any public PSI in Michigan

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Peoria Promise	Peoria, IL	2008	• Graduate from public school in Peoria • Continuous enrollment since 10th grade • Continuous residency since 10th grade.	Sliding scale; 50% to 100% of tuition for up to 64 credit hours	Illinois Central College
Pittsburgh Promise	Pittsburgh, PA	2006	• Graduate from Pittsburgh Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.5 GPA • 90% attendance	Sliding scale; $1,000 to $10,000 per year	Any accredited PSI in Pennsylvania

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Promise for the Future	Pinal County, AZ	2001[b]	• Graduate from Pinal County HS • Continuous enrollment since 8th grade • 2.75 GPA	Full tuition (4 semesters)	Central Arizona College
Say Yes Buffalo	Buffalo, NY	2012	• Graduate from Buffalo Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade	Sliding scale; 65% to 100% unmet need	Any State University of New York or City University of New York campus.[c]

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Say Yes Syracuse	Syracuse, NY	2009	• Graduate from Syracuse Public Schools • Continuous enrollment since 10th grade • Continuous residency since 10th grade.	100% unmet need	Any State University of New York or City University of New York campus.[c]

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
School Counts Program	Hopkins County, KY	Unknown	• Graduate from Hopkins County HS in 8 consecutive semesters • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.5 GPA yearly • 95% attendance • Exceed graduation credit requirements.	$1,000 per semester for 4 semesters	Madisonville Community College

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Sparkman Promise	Sparkman, AR	2011	• Graduate from Sparkman Public Schools • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Ventura College Promise	Ventura County, CA	2006	• Graduate from Ventura County HS • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships	Enrollment costs for 1 year	Ventura College

Source: http://www.upjohn.org/Research/SpecialTopics/KalamazooPromise/PromiseTypeScholarshipPrograms, Gonzalez et al. (2011), and authors' research. Program details have changed over time; for brevity, all details reported represent current program configurations.

[a] Announced in 2009, but no details of eligibility or amount have been provided to date. Due to the high degree of uncertainty, was not included in analysis.

[b] While the Kalamazoo Promise is often referred to as the first in this class, we have found a source dating the start of the Promise for the Future back to 2001 ("Deadline to enroll in *Promise for the Future* Scholarship approaching" *The Superior Sun.* April 15, 2009.). Historical program details were not found during our research.

List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools

[c] There are other “Say Yes” partner schools, but additional restrictions apply.

APPENDIX B

EXCERPTS FROM FUNDRAISING WEBSITES

How to Get Businesses To Donate Items For A Raffle Fundraiser — eHow.com

"Raffles can be a great way to raise funds for a non profit group or project. Items for the raffle can usually be obtained free by asking area businesses for donations. Asking for donated items is a lot easier then asking for money because not only can you give the business free advertisement but the process is a lot easier for the business."

"A business will always be a business first when asking for donations for a raffle always let them know how them donating can help them. Also make sure to be able to represent why your project is needed and how the organization is going to make use of the raffles profits."

How to Ask a Business to Donate Merchandise for a Raffle — eHow.com

"Present your fundraising raffle to businesses as an inexpensive marketing and public relations opportunity. Invite interested business owners to make a donation of merchandise that will be included in the raffle. Advertising rates are expensive for every media outlet. Business owners are often looking for more cost-effective routes to get their name out in the community. Promote the raffle to make it profitable for everyone involved by advertising the businesses and the merchandise that will be given away."

"List all the different ways you plan to promote the raffle event and all the business that donate. Use solid numbers like the attendance of last year's raffle or in how many newspapers your ad will appear."

"Offer the business owner or marketing manager a chance to attend the raffle and further promote the business."

How to Get a Business to Give Donations to a Non Profit Organization — eHow.com

"Create fundraising opportunities that offer value to targeted businesses. Companies can benefit financially from public relations opportunities. Plan fundraising events that mesh well with the public relations goals of your target companies... Design event literature and advertising material to display the logos and information for your donating companies as a free advertising incentive."

How to Get Companies to Donate Free Products to Charity — eHow.com

"Type a letter to the first company on your list. Use flattery in explaining to the company's public relations manager how the product you'd [sic] wish to have donated would benefit the charity and why only their product would work best."

"Your letter should also explain how donating a product to your charity would benefit the company in the long run. Explain the advantages of free advertising by having the company's name in any of the charity's advertisements or fundraising activities. Explain that if the company donates an item, people will hear the company's name associated with a good cause."

"[Solicit from] every company on your list until multiple companies donate to your cause."

Raffle Fundraising Ideas — eHow.com

"You may even end up with more than one , which will provide an opportunity to sell raffle tickets at varying prices depending on the worth of the prize. Most businesses will be happy to oblige, as it also provides them with low-cost advertising."

www.better-fundraising-ideas.com

"Companies may choose to be involved with your organisation because -

- They like to be viewed as good citizens.
- Being associated with certain causes can boost their image within their target markets.
- There might be Public Relations opportunities to be had.
- Their employees want them to. Company involvement in good causes can aid staff satisfaction, recruitment and retention"

"All companies are however most interested in their bottom line. Working with you must deliver some benefit. Trying to understand things from a company's viewpoint is an important part of seeking a company donation"

"Nearly all company donations of whatever kind are made either locally in the community in which they are operating, or to organisations working within the same area of interest such as a surf-wear company supporting beach clean up campaigners"

"[Online auctions] can also help with requests for suitable donations. Many companies will be far more likely to 'stump up' if they know their gift will be presented on a national or international platform . Indeed many companies will use charitable donations as a marketing tool to generate a buzz about certain products, for example tickets to Film Premieres."

APPENDIX C

EXPERIMENTAL INSTRUCTIONS

C.1 INTRODUCTION

This experiment is a study of decision-making. Your earnings will depend on the decisions that you and others make during the experiment. At the end of the experiment, you will be paid a $5 participation fee plus whatever you earn during the course of the experiment. Payments will be made privately and in cash. Please do not talk to other participants during the experiment. If at any point you have a question, raise your hand and an experimenter will answer your question in private.

The experiment will consist of two sequences of 20 rounds for a total of 40 rounds. In each round, you will be randomly assigned to a group with other participants in the room. All decisions are made anonymously. You will never be able to identify your group members, nor will they be able to identify you.

There are two types of participants in this experiment: "first-movers" and "second-movers". You will be randomly assigned to one of these roles at the beginning of each 20 round sequence and will remain in that role for the entire sequence. If you are selected as a first-mover in the first sequence, you will be ineligible to be a first-mover in the subsequent sequence. In each round you will be randomly assigned to a group with five other participants. The group of six consists of **two** first-movers and **four** second-movers.

The decisions that both first-movers and second-movers will be asked to make in each round will not vary over the course of the experiment. However, the way in which these decisions affect your earnings will change in the second sequence. At the end of the first sequence of 20 rounds, we will pause briefly to hand out and read a new set of instructions which will apply to the second 20 round sequence.

At the end of the experiment, one round from each sequence of 20 rounds will be randomly selected for payment. Your earnings in points from these randomly selected rounds will be converted into cash at a rate of $0.30 per point.

C.2 DECISIONS

Each participant receives an endowment of tokens at the beginning of each round. Each round then proceeds in two stages. In stage 1, the first-movers make decisions; in stage 2, the second-movers make decisions. We will review each of these decisions in detail later on.

Stage 1: In this stage, each first-mover will declare how much from their initial endowment of **20 tokens** they wish to offer to a lottery prize which will be available to the second-movers in the group.

Stage 2: In this stage, each second-mover will allocate an endowment of **25 tokens** between purchases of lottery tickets and investments in a private account. Purchases of lottery tickets will increase the chances that each second-mover has of winning the prize, but tokens spent in this way will also be added to a group account which benefits all of the second-movers in the group. Tokens invested in the private account will be converted into point earnings only for the second-mover making the investment.

These decisions will determine your earnings in points for the round, which will be described in detail below.

- The earnings of each first-mover will depend on the total amount of tokens allocated to private accounts by all second-movers in their group, the first-mover's own offer to the prize fund, and whether the offer was greater than the other first-mover's offer.
- The earnings of each second-mover will depend on the allocation decisions made by all second-movers in their group between purchases of lottery tickets and investments in the private account.

We will now review these decisions in order, starting with the first-movers' decisions in stage 1.

C.2.1 Stage 1: First-mover decisions

At the start of each round, each first-mover will be granted an endowment of **20 tokens**. Each first-mover must then decide on an amount of tokens to offer towards the lottery prize available to the second-movers.

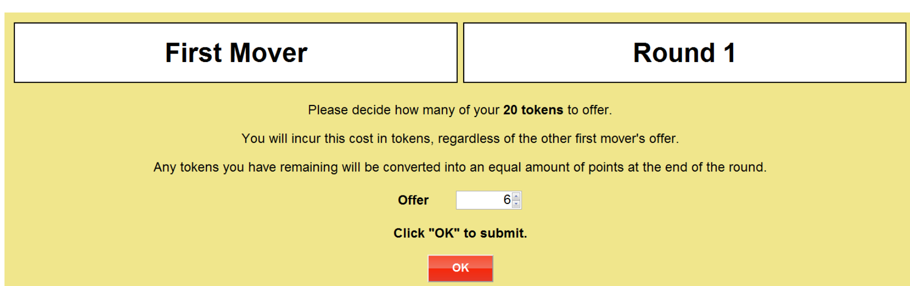

Figure C1: First-mover Decision Screen

- The sum of the two offers— one offer from each first-mover— will be revealed to all second-movers in the group as the lottery prize.

- After the second-movers have made their decisions, 1/3 of the total tokens allocated by all four second-movers to their **private** accounts will be **shared** between the first-movers.

- The first-mover that made the higher offer to the lottery prize will receive a 4/5 share of the total amount (1/3 of all **private** account investments). The first-mover that made the lower prize offer will receive the remaining 1/5 share of the total amount.

- In addition, both first-movers will have their own prize offer deducted from their initial endowment, with any remaining tokens converted one-for-one into points.

As a result, the point earnings of the first-mover that made the higher offer are

$$(20 - \text{Prize Offer}) + 4/5 \times (1/3 \times \text{All Tokens in Private Accounts})$$

The point earnings of the first-mover that made the lower offer are

$$(20 - \text{Prize Offer}) + 1/5 \times (1/3 \times \text{All Tokens in Private Accounts})$$

In the event of two identical prize offers, one of the first-movers will be selected at random with each receiving equal probability— as if by a coin flip— and his or her offer will be treated as the higher offer for the purposes of dividing the total available points. If both first-movers offer zero tokens, they each receive half of the total amount of available points.

In summary:

- The two first-movers share an amount of points equal to 1/3 of the total tokens allocated by all four second-movers to their **private** accounts.
- You receive a 4/5 share of these points if your offer was larger than the other first-mover in your group and a 1/5 share of these points otherwise.
- Larger prize offers both reduce the amount of points retained from your initial endowment and also might make purchases of lottery tickets more attractive, thus reducing investments by each second-mover in their **private** accounts.

C.2.2 Stage 2: Second-mover decisions

After the first-movers in the group have made their decisions, the second-movers will observe a lottery prize amount that is equal to the sum of the two offers made by first-movers in the preceding stage. Each must then decide how much from their 25 token endowment to spend on purchases of lottery tickets, with any remaining tokens being invested in their private account.

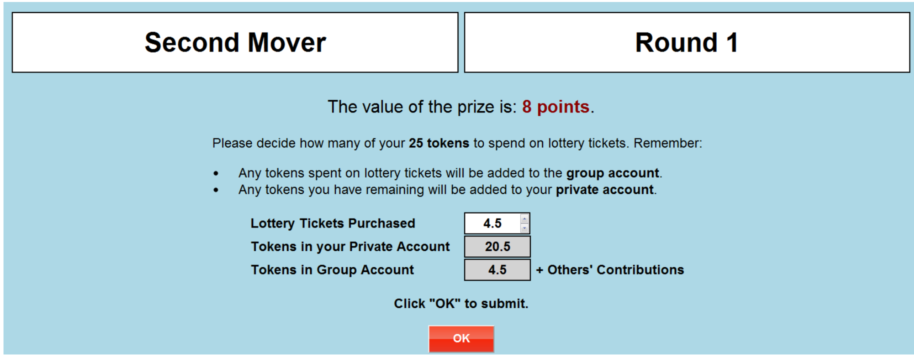

Figure C2: Second-mover Decision Screen

- The winner of the prize is randomly selected from those individuals purchasing lottery tickets. The probability that a given second-mover wins the prize is equal to the ratio of the number of tickets they purchased (1 ticket per token spent) to the total number of tickets purchased by all four second-movers. If every second-mover in your group spent one token and purchased one ticket, there would be four tickets in total and each second-mover would have a 1 in 4 chance of winning the prize.
- Any tokens used to purchase lottery tickets are placed in a group account. **All** second-movers in a group, those who purchase tickets **and** those who do not, earn additional points equal to 1/4 the number of tokens in the group account at the end of the round. *Note: This is true even if both first-movers make offers of zero.*
- Any remaining tokens will be invested in the private account. Each second-mover will earn additional points equal to 2/3 of the number of tokens they allocated to their own private account.

As a result, each second-mover's earnings in points represents the sum of three numbers:

$$\underbrace{1/4 \times \text{Tokens in Group Account}}_{(1)} + \underbrace{2/3 \times \text{Tokens in your Private Account}}_{(2)} + \underbrace{\text{Prize Winnings if Group's Winner}}_{(3)}$$

In summary:

- Purchasing lottery tickets increases your own chance of winning the prize, if one is available, and also earns each second-mover in your group, including you, an additional 1/4 of a point per token spent.
- Any remaining tokens are invested in a private account, earning you alone an additional 2/3 of a point per token invested.

C.3 EXAMPLE

Please direct your attention to the screen for an example of a typical round that you may experience. The numbers in this example are illustrative and may be different from what actually occurs during the course of the session. The example is designed to help you understand the impact on earnings of the decisions made by you and your group members. Use the questions provided below to follow along with the example.

1. Which first-mover will obtain the larger 4/5 share of total points?
2. How many tokens from first-mover 1's endowment will be converted into points at the end of the round?
3. How many tokens from first-mover 2's endowment will be converted into points at the end of the round?

If the three other second-movers act as depicted as well,

4. What is the probability that the second-mover shown will win the prize?
5. How many tokens will be in the **group account** at the end of the round?
6. How many points will the second-movers receive from the group account?
7. How many points will each second-mover receive from their private accounts?

8. How many points will the **first-movers** share from investments in the private account?

C.4 QUESTIONS?

Now is the time for questions. If you have any questions, please raise your hand and an experimenter will come answer your question in private. Are there are any questions before we begin?

C.5 QUIZ

Before we begin, we ask that you first take a brief quiz to test your understanding of the decisions you will make over the course of the session. Your responses on this quiz will have no impact on your earnings today. As in the example, the numbers in these questions are illustrative and may be different from what actually occurs during the session. When you are finished, please detach this sheet and place it on the shelf above your workstation.

1. Suppose first-mover 1 offers **3 tokens** and first-mover 2 offers **6 tokens**. First-mover _____ will obtain the larger 4/5 share of total points made available from investments in second-movers' **private accounts**. First-mover _____ will obtain the remaining 1/5 share. At the end of the round, _____ tokens from first-mover 1's endowment and _____ tokens from first-mover 2's endowment will be converted into points.
2. In the second stage second-mover 1 spends **4 tokens** on lottery tickets and the remaining 3 second-movers spend **8 tokens each** on lottery tickets. As a result, second-mover 1 will have _____ tokens in his or her **private account** and there will be _____ tokens in the **group account**. Second-mover 1 will earn _____ points from his or her **private account** and **all** second-movers will earn _____ points from the **group account**. In addition, second-mover 1 will have a _____ chance of winning the prize.
3. Suppose first-mover 1 offers **4 tokens** and first-mover 2 offers **1 token**. First-mover

_____ will obtain the larger 4/5 share of total points made available from investments in second-movers' **private accounts**. First-mover _____ will obtain the remaining 1/5 share. At the end of the round, _____ tokens from first-mover 1's endowment and _____ tokens from first-mover 2's endowment will be converted into points.

4. In the second stage second-mover 1 spends **1 token** on lottery tickets and the remaining 3 second-movers spend **5 tokens each** on lottery tickets. As a result, second-mover 1 will have _____ tokens in his or her **private account** and there will be _____ tokens in the **group account**. Second-mover 1 will earn _____ points from his or her **private account** and **all** second-movers will earn _____ points from the **group account**. In addition, second-mover 1 will have a _____ chance of winning the prize.
5. True or False: A participant can be a first-mover in both 20 round sequences in today's session.
6. True or False: As a first-mover, any amount you offer in the first-stage— regardless of the offer made by the other first-mover— will be deducted from your 20 token endowment before it is converted into points at the end of the round.
7. True or False: As a second-mover, you earn points from others' contributions to the group account even if you did not purchase any lottery tickets.
8. True or False: You will be paid your earnings in exactly two randomly selected rounds in today's session.

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