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UNIVERSITY OF CALIFORNIA,
IRVINE

Essays in Crime and Behavior

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Andrew Joseph Porter

Dissertation Committee:
Professor Marianne Bitler, Chair
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2015

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DEDICATION

to my wonderful wife

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ABSTRACT OF THE DISSERTATION

Essays in Crime and Behavior

By

Andrew Joseph Porter

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Professor Marianne Bitler, Chair

This dissertation is composed of three chapters that contribute to the fields of Public Economics, Law and Economics, Labor Economics, and Behavioral Economics.

In chapter one, I show that public institutions such as libraries, schools, and shelters create crime spillover effects by attracting victims, witnesses and incapacitating criminals. This paper is the first to estimate the causal impact of public institutions on local crime rates. These crime effects are identified by exploiting exogenous variation in the hours of the Los Angeles Public Library. I find increasing hours of public institutions decreases non-gang related aggravated assault rates and car burglary rates nearby. Public institutions reduce crime by attracting patrons that act as witnesses against car burglary, and by keeping potential aggravated assault victims or aggressors occupied in a safe environment.

In chapter two, I exploited variation in the Google Fiber internet roll out in Kansas City, Kansas and Missouri to estimate the impact of internet availability on crime. Google Fiber preregistration rates by neighborhood were used as a proxy of demand for internet services; a novel control in studies of internet effects. Young men commit crimes and are victimized at high rates compared to other demographic groups. If the internet is an attractive use of time for young men, the opportunity cost of criminally risky activities would be higher for victims and criminals, which could cause a decrease in the crime rate. No effects of increased internet availability on crime were found. This finding may suggest a temporal displacement of crime from increased internet availability.

In chapter three, I present experimental results examining valuations of prizes in different contexts: winners of solo and group competitions, the losers of these competitions, and random assignment. Subjects are found to expend the same effort on average in the group and solo competitions. Relative to random assignment, I find prize valuations rise for winners and fall for losers once competition is introduced. Competition losers value the prize the lowest, nearly fifty percent of its retail value, compared to ninety percent for winners. Within competitions, I find no effects of group versus solo structure on valuation.

Chapter 1

Libraries Fighting Crime: Spillover Effects of Public Institutions

1.1. Introduction

Public brick and mortar local institutions[1] such as libraries, schools, homeless shelters, and parks are important establishments in communities. When open, these are places where people congregate and spend time. While stated missions of these public institutions might concern literacy, homelessness, and education, among others, these public institutions may also have crime spillover effects. As crime requires victims as well as perpetrators, at any place and time where individuals congregate, crime is possibly affected. Public institutions can affect the supply of crime opportunities by attracting potential victims, and simultaneously decrease crime by attracting potential witnesses. Public institutions can incapacitate potential criminals who are utilizing the public institution's services indoors. In addition, potential victims may be safer inside the public institution while at risk outside on the street. Understanding how crime responds to the opening and closing of public institutions has a broad potential impact, as these types of institutions are common in all cities.

Institutions can affect crime in three different ways. By creating a free public space, people from all income levels can come and potentially utilize the institution. By spending time within the institution instead of on the street, a person will be at less risk of getting arrested or

[1] The word "institutions" has many meanings to many economists. In this paper I refer to public institutions as physical locations that are open to the public or a relevant demographic slice of the public, where individuals come to spend time, such as libraries, schools, parks and shelters.

becoming a victim of a crime. In addition, these institutions provide "eyes upon the street" when people attend. The appearance of visitors is a deterrent to potential criminals, as the person can act as a witness and report the crime to the police and potentially identify them in a lineup. Adversely, institutions also provide a boon to criminals by attracting suitable targets for crime. Every time someone new enters the institution or is in the nearby area there is an additional person who can be victimized. These three mechanisms by which institutions can influence crime also interact in complex ways, making it necessary to consider all simultaneously and their combined impact on different types of crime when determining the spillover effects of public institutions on crime.

Testing the causal effects of any observational treatment is difficult as exogenous variation in general is rare, including exogenous variation in the availability of public institutions. In this paper I exploit exogenous variation in the opening hours of branches of the Los Angeles Public Library during a period when the system experienced a dramatic change in hours. During the Great Recession, the Los Angeles Public Library's budget was cut and later partially restored. These budget cuts led to cuts in hours which varied by the day of the week on which they occurred, the library types to which they were applied, the dates on which they took effect, and the number of hours by which they cut library service. This exogenous variation represents a unique opportunity to empirically test the impact of libraries open hours on crime and uncover how opening and closing public institutions alters the criminal landscape.

Libraries are an ideal institution to test the effects of because they are enjoyed by the wealthy and the poor, college graduates and those studying for general educational development (GED) tests. However, the library system is much more important to the disadvantaged (Zickuhr Rainie Purcell 2013). Those without internet or computer access can use the library's free

internet and computers to apply for jobs, connect with friends and family, read the news, enjoy entertainment, and much more. Those without shelter can find a place to sit and read. Those struggling with literacy can attend adult education classes. The library can act as a safe haven, a compelling place to spend instead of possible worse alternatives outside on the street. The traditionally disadvantaged use the library more than others, and simultaneously are much more at risk of becoming victimized or arrested (Zickuhr Rainie Purcell 2013). This makes libraries an especially salient type of public institution to estimate the impact of crime.

In the following paper I show evidence of the impact of hours public institutions are available on local crime rates. To accomplish this I utilizes exogenous variation in the hours the Los Angeles Public Library system is open. First, I review the context of the Los Angeles Library system hours during the Great Recession, discussing the three mechanisms by which local institutions can affect crime rates. Next, I describe the crime and library data and my unique library map construction. Following this, I explain my empirical model which utilizes a difference in differences empirical strategy and present my results and conclusions. I find statistically significant and negative effects of library hours on car burglary and non-gang aggravated assault near libraries, and find evidence that implies substitution from car burglary into residential and business burglary.

1.2. Los Angeles Public Library During the Great Recession

The Los Angeles Public Library, first established in 1872, includes 73 branches with a collection of over six million volumes (American Library Association 2012) which serves 3.8 million residents of the city of Los Angeles. Among public libraries in the United States and according to the American Library Association in 2012, the Los Angeles Public library serves

the most residents, has the second most total visits, fourth most holdings (print materials, electronic books, audio and video,) and is twelfth in total circulation.[2] In the 2010-2011 fiscal year, the Los Angeles Public Library system had 12.5 million visits, 1.2 million borrowers of books, and 3.4 million users of internet connected computers (California State Library Statistics 2012). The Los Angeles Public Library is a widely used library system that affects a great number of people. If the Los Angeles Library system were smaller, the effects of changes in hours on crime might be harder to observe. This contributes to its salience for use in this quasi-random experiment in public institution availability.

During the Great Recession of 2009, low tax revenues hit California and Los Angeles particularly hard. The city faced a budget shortfall of $212 million during the 2009-2010 fiscal year and a $485 million shortfall during the 2010-2011 fiscal year.[3] In response to this, the library's total expenditures were cut by 15 percent from 2009 through 2011 (California Library Statistics 2012). Most of the impact of these cuts was felt in total hours library branches were open, which fell during this time period by 25 percent. On various dates from 2009 through 2010, library administrators eliminated service on Sunday and Monday completely, as well as shortening hours on the rest of the days of the week. Monday service was partially restored in the summer of 2011 (Los Angeles Times 2011).

In response to cuts to the library hours, fewer people visited the library and utilized its services. Initially, library administrators were hopeful patrons would shift their library usage to other, open days of the week[4] resulting in the same total number of visitors. Unfortunately for the

[2] The New York Public Library is first in holdings, visits, circulation, and 2nd in population served. However, the County of Los Angeles Public Library is a separate entity from the Los Angeles Public Library. The county library is large, albeit smaller than the city library.
[3] From the City of Los Angeles Budget Summary, 2010-2011 and the minutes of the Board of Library Commissioners, City of Los Angeles, March 25, 2010 and April 15, 2010.
[4] Minutes of the Board of Library Commissioners, City of Los Angeles, July 29, 2010.

library, as hours fell by 25 percent during the 2009 through 2011 period, visits to the Los Angeles Public Library fell by 28 percent as well (California State Library Statistics 2012). Circulation, which is a measure of the number of checked out volumes in the library system, also fell by 20 percent. As the 20 percent reduction in circulation is less than both the number of hours and the number of visits percent change, the most devoted library patrons may have decreased the number of visits but partially shifted the days and times when they check out books.

Many individuals use the library for the free public computers. A Pew survey found that 26 percent of Americans age 16 and older used the internet, the computers, or free Wi-Fi at a local library (Zickuhr, Rainie, Purcell 2013). In a Pew Research study released in 2013, computer use was found to be a common and expected use of public libraries. The Pew study reported 26 percent of Americans age 16 and older used the internet at the library through the computers or free Wi-Fi. The usage of the library's internet-connected computers fell during the same period by 33 percent. This means individuals who used the library's free computers did not, or were not able to, shift their usual computer time onto other days by a greater amount than the hours cut. Since the rates of computers used, circulation, hours, and visits are largely in the same ballpark, we can conclude the cuts in library hours adversely affected how many individuals were coming to and utilizing the library.

1.3. How Libraries and Institutions Affect Crime Rates

In this section, I describe the three theoretical mechanisms by which libraries and comparable institutions can influence crime rates: the social services theory, the eyes on the street theory, and the suitable targets mechanisms are all derived from routine activities theory

(Cohen, Felson 1979). Each mechanism offers a compelling theoretical argument for how and why crime should be affected by the presence of open institutions such as libraries. All three mechanisms happen simultaneously and are directly intertwined to the costs and benefits of public institutions. However, as I detail in the results section, different crimes are affected by different mechanisms.

Each of the three mechanisms are also related to the rational criminal model. As criminals have been shown to respond to incentives, the rational criminal model developed by Becker is an important tool for understanding both criminal behavior as well as what strategies might mitigate the occurrence of potential crimes. Becker (1968) developed the rational model of crime: depicting a criminal deciding whether to commit a crime given his available opportunities. Researchers have been utilizing the insights of the Becker rational criminal model of crime (Sjoquist 1973; Dills 2009; Melkhop 2010) to better understand how to deter criminal behavior. In a simple version of this model, a criminal decides to commit a crime if the net expected benefits outweighs their reservation wage or opportunity cost. Traditionally, the opportunity cost of committing a crime has been studied in a labor economics context (Cornwell 1994; Donahue 2001; Gould 2001; Raphael 2001; Rege 2009; Phillips 2012), investigating a weak but significant link between unemployment, wages and crime. In the following section I discuss the opportunity cost of crime in the context of local institutions such as libraries, as well as in the potential choices a victim or criminal might make.

Alternatively, private businesses could provide many of the features public institutions do. Private businesses such as restaurants or coffee shops can house people indoors instead of potentially risky areas outside, and attract eyes on the street as well as suitable targets. This is supported by research by Jacobson and Chang (2014) who found that on days when medical

marijuana dispensaries were closed, nearby car burglaries increased. However, while private business are still an interesting and worthwhile avenue for research, their potential impact on all types of crime is limited by the business they generate, which is from paying customers. By focusing this research on public institutions, I am able to consider the most criminally risky segment of the population, the poor, who may not be able to afford entry into private businesses.

1.3.1. The Social Services Mechanism

Libraries are popular institutions many different kinds of people enjoy. However, when these institutions are removed, low income individuals are the worst off. This insight is supported by the results of a Pew survey on library usage, which found Blacks, Hispanics and low income individuals are 50 to 100 percent more likely to say library services are "very important" for almost all of library services (Zickuhr Kathryn Rainie 2013). Library services include quiet spaces, internet and computers, youth programs, job search assistance, and government services help.[5] In a library, a low income individual can use free services they may not have access to at home. The time spent inside the library is time not spend in a criminally risky situation.

In addition to utilizing library services more, the poor and racial and ethnic minorities suffer disproportionately from the effects of crime. The rape, robbery and aggravated assault victimization rate is 169 percent higher for blacks than whites and the overall arrest rate is 241 percent higher for blacks than whites. While the burglary victimization rate for individuals making less than $7,500 a year is 213 percent higher than those making more than $75,000 (BJS

[5] Books and media had the most similar response rate across groups, though Blacks, Hispanics and low income individuals still had the highest "very important" response rate (Zickuhr, Kathryn; Rainie 2013).

2010; FBI 2012; Census 2012). Low income and disadvantaged individuals may also commit more crimes due to peer and network effects as well, an individual may commit more crimes if his neighbors are committing crimes (Glaeser et al. 1995; Freeman 1999).

Therefore, the same groups that most demand library services would likely be hurt the most should library hours be cut. These groups will be forced into alternative activities during the day that may be more criminally risky than before. They may be more at risk as potential victims and be less occupied turning into potential criminals. Parents who would have dropped their teenagers off at the library if they could not afford a babysitter will be out of an option. Recently paroled felons who are looking for work and need assistance looking will have to go elsewhere. Students who don't have a quiet space to study and the elderly who don't have a sheltered place to use a computer or read will have fewer options than before. If these patrons spend more time in bad neighborhoods or out on the street, crime may increase as a result of the cuts to library hours.

The social services mechanism draws on findings from the incapacitation literature, showing the less time a potential criminal has available due to being otherwise busy or incapacitated, the fewer crimes are committed. Traditionally, this literature has shown increasing prison sentences decreases crime rates (Ehrlich 1972; Levitt 2007). By having individuals stay in prison longer after committing a crime, future crimes are reduced due to the most likely culprits are already in prison. By removing them from the pool of available criminals, law enforcement effectively lowers crime. In non-prison setting, Dahl and Della Vigna (2009) found the release of violent movies reduced assaults that same night and weekend. The authors argue this reduction is due to potential violent criminals enjoying violent movies instead of drinking alcohol at nearby bars and committing assault. This mechanism applies to potential victims of crimes as well. By

victims self-selecting into a relatively protective place such as a library, they remove themselves from the pool of potential victims. If criminals are incapacitated or victims are in a safe haven, both the supply, the pool of criminals, and the demand, the pool of victims, of crime are reduced resulting in less crimes committed.

Free internet and computers are a strong draw for visitors to the library (Zickuhr Rainie Purcell 2013). One possible use of the internet is to look for employment. Kuhn and Skuterud (2004) find internet job searchers become employed faster during unemployment spells. However, once controlling for individual characteristics the gains from internet searching is eliminated (Kuhn Skuterud 2004). Stevenson (2008) finds labor search is more efficient since the advent of the internet. Employees who have jobs are more likely to pursue better job opportunities elsewhere, and the unemployed are more likely to find jobs that best suit their skills. In addition to labor, the internet can substantially decrease consumer search costs. Brown (2002) found internet use decreased costs of life-insurance plans due to increased price competition among providers. As individuals are better able to find jobs with internet access, access to libraries may increase the probability of finding a job, therefore increasing the opportunity cost of crime. Individuals can also use the computers to connect with family and loved ones or solely for entertainment purposes. If the individual is spending time with the computer indoors that person is effectively removed from both the pool of potential victims and potential criminals,[6] thereby lowering the number of crimes committed.

[6] Computer-driven crime such as identify theft and scamming are a large problem worldwide and could be facilitated by a library's provision of free computers and internet. However according to the LAPD incident narratives from 2009 to 2011 the number of reported crimes involving a stolen identity was 15 total, out of almost 300,000. This may be underestimating the true stolen identity total as the data does not list if the victim's identity was stolen, only the narrative.

1.3.2. Eyes Upon the Street or Potential Guardians Mechanism

As more individuals gather together, there is a greater likelihood someone will see a crime being committed, interfere with the criminal, take photos or call the police. This is known as "eyes upon the street" or "potential guardians" in the routine activities criminology literature (Cohen Felson 1979). Chang and Jacobson (2014) found after marijuana dispensaries were unexpectedly shut down, effectively closing these businesses, nearby burglaries from vehicles went up substantially and decreased in distances further away from the dispensary (Chang Jacobson 2014). The authors believe the most likely reason responsible for the crime increase was due to the traffic coming in and out of the dispensary.

If there are many "eyes upon the street" present while a criminal considers committing a crime, he may be discouraged. They become discouraged because the presence of people nearby increase the chance of being caught. Individuals nearby can see the crime committed and become a witness, or potentially break up a crime in progress. If caught and convicted, the eventual punishments (fines, jail time, and future lack of employment) are in the future and will be discounted much more heavily in any cost benefit calculation than the probability of apprehension, which occurs in the present and thus is not discounted. This suggests for crime reduction and policing purposes, increasing the number of "eyes upon the street" and thereby increasing the perceived chance of getting caught could be more effective at preventing crime than other methods such as increasing sentencing. This is supported by research finding the probability of arrest is a much more powerful predictor of reductions in crime than the severity of punishment (Davis 1988; Trumbull 1989). This literature informs us by having an institution open, the visitors that come to the institution will act as de facto "eyes upon the street" who inhibit criminal activity by increasing criminals' perceived likelihood of getting caught. The

result of this mechanism is that the supply of criminals is reduced. Criminals facing the increased chance of being caught would result in a reduction of the supply of crime, reducing crimes committed.

1.3.3. Suitable Targets Mechanism

Criminals must weigh the benefits and costs of committing a crime and choose the best course of action for them. An open library provides "suitable targets" because there are more people to try to victimize when the library is open. Libraries are frequently used by students carrying laptops, smartphones, and other expensive electronic devices. These devices are ideal items to steal and are plentiful inside libraries. Therefore, any open library would have to contend with an increase in the number of would-be criminals that would accompany any open library, each looking to take advantage of other patrons' belongings.

The social services, eyes upon the street, and suitable targets mechanism portray a complex and deep picture of institutions and how they may affect crime. For instance, all three mechanisms either increase or decrease the number of crimes committed or increase or decrease the price or cost of crime depending on which effect dominates. Every hour a library or comparable institution is open, all three are in play and their effects are difficult to disentangle. In the sections that follow, I detail my data, empirical strategy and results of the Los Angeles Library natural experiment.

1.4. Empirical Strategy and Data Description

In the following section I describe the variation in library hours and sources of possible endogeneity and bias. Following that, I describe the crime data and the Los Angeles library map. Finally, I discuss my benchmark difference-in-differences model.

1.4.1. Variation in Library Hours

As shown in Figure 1.1, Table 1.1 and Table 1.2, the Los Angeles Public library changed its hours on many occasions during the sample. Not only did the hours change on many different dates, the hours changed on different days of the week, and for different library types, and all types experienced cuts and restorations. Some libraries were closed due to renovations or haven't opened yet, another source of variation in library hours. An in depth discussion of the possible sources of bias occurs following the model description.

1.4.2. Dividing Los Angeles into Library Zones

To test the hypothesis that providing free and available library affect crime, the distances between each library were calculated. This process allowed me to divide Los Angeles into separate "library zones" where for any given location in Los Angeles, I know which library is the closest library. The closest library is relevant to my analysis under the reasonable assumption that an individual can only travel to one library at one time, and if any library has any effect on him, it will be the closest. This division of Los Angeles is shown in Figure 1.2, where the polygons represent distinct "library zones" and each dot represents a library.

The criminology literature that focuses on how far criminals travel to commit crimes has found that most crimes occur close to the criminal's home. Using burglars, the most common distance found from the criminal's home to the crime scene is under one mile (O'Leary 2011). Similarly, Snook found that one mile was the median travel distance for burglars and found that 33 percent of all burglars travel less than one kilometer or roughly 0.6 miles (Snook 2004). Informed by this literature, I divided every library-zone into three discrete "library radii-zones," to test the effects of changes in library hours on crime in areas closest to the library versus far away. Dividing the library-zones by distance to the library allows me to test differential effects of library closures by distance and by neighborhood. For instance, the library closures might affect those closest to the library the most versus far away. An inner library radii-zone is a circle with a radius of 0.6 miles and under "as the crow flies." The next closest is a donut between two circles with a radii of 1.0 – 0.6 miles, and the furthest zone is the area greater than 1.0 miles away from a library. This means if a crime was committed within a given library radii-zone, the crime is associated with the closest library and the radii in which the crime occurred. For instance, if a crime occurred within 0.6 miles of Library A, the crime will be attributed to Library A, at a radii of 0.6 miles and under. This methodology allows me to test the effects of library hours on crime locally, as the places closest to the libraries should see the largest effects of library hour changes. This construction is shown in Figure 1.3, which shows each distinct "library-radii-zone" area within 0.6 miles, between 0.6 miles and one mile, and greater than one mile for each library in Los Angeles.

Population counts are needed for each library zone to normalize crimes committed by their respective population density. The zones were overlain with Census block population data retrieved from the 2010 Census. The population of each library zone was calculated by summing

the total population of the Census blocks residing in the zone. If a Census block is contained in multiple library areas, that Census block's population count was divided amongst the libraries by the percentage of area the Census block inhabited in each library zone.

1.4.3. Crime Data Description

The crime data is from January 1st 2009 to September 17th, 2011, encompassing all part one crimes committed in Los Angeles city and reported by the Los Angeles Police Department (LAPD).[7] Part one crimes include all violent crimes such as robbery, assault, and homicide as well as property crimes consisting of burglary and theft (including motor vehicle theft). Due to privacy issues, rape was not recorded in the data.

In total, 298,659 crimes were committed in Los Angeles during this period. Of those, 242,150 were property crimes, and 56,509 were violent crimes. As is typical of every major US city, most crimes are property crimes. The type of crime committed by library radii-zone is provided in Table 1.3.A. The data is at the reported incident level, such that date, time, offense and location are recorded. For the Los Angeles city libraries, their locations were obtained from the city libraries website. The locations of each crime were collected by the LAPD. The LAPD reports two dates for a reported crime, an end date and a beginning date. These dates might not equal each other for a variety of reasons, e.g., an embezzlement that continued for a few days or longer, a burglary during an extended vacation, or a theft that went initially unnoticed. Below in Table 1.3.B, I present crime totals for each library radii for the subset of crimes where the difference between the end date and the beginning date are is equal to one or zero days. Predictably, property crime falls when the dates must be equal to each other, as many property

[7] Concerns about underreporting from the Los Angeles Times report will be addressed in a robustness section.

crimes can happen overnight or over a weekend without notice. Despite this potential data issue, the number of crimes only falls by eight percent when the sample is restricted in this way.

Crimes are counted for each library radii-zone on a daily basis. Each observation indicates the relevant radii of the library zone and the library for each day in the sample. Daily crime counts in each library radii-zone are reported per 100,000 population. To show what the occurrence of crime in Los Angeles looks like graphically, a "heatmap" displaying crime counts by year for the one mile library-radii-zone is shown in Figure 1.4. From the heatmap, we can see that most crimes occur in the densely populated areas downtown. Also, we can observe that the yearly crime counts are relatively consistent. This motivates the use of area controls in the model described in the next section, to remove any geographic crime effects.

1.4.4. Benchmark Model Specification

Below, I specify the benchmark model.

$$CrimeRate_{it} = \beta_0 + \beta_1 LibHours_{it} + \beta_2 (radii_{r=0.6} * LibHours_{it}) + \beta_3 (radii_{r=1.0} * LibHours_{it}) + X_{it}\Phi + \alpha_i + \delta_t + \varepsilon_{it} \quad (1)$$

The model variables are defined as follows: $CrimeRate_{it}$ represents the number of crimes reported per 100,000 population per day (t), per library radii-zone (i). The fixed effects captured in the model are α_i, which represents library radii-zone location fixed effects, and time fixed effects by date, δ_t. The variable $LibHours_{it}$ is the total number of hours per library radii-zone. For each library, the hours applicable to each radii-zone are the same. The variable $radii_{r=0.6}$ and $radii_{r=1.0}$ represents values for the categorical variable radii, for radii-zones of 0.6 miles and under, and between 0.6 miles and 1.0 miles radii. The baseline library-zone radii is that of radius greater than 1.0 miles, this is represented by the main library hours variable. Therefore, the

interpretation of the interaction between $LibHours_{it}$ and the radii coefficient is the difference of the slope of library hours of a radii-zone close to the library versus the baseline category, farther than 1.0 miles away. The baseline $LibHours_{it}$ coefficient therefore represents the slope of library hours for areas further than 1.0 miles away.

The vector X denotes a selection of school day variables. These count how many schools of different types, elementary, middle, high, and continuing education, are open in a particular day t and library radii-zone i. If school days and hours are correlated with library hours, and if school days determine crime effects, then the model may suffer from omitted variable bias if school days are left out. To account for this possibility, they are included. This is further discussed in the robustness section.

Standard errors are clustered at the library level throughout, the largest level of my variation in library hours as recommended by Barrios et al. (2010).

1.4.4.1. Possible Sources of Bias

While the library hours ($LibHours_{it}$) cuts and changes the library administrators chose were different by library type, date, amount and day of the week, they were not randomly assigned. The library administrators faced a choice in deciding what days to cut and how much of each day to cut the hours by and in which facilities. From the public minutes of the Board of Commissioners Library Council meetings, the administrators were concerned and argued about this very issue. The true reason for the cuts is important for the interpretation of my results because the marginal hour cut in each library likely has a different effect than a random hour cut would. If the council chose to cut hours on busy days, I will be estimating the effect of cutting hours on the busiest days on crime. Similarly, if the council chose the least busy days, I will be

estimating the effects of cutting hours on the least busy days on crime. The minutes show the administrators focused on disrupting library visitors as little as possible by cutting the least busy whole days.

The Board of Commissioners gave several reasons for choosing Sunday and Monday to cut entirely. The meeting minutes state cutting whole days is preferable to imposing several half-days because half-days may involve complicated "labor rules and negotiations to break workdays in half[8]" in addition to there being "no way to determine whether two half-days would be more efficient in reaching a number of residents than it would to close for one day" as well as "cause confusion" as to the library's opening hours.

Their minutes also show Sunday closure was chosen to because it provides the "greatest salary savings" and "Sunday is among [sic] days with the lowest use[9]". Monday was chosen as "many holidays fall on Monday", reducing the disruption to library traffic by selecting a day of the week that was already going to be closed numerous times throughout the year. By cutting Monday and Sunday hours completely, the library saves any maintenance, security and administrative costs of opening the library, while minimizing the impact on library patrons.

Given the library administrators decided to cut the least busy days and minimize impacts to visitors, I conclude my estimates are representative of cuts on the least busy day instead of the effects of cutting a random hour. Foot traffic is related to crime levels; with fewer people there is less to steal or victimize and fewer people to do the stealing or victimization, lowering crime. However, there are also fewer "eyes on the street" to deter crime. As a result my coefficients will be representative of the effects of reducing the least popular and open library hours on crime.

[8] Minutes of the Board of Library Commissioners, City of Los Angeles, June 10, 2010.
[9] Minutes of the Board of Library Commissioners, City of Los Angeles, March 25, 2010.

1.4.4.2. Presence of Security at Libraries

One possible source of omitted variable bias could occur if the library system cuts affected the security guards present at various libraries. If the presence or absence of security guards is the major catalyst for any crime changes from library hours cuts, then the security guards at library will be the driver of hours effects. However, the department responsible for library security, the Office of Public Safety stated none of the current "security officers will be laid off[10]". To my knowledge, there is no evidence of any other security changes occurring during the sample period.[11]

1.4.4.3. Temporal Displacement of Library Patrons

Another possible source of bias is the displacement of library patrons in time or across places. When faced with a closed library on some day, if patrons decide to skip their usual library visit, then my interpretations hold as stated. However, if library patrons respond to hours cuts by shifting their library visits to other days, then other days may or may not have an increased criminal or victim presence due to the influx of patrons who would not have otherwise been there. However, annual visits declined by approximately the same percentage as the number of hours cut. From 2009 to 2011 library hours fell by approximately 25 percent, and visits fell by 28 percent (California State Library Statistics 2012). Given visits fell by a similar amount to hours, I conclude most visitors responded to the library cutbacks by visiting the library less frequently.

[10] Minutes of the Board of Library Commissioners, City of Los Angeles, June 10, 2010.
[11] To my knowledge, security officers stay inside during open hours to protect the library, so any effect of their absence should be minimal on the areas outside of the library.

1.4.5. Argument for Weighted Least Squares

The creation of library radii-zones is intended to capture the crime effects of areas close to and far away from each library. One cost of this method is the library radii-zones have non-identical populations. As the population of the areas becomes smaller, the variance of any type of crime rate increases, identifying heteroscedasticity in the data. To address this, in my benchmark model I perform weighted least squares, where the weights used are the resident population. There are two reasons for this. First, the nature of this dataset involves construction of library radii-zones by dividing Los Angeles. A casualty of this method is not all areas are equally sized. Therefore, the census population estimates, an average of the census block estimates, get progressively more imprecise as the area becomes smaller. Secondly, the crime rate is by definition the number of crimes divided by the number of population. Given my population estimates vary significantly by place, the variance of my crime rates increases as the population estimates decrease. See appendix A1 for a figure displaying the inverse relationship between crime rates and population. In the next section I describe my results, the empirical evidence linking library hours and crime rates.

1.5. Empirical Evidence of Library Hours Affecting Crime Rates

First, in Figure 1.5, I show how de-meaned car burglary rates responded to changes in library hours for a subset of the available variation. To illustrate the variation in library hours clearly, the figure only shows branch libraries, without holidays, without any library undergoing renovations during the sample period, for areas within 0.6 of a library by day of the week. As seen in the figure, the cuts on Monday and Wednesday, show corresponding increases in car burglary rates near libraries.

Table 1.4.1 contains my preferred benchmark results. Next, in Table 1.4.2, I will present results that loosen the reported incident date range restrictions, to remove the possibility of imprecise incident dates clouding my results. Third, in Table 1.5, I show results that disaggregate reported crimes by whether they are gang crimes or not gang crimes. Tables 1.6.1.1, Table 1.6.1.2 show a series of robustness checks.

As the results in Table 1.4.1 show, burglary from vehicle (column 11) and burglary (column 10) display negative and statistically significant effects at the one and five percent level for my covariates of interest. For burglary from vehicle, I find coefficients of -0.0172 for areas within 0.6 miles, and -0.0162 for areas between 1.0 and 0.6 miles statistically significant at the one percent level. The effects show a change in the burglary from vehicle crime rate from -0.83 percent to -0.37 percent on average per one library hour increase. As this coefficient is part of an interaction of radii variables, the interpretation of the -0.83 percent to -0.37 percent is the difference in crime reductions for each hour open between areas within one mile from a library and from areas greater than one mile away from the library. However, despite negative and statistically significant coefficients on the interaction between library hours and nearby areas for the overall crime rate the base coefficient on library hours is positive and statistically significant at the ten percent level (column one). This is being driven by positive coefficients for the main hours in the regressions for burglary, theft and assaults. One possible explanation of this is displacement. For instance, as library hours increase, individuals may leave their homes in areas far away from a library, which makes it vulnerable to burglary as well as removes a car to burglarize. Burglars who are also familiar with car burglary may substitute from thefts from cars into residences or other spaces as fewer vehicles are nearby.

1.5.1. Crime Date Interval Restriction Results

As noted above, each crime is recorded with two dates, the beginning and end date. These dates represents the interval during which the crime occurred, or could have possibly occurred. For some types of property crime, particularly burglary, victims may not notice they have become victims until several days later. This introduces measurement error into the crime counts by date, which will make my estimates less precise. To assess this, a subsample is created where if the end date from any crime is more than a day later than the beginning date, the crime is dropped. This results in a loss of almost 24,400 property crimes, the majority of which are burglary and burglary from vehicle. Despite this the results from Table 1.4.2 (the restricted sample) are in line with the full sample from Table 1.4.1: I find negative and statistically significant effects of library hours on nearby areas on burglary from vehicle rates.

1.6. How Changes in Library Hours Affect Gang and Non-Gang Crime

Gangs are criminal organizations and their members may react differently to changes in library hours than do non-gang members. For instance, if gang members are not regular library users, then they may not respond to local changes in library hours. Therefore, to the extent that gang-related crimes are a significant portion of violent crimes, it may blur any possible effects of library hours on types of violent crime. Any time a crime is catalogued by the LAPD, officers will record relevant *modus operandi* or M.O. codes in the crime database. One of these codes is "gang-related," meaning any crime where gangs are involved. Predictably, the catalogued gang crimes are almost always violent crimes as property crimes will only be noted as gang crimes if the gang were to leave incriminating gang-related evidence. Violent crimes, on the other hand, have victims that may be gang members themselves or may have heard a gang-related threat.

After categorizing violent crime into gang-related and non-gang-related crimes, gang-related violent crime represents more than 22 percent of all violent crime.

In Table 1.5, using gang-related crime rates as the dependent variable, I find that only robbery shows a marginally statistically significant effect from changes in library hours (at the ten percent level). All other effects are statistically insignificant at conventional levels. However, for non-gang-related aggravated assaults, I find statistically significant and negative effects of changes in library hours in areas nearby libraries. The interpretation of the coefficient is that a one hour increase in daily library hours decreases the non-gang aggravated assault rate by 0.01 per 100,000 population, which represents a 1.9 percent decrease on average compared to effects in areas far away from the library. I find no other statistically significant effects for any other non-gang violent crime. This is plausible as gang members may not be personally interested in spending time in a library, therefore contributing noisily to overall aggravated assault effects. By removing the gang-related aggravated assaults, I am able to estimate the impact of increased library hours on a potentially more relevant section of the population of crimes.

In addition, roughly 50 percent of aggravated assaults are committed by a friend, relative, or an intimate partner (BJS 2010). Therefore, one possible explanation of these results is that these types of heated encounters amongst non-strangers may have been avoided if the victim or perpetrator was in a location like the library rather than in their residence. If the perpetrator was not in contact with their potential victim, then there is no possibility for an assault to occur.

1.6.1. Controlling for Library-Zone Effects with Base Coefficients

In my benchmark model, I explicitly control for city-wide daily effects, and individual library-radii-zone effects. However, I am unable to control for all possible library-radii-zone-day

covariates affecting crime. Examples of these include daily foot-traffic, which would control for the flow of people in a library-zone in a given day and is directly related to the amount of crime. To account for this, I place more emphasis on the differenced coefficients, which are the interaction coefficients of library hours and 0.6 mile radii and between 1.0 and 0.6 mile radii. The base coefficients themselves capture library-zone-day effects, as they capture variation common to all three radii. Therefore, the most trusted coefficients for making inference must be the interactions, as they have differenced out any effects common to all three radii I am not able to control for. This holds under the assumption that any omitted variable bias is constant within a given library-zone.

Table A2 in the appendices presents an alternative specification with library-zone by date and radii fixed effects. This controls for inter-library-zone variation by day, but I lose the ability to estimate the base coefficient. However, the interpretation of the differenced interaction variables of interest remains the same. I find results consistent with my earlier estimates, statistically significant and negative effects of more library hours on burglary from vehicle and non-gang related aggravated assault within one mile of the library.

1.7. Robustness

In this section I discuss potential endogeneity bias and confounding variation concerning the Great Recession and school days, as well as robustness specifications for each of my results.

1.7.1. The Great Recession and Possible Endogeneity Bias

Library hours were changed across the city at distinct and different dates for most libraries simultaneously.[12] The Great Recession was certainly the catalyst for the library hour changes, as budget shortfalls caused by the Great Recession caused the city to issue funding cuts to the library system. Therefore, one possible source of endogeneity bias is the timing of the cutbacks with the Great Recession if it is also associated with less crime. Unemployment and other features of the Great Recession are also correlated with crime. Because of this, it is an initially worrisome alternative interpretation that the effects I observe of library hours changes on crime are actually the effects of the Great Recession and its correlates on crime, if the Great Recession caused the library cuts and is correlated with crime.

However, while the recession and especially the employment effects on crime plausibly affected Los Angeles's overall crime rate, these effects were felt by the city as a whole, on dates not correlated with the library bureaucracy meetings, not correlated by type of library, and not by day of the week. In addition, several branch libraries were under renovations, had unexpected closures, or had a grand opening in late 2009. These libraries experienced zero hours and provide an additional plausible counterfactual. To test this, I regressed Los Angeles weekly gas prices, a plausible correlate of economic activity, on library hours and find absolutely no statistically

[12] Exceptions to this include the Central and regional libraries, and the libraries not yet opened or under renovations, please see Table 1 and Table 2 for more information.

significant correlation between library hours and weekly gas prices.[13] I conclude the variation in library hours I observe does not suffer from Great Recession related omitted variable bias.

1.7.2. Los Angeles Police Department Underreporting

After an investigation by the Los Angeles Times, the LAPD was found to be underreporting violent crime counts between 2012 and 2013 (Los Angeles Times 2014). Most of the underreporting consisted of downgrading aggravated assaults into simple assaults. I do not believe that the underreporting affects the results of this paper for three reasons. First, it seems implausible to suspect that the misreported counts in any way correlates with library hours. There is nothing from any statement or the findings of the article to suggest that the officers used the changing library hours as a catalyst for their misreporting. Second, even if supposing the previous point is false and the misreported crime counts happen to be correlated with library hours, the correlation would likely be positive overall as library hours fell while aggravated assaults were underreported. However, my results show a negative relationship between library hours and the aggravated assault rate near libraries. Therefore, if the underreporting is correlated, the positive sign effect has not affected the overall negative sign of my results, and my results could be underestimated. Three, the investigation used 2012-2013 data, a year after the data used in this paper. While it could be the case that the data manipulation occurred earlier, there is no direct evidence.

[13] After controlling for the year and day of the week, I find no correlation between weekly gas prices and library hours. Localized gas prices would be ideal but were unavailable to the author at the time of this draft.

1.7.3. Confounding Variation from School Days

One possible source of confounding variation is that libraries are open in part on school days. If library hours are correlated with school days and hours, and school days and hours are correlated with various crime rate outcomes, then my results may be biased. To address this I have included controls for every school in Los Angeles Unified School District, the largest public school system in California and the 2nd largest in the United States. Every school's geographic location is placed within the appropriate library radii-zone the school is located in. The calendars for each type of school were collected to determine for each day in my sample which schools are open by type and for what date. Therefore, any confounding school variation by day, by place and by school type is accounted for. A map of Los Angeles with the variation in school placement is displayed in Figure 1.6. The inclusion or exclusion of school days into my specifications has no appreciable effect on any of the results.

1.7.4. Robustness of Burglary from Vehicle

In Table 1.6.1.1 and Table 1.6.1.2 I present robustness checks of the results for the crime category burglary from vehicle. This was chosen for extensive robustness checks because car burglaries typically occur when the owner is not present. Therefore testing robustness according to how I code dates, either by using the beginning or end date, is important. The specifications tested included varying the location fixed effects from library-radii to library, beginning date, end date, date interval one or zero, reducing the farthest away region to 1.5 miles, including library by date and radii fixed effects, and excluding population weights. The only specification that produces relatively different results from my benchmark findings are the un-weighted specifications, where one out of those two is statistically significant at conventional levels.

1.8. Robustness Specifications: Radii and Library Zones

As shown above, my results are robust to changes in day type, fixed effects and many other specifications. One potential problem these specifications could overlook are artifacts of the map construction. Specifically, it may be the construction of the library-radii-zones themselves that are driving my results, instead of the crime effects. To test this, I conduct two full robustness specifications where the library radii-zones are aggregated together to create library-zones instead of library radii-zones. This way there is only one area per library, instead of three as in the benchmark case. As shown in Table 1.6.2, I find effects on burglary from vehicle are negative and statistically significant at the five percent level with comparable magnitudes to my previous estimates. I find no statistically significant effects for any other type of crime, which fits in with the overall effects observed from the previous tables. These tables showed statistically significant effects of library hours by radii, while did not show any significant change overall.

For the second robustness specification, in Table 1.6.2, I cut my sample and only include the library-zones into areas with a max radius of one and a half miles. This specification tests for the possibility that the results from the previous specification are being driven by including areas very far away from any library, thus unknowingly incorporating omitted variable bias from unobserved far away shocks. After restricting the outer radii areas to one and a half miles at most, I run the same benchmark model as before. My results are consistent with my earlier estimates, as I find negative effects of hours changes on burglary from vehicle that are significant at the one percent level and no other effects. These two robustness specifications show the map construction, at least on the radii level, was not responsible for the observed negative effects library hours had on car burglaries.

1.9. Effects of Library Hours on High Violent and Not-Low Crime Areas

The areas that should see the most crime reductions from increased library hours will likely not be the areas with the lowest crime rates. As crime levels cannot go below zero, larger effects should plausibly be observed in areas with significant crime rate levels. While we might observe effects in areas with low but not zero crime rates, we should observe the largest and most salient effects of library hours on high crime areas. Since violent crime is by far the most dangerous and economically disruptive type of crime, I investigate how library hours affect areas with the highest violent crime rates.

To complete this analysis I utilize the aggregated library zones developed in the previous robustness section to investigate how in ex-ante crime-ridden library-zones respond to changes in hours. I aggregated the library-radii-zones into library-zones because I wanted to test for possible cross library-zone effects of library hours between high and low crime library-zones, instead of intra-library-zone as in the benchmark specification. This enables me to test for effects across entire library zones. These results are present in Table 1.7.1, where I interact the library hours variables with a variable indicating if the violent crime rate for the library zone is above the 75^{th} percentile for the whole city. To determine the percentiles, I calculated the violent crime rates for each library zone before the treatment took effect. The pre-treatment levels for all non-renovated library is before the first set of library cuts, December 9^{th} 2009.

The aggravated assault and homicide coefficients on the interaction term between the main effect library hours coefficient and the 75^{th} percentile violent crime rate coefficient are negative and statistically significant at the five percent level for homicide, and at the one percent level for aggravated assault. Additionally, I find a positive and statistically significant at the five percent level on the library hours coefficient for grand theft person. The coefficient on library

hours for aggravated assault represents a decrease in the aggravated assault rate by 1.2 percent per library hour, for high violent crime areas above and beyond changes in average or low violent crime areas. However, given the base library hour variable is positive, though not statistically significant, the overall effect on Los Angeles as a whole may be small or zero given that the magnitudes are similar.

The homicide coefficient represents an average decrease of 4.4 percent per library hour for high violent crime areas. The main effect coefficient of library hours for homicide is also negative, albeit insignificant, indicating library hours do have an overall effect on homicide rates. A plot of the relationship negative relationship between homicide rates and library hours is shown in Figure 1.7. What may be occurring is the following combination of the three mechanisms of how institutions affect crime. If the libraries are open, there are more eyes on the street to deflect and identify possible assailants. If more criminally risky individuals use the library, and subsequently are less able to fight at home, aggravated assaults may decrease. If aggravated assaults decrease in high crime areas, aggravated assaults that would have escalated into homicide should not occur.

Despite this, 4.4 percent per hour appears high, even for high crime areas. As a robustness check, I used another indicator variable, areas above the 25th percentile in overall crime rates instead of violent crime rates. This indicator is interacted with library hours, using the same library-zone map construction instead of the library-zone-radii construction. I chose to use the 25th percentile[14] to difference the areas that have at least some amount of crime from those that have low crime rates, and to test whether the earlier homicide result is an artifact of the

[14] To determine the percentile, I only counted areas without renovations or closings, before December 9th 2009, that start of the library hour cuts.

violent crime rate percentile construction, and not a result of changes to library hours.

As shown in Table 1.7.2, the coefficient on homicide is negative and significant at the five percent level. The magnitude decreases from the previous specification to a 3.1 percent decrease in homicide rates on average per library hour for areas above the 25th crime rate percentile. While still large, both specifications are consistent with each other. In Table A1 in the appendices, I show an additional interaction model with a 5th crime rate percentile indicator, which is essentially all library zones with at least some crime. I find results consistent with my earlier specifications.

Additionally in Table 1.7.2, I find coefficients of library hours changes that are positive and statistically significant at the five percent level for grand theft person and burglary. The grand theft person coefficient is consistent with the suitable targets mechanism described earlier in the paper. As individuals often carry phones and laptops in the library, an increase in grand theft person, theft above roughly $1,000, seems plausible. Also, burglary might increase in high crime areas as individuals who drive to the library may leave their homes or shops unattended. As a result, library hours could increase the burglary rate in high crime areas.

1.10. Discussion

In this study I estimate how public institutions change the nearby criminal landscape through opening their doors and attracting witnesses, victims and criminals. To identify the impact of public institutions on local crime rates I exploit exogenous variation in public library hours in from 2009 through 2011. I find an increase of library hours lowers car burglary rates near libraries. Given car burglaries happen outdoors or in garages were cars are parked, I find

eye's upon the street mechanism is a good explanation of why increased public institution hours result in lower car burglaries.

I also find evidence of possible burglary substitution, as burglars faced with increased witnesses near public institutions substitute from car burglary to burglary of the potentially un-inhabited homes of the public institution's patrons. In addition, the burglars far away from the public institutions lose cars to burglarize if the patron uses a car to travel to the institution, but gains an open and potentially unguarded house or business. This is supported by the combined burglary rate decreasing monotonically in distance to the public institutions as operating hours increase.

I find non-gang related aggravated assaults decrease near libraries compared to areas farther away as a result increasing hours. Individuals using the library as a safe haven might get into fewer arguments and fights, and fewer deadly fights as a result. For non-gang members, the individuals who do not implicitly endorse violence by gang membership, public institutions provide a place to spend time safely as opposed to being out on the street.

In high crime areas, free local safe havens may be in short supply, making the public institution a critical local resource. This may be due to increased eyes upon the street, discouraging potential fights, or because the potential aggressor or victim is at the public institution instead of at home or in the same room as their aggressor. If any significant percentage of these fights turn deadly, then the public institution has plausibly prevented a loss of life.

As a result of the cuts to the Los Angeles Public Library between 2009 and 2011, an estimated 42,515 total library hours were cut. If we consider the overall effects for the benchmark specification, this resulted in an estimated 28 additional homicides occurred, as well

as 514 additional car burglaries, and 129 non-gang related aggravated assaults near libraries. Homicide is extremely expensive, each occurrence is estimated to cost nearly $10 million in 2014 dollars (McCollister et al. 2010). Therefore, a back-of-the-envelope calculation of the total criminal social cost, including the effects of all types of crime, ranges between $203 million in the benchmark specification, to $244 million dollars in the high violent crime specification[15] (McCollister et al. 2010). The city saved only $21 million in 2014 dollars from cutting the library, resulting in a net spillover loss of $182 to $223 million dollars.

Public institutions are already demanded by citizens for the obvious and non-spillover services they provide. This research shows that by removing access to these public institutions, there are large criminal spillover effects. For policy, these spillover effects are important economic and social results that should inform policy makers to be aware of the criminal consequences of cutting the open hours of public institutions. Crime is a complex issue with many interlocking parts. More broadly than simply advancing policy, this research also improves our understanding of one more piece of the crime puzzle: that public institutions can affect crime rates by preventing many types of crime including aggravated assault, homicide, and car burglary, but possibly increasing burglary. However, the complete picture shows that public institutions prevent more types of crime than they cause, and the types of crime they prevent are the most serious and costly.

[15] Homicide has the largest cost of any type of crime by a significant margin. McColister et al. estimate the total cost of a homicide at $9.9 million 2014 dollars, including tangible and intangible costs. The next most expensive is aggravated assault at $0.12 million 2014 dollars.

Chapter 2

The Impact of Internet Availability and Crime: Evidence from the Roll out of Google Fiber

2.1. Introduction

Since the 1990s, crime rates in the United States have fallen dramatically. From 1993 to 2012, the violent crime rate fell by almost 50 percent (Federal Bureau of Investigation 2012; Bureau of Justice Statistics 2012). A number of theories have been advanced by criminologists and economists to explain this unprecedented decrease in crime.[16] These theories include the decline in the unemployment rate, the increased police presence, and the rising prison population, among others. While these explanations have been shown to be causal drivers of the crime rate (Raphael Winter-Ember 2001; Marvel Moody 1996; Klick Tabarrok 2005; Levitt 1996, 2007), they all fail to explain the continued decline in crime during the Great Recession. Figure 2.1 shows the violent crime rate by year plotted with the unemployment rate, police officers per capita[17] and the incarceration rate.[18] Unemployment has been to proven to be a positive driver of the crime rate, while the presence of police officers and incarceration rates have been shown to be negative drivers. Many of these drivers of crime diverged sharply from expectations during the Great Recession, and the violent crime rate continues to fall. These

[16] For a comprehensive review of various explanations of why crime fell in the 1990s, please see Levitt (2004).
[17] The percent of the U.S. population that are police officers.
[18] The percent of the U.S. population that are incarcerated in federal, state, local and private prisons or jails.

theories have not been proven incorrect; however, the sustained fall in violent crime rates illuminates our incomplete understanding of the causes of crime.

One major change in the United States since the 1990s has been the increased use of computers and computer-related technology, specifically the internet. Since its widespread adoption during the 1990s, email, social media and streaming video have altered how most individuals communicate and gather information. At present, younger individuals are heavier users of the internet than the rest of the population; in 2014, 97 percent of individuals aged 18-29 used the internet, email or a mobile device, compared to 88 percent of those aged 50-64.[19] In addition, younger individuals are more likely to be arrested or to be a victim of a crime in comparison relative to the rest of the population (FBI 2012). If spending time using the internet is desirable enough to raise the opportunity cost of criminal activities in favor of non-criminal actions, individuals may choose to spend more time online and indoors instead of outside and on the street. More time in a potentially safer environment could result in fewer crimes being committed due to a reduction in the supply of criminals and/or victims. In the lower right panel of Figure 2.1, the violent crime rate and the non-internet user rate are displayed, showing a relatively steady positive correlation in the time series. As internet adoption spreads and the number of non-internet users decreases, the number of individuals potentially without an attractive way to spend time safely indoors may also decrease. As a large share of the population has received internet access, and as the internet is attractive to young people, a large share of the most criminally risky demographic group, the young, now has a safer alternative way to spend time.

[19] Please see http://www.pewinternet.org/data-trend/internet-use/latest-stats/ for more information.

To test the theory that an increase in internet adoption rates raises the opportunity cost of crime, resulting in a lower crime rate, the daily variation in the roll out of Google Fiber in Kansas City, Kansas and Kansas City, Missouri from 2012 to 2013 was exploited. Exogenous variation in internet availability is difficult to find. It is usually plagued by endogeneity concerns, as cities that show an upward change in internet adoption are also likely to experience simultaneous rises in median income, education levels, or other combinations of factors related to crime. From 2012 to 2013, Google Fiber was installed in neighborhoods in order of the percentage of neighborhood households that had pre-registered for the service. Within each state, the neighborhood with the highest preregistration percentage had Google Fiber installed earlier. However, within neighborhoods with nearly identical Google Fiber preregistration levels, neighborhoods in Kansas had Google Fiber installed earlier than did neighborhoods in Missouri. This cross-state variation allowed the study of the effects of internet availability within any neighborhood preregistration percentage, given the cross-state variation in roll out. This was an important control, as differences in neighborhood preregistration levels act as a proxy for internet demand, and may be correlated with crime outcomes in relation to other demographic variables, such as income.

To demonstrate the validity of this experiment, it needed to be ascertained that in addition to the roll out of Google Fiber, that the population in Kansas City actually used Google Fiber. In order to confirm this, a Google Trends search was performed, using terms relative to the popularity of relevant searches, such as ‘netflix’ and ‘youtube’ from Kansas City. These were then compared to a comparable control city, using the average installation population of Google Fiber. This provided additional plausibility evidence, as the data revealed that in addition to Google Fiber being installed on certain dates, the Google Trends evidence showed that usage

behavior also changed on those dates. This was visual evidence that Google Fiber installation did influence internet behavior, and that the variation in Google Fiber installation represented a valid experiment in internet availability (discussed in depth section 5.2.1).

Therefore, one of the main contributions of this paper was the novel use of the exogenous variation of daily internet availability across locations. While the variation in internet roll out in Kansas City did not extend sufficiently into the past to explain the crime rates seen in the Great Recession, it was used here to test the hypothesis that increased internet availability decreases crime rates.

2.2. Background

In the economics literature, the importance of the internet is recognized as improving employment and consumer searches[20] (Kuhn, Skuterud 2004; Brown, Goolsbee 2006; Stevenson 2008). The internet is especially important to young people, as spending time online is very popular (Pew Research 2012). This behavior is potentially important to the study of crime, as this demographic group has high arrest and victimization rates (FBI 2012). If this criminally risky group spent more time online instead of outside on the street, crime rates would be expected to decrease. This is related to the Becker rational criminal model, which dictates that increases in opportunity cost results in fewer crimes committed (Becker 1968). This is also related to the incapacitation literature, which has shown the permanent or transient crime effects of staying in prison or, at the opposite end of the spectrum, staying in movie theaters (Levitt 1996, Dahl, DellaVigna 2009).

[20] The effects of internet adoption cited here may, however, also be interpreted as small relative to the ubiquity of internet usage.

From 2003 to 2012, as internet availability increased, the time spent using the internet has also grown (Figures 2.2 and 2.3). For those aged 18-24 years, time spent using a computer for leisure more than doubled (American Time Use Survey 2012). For black males aged 15-17, the time spent playing online games has approximately tripled, from more than 40 minutes per day to over 130 minutes per day (ATUS 2012). During this same time period, the arrest rates for juveniles have also declined (Figure 2.4). This shows that as the availability and adoption of technology has risen, the use of these technologies has also risen, and that this has occurred simultaneously with decreases in crime.

Young individuals, especially young men, commit crime and become victims at rates higher than any other demographic group. People aged 12-17 years have a violent crime victimization rate of 48.4 per 1,000, the highest of any other comparable age bracket and roughly 50 percent higher than those aged 35-49 years (BJS 2012). The gender differences in crime victimization and arrest rates are also large. For violent crime, the male victimization rate is 1.25 times that of females, and the male violent crime arrest rate is four times that of females (FBI 2012). Therefore, it is unsurprising that among men, young men are more likely to be arrested. The arrest rate of men who are aged 15-24 years is 1.6 times that of men aged 25-45. Therefore, it is plausible to expect crime effects from altered behavior of this salient and criminally risky demographic group if the increased internet availability leads more young people, and especially young men, to use the internet safely indoors.

Alternatively, increased internet availability could result in increased crime. Bhuller, Havnes, Leuven and Mogstad (2013) found that, in Norway, an increase in sex crimes (rape and child abuse) were associated with higher yearly broadband adoption rates. However, their paper reported findings consistent with those of this study in regards to every other type of crime, as

neither paper identifies effects due to internet adoption.[21] One way in which this paper improved upon the experimental design used in Bhuller et al., (2013) was through the use of data with daily variation in internet availability and crime instead of yearly variation. However, it is important to reiterate that this variation measured the dates within a given year when a neighborhood gained increased internet availability; therefore, there was no possibility for the temporal displacement of crime that may have occurred in other studies of crime, such as that by Dahl and DellaVigna (2009).

Using daily data significantly reduces the possibility of spurious results that may occur with the use of yearly data. For example, if yearly data is used, there is a danger that two variables, one occurring at the beginning and one at the end of the same year (such as increases in internet access rates and in sex crime rates), may be mistakenly associated, although due to unrelated mechanisms. In addition, the use of yearly measures can lead to the possibility that the crimes were committed earlier in the year, before internet availability increased, or that both the crimes and the increase in availability were driven by a confounding variable. In addition, a daily design is a more salient test of the internet as a driver of crime. If individuals treated with increased internet access immediately respond with increases in sex crimes, it is plausible to assume that the resulting crime would occur close to the treatment date, before any other activity distracted the potential perpetrator or victim. This was unable to be tested in Bhuller et al., (2013) as yearly data was used.

[21] Bhuller et al., (2013) used a public program which expanded internet access in Norway as an instrument for internet adoption to model how internet use affects sex crime rates. They argued that the increase in sex crimes could have been due to the increased ability or willingness to report, an increased exposure to pornography (which they argued could have increased the propensity of potential offenders to commit sex crimes), or that increased internet access might have facilitated meetings of potential criminals and victims through increased access to chat rooms, message boards and social media.

However, no crime effects as a result of increased internet availability were found. This finding is notable for several reasons. In contrast to other research that reported a potentially temporary incapacitating effect on crime, such as that by Dahl and DellaVigna (2009), the change measured in this study represented a permanent change. Once internet access is deployed, it is always available in the household. Therefore, any temporal displacement of crime, when a criminal decides to forgo committing a crime today but commits it tomorrow, would not affect crime rates in this study, as once the increased internet availability occurs it is permanent. This means that the null finding does not indicate that potential criminals and victims did not change their behaviors in any way, but that any change in behavior did not result in any appreciable crime effects.

2.2.1. Google Fiber roll out in Kansas City

In 2010, Google announced that it would be deploying a one gigabit per second fiber-optic network in a selected United States community. After reviewing over 1,000 applications, Google selected Kansas City, Kansas. Later that year Kansas City, Missouri, was included (Google 2011). Google's stated reasons for selecting both of the Kansas Cities were the high level of support and coordination of the cities, and that Kansas City was a place where Google "could make an impact on the community" (Google 2011). Google divided both Kansas Cities into neighborhoods, and preregistration drives were launched in each neighborhood. Google would only deploy the fiber network in a neighborhood if a certain percentage of households in that neighborhood were pre-registered. The preregistration cost was $10; that cost would be applied to a future bill. Afterwards, neighborhoods that met the preregistration goal had Google Fiber installed. The order of installation was derived from the Google Fiber preregistration

percentages within Kansas and Missouri separately. This meant that neighborhoods that had the highest "demand" for Google Fiber (i.e., the highest percentage of household subscribers) would have Google Fiber installed earlier. However, Google chose to deploy Google Fiber in Kansas City, Kansas before Kansas City, Missouri. This meant that demand-related idiosyncratic differences across neighborhoods were able to be controlled for in this study, and that a greater variation in internet installation dates were able to be utilized than if both states had had the same installation schedule.

2.2.2. Preregistration level as demand proxy

Preregistration levels for Google Fiber are used throughout this paper as a proxy for demand. For each neighborhood, preregistration levels represented the portion of the neighborhood's households which desired Google Fiber at the quoted price. If the preregistration level of a neighborhood is used as part of a weighted average of the percent of available dollars in each neighborhood that desired Google Fiber (preregistration percentage multiplied by available dollars), these dollar values can be arranged from highest to lowest, showing a decreasing curve. This represents the demand proxy per neighborhood for Google Fiber versus the number of households that pre-registered.

In Figure 2.5, the preregistration percentage of the neighborhoods (y-axis) are displayed against the installation date of Google Fiber (x-axis), by state. The larger markers indicate a larger number of neighborhoods present at that preregistration level. For example, the earliest installation dates occurred on 12 and 15 November 2012 and 31 January 2013, with preregistration levels between 40 percent and 60 percent of households in Kansas. In Missouri, neighborhoods in the same range of preregistration percentages had Google Fiber installed

between 18 April 2013 and 11 July 2013. This revealed a variation in Google Fiber installation dates between neighborhoods of a maximum of 241 and a minimum of 77 days. Google began installing Google Fiber in a Kansas neighborhood in late 2012 and continued throughout 2013.

2.2.3. Google Fiber cost

The Google Fiber internet service included a gigabit-speed internet connection for US$70 per month, gigabit internet and television service for US$120 per month, or a five megabit download speed internet service for a nominal fee of US$300 or US$25 per month until the US$300 had been recouped, with zero additional cost guaranteed for seven years. This last low-cost internet option was integral to this analysis, as the gigabit internet service alone may have priced individuals and families out of buying it. The low-cost internet option was, in theory, affordable to the majority, as the price over the seven guaranteed years was $3.57 per month.

2.3. Data

The data for this research came from four sources. The crime data were administrative incident-level data from the Kansas City, Kansas and Kansas City, Missouri police departments. Internet availability data were collected from the roll out websites of Google Fiber for each Kansas City. Demographic data were collected from the U.S. Census Bureau.

Crime data

Crime data for both Kansas City, Kansas and Kansas City, Missouri were recorded at the incident level. Therefore, each time a crime was reported to the corresponding department, the crime was counted. The records were dated from 01 January 2012 to 31 December 2013. The

date range of the sample included the first two years of the roll-out of Google Fiber (2012 to 2013). The types of crimes catalogued in the data included homicide, rape, robbery, aggravated assault, burglary and theft.

Each record included the type of crime, the date, and the address or the latitude and longitude coordinates where the crime occurred.[22] The Kansas data included latitude and longitude coordinates.

Google Fiber

The Google Fiber data was retrieved from Google Fiber's Kansas City, Missouri and Kansas City, Kansas webpages. During the registration drive, Google publicly displayed the names, boundaries and locations of each neighborhood, and the ratio of households that pre-registered in comparison with the total number of households in each neighborhood.[23] Google designated their neighborhoods to be roughly in line with Census tracts.

Google began rolling out Google Fiber to neighborhoods with the highest preregistration levels (within each state) by certain deadline dates. Figure 2.6 shows the map of neighborhoods in both cities categorized by Fiber demand. As displayed in the map, the distribution of demand for Google Fiber was heterogeneous, with some areas having preregistration levels of over 60 percent of households, while other areas were under five percent. Internally, Google kept a demand cutoff percentage for each neighborhood. If the neighborhood had achieved a preregistration percentage that was higher than the cutoff, Google would then display the

[22] The Missouri data included addresses alone; these were geocoded using Google's geocoding service (geocoding refers to matching addresses to latitude and longitude coordinates for use in mapping software).
[23] These data were collected from Google Fiber's webpage as cached by the Way Back Machine. The Way Back Machine caches internet pages by date, and is a convenient tool to access previous versions of webpages (located at https://archive.org/web/).

preregistration percent, but would otherwise withhold that information. Google did display the location and name of every neighborhood that was considered for the roll out. Therefore, while the ability to control for separate very low demand levels that did not meet the cutoff was lost, as these were not reported by Google, this study did have the ability to use this group of neighborhoods (i.e., the neighborhoods with the lowest demand, which therefore did not receive Google Fiber) as a counterfactual control against other lower demand neighborhoods.[24]

Demographics

Demographic data were mined from the American Community Survey (2012). To find the demographic information relating to these Google constructed neighborhoods, the Census data by tract was overlaid onto the Google Fiber neighborhoods. If a neighborhood contained an entire Census tract, then the entire population of that Census tract was assigned to the Fiber neighborhood. If only a portion of a Census tract overlapped the neighborhood, then the percentage of the area of the tract inside the neighborhood was multiplied by the tract's population, and assigned to the Fiber neighborhood. By proceeding in this way, area weighted to population counts (and other count demographic variables) were created for each Fiber neighborhood. For other measures, such as median household income or the unemployment rate, which are not counts, a similar area-weighted average was used as well. If the tract was larger than the neighborhood, the reverse process was used, and the fraction of the total area occupied by a neighborhood was multiplied by the tract's population count.

[24] Please see Appendix Figure B1, Table B1, and Table B2 for a histogram and tables displaying the distribution of preregistration levels in the sample along with other descriptive statistics. The group of neighborhoods which did not meet their preregistration goals represented 11 percent of the sample (Figure B1).

In Figure 2.7, four graphs are shown plotting various demographic characteristics versus Google Fiber demand, by state. The demographics used were percentages of the population that identified as black or white, the unemployment rate and median household income. Unsurprisingly, as Google Fiber is a consumer product and is therefore not provided free of charge, strong correlations were found between the Fiber preregistration levels and other measures of material well-being. The demand rate was negatively correlated with the unemployment rate and the percentage of black inhabitants, while it was positively correlated with the percentage of white inhabitants and the median household income. As race is strongly associated with income level, these findings were unsurprising. In general, Kansas and Missouri had similar correlations between these demographic characteristics and the Google Fiber preregistration level. In Figure 2.8, a map of both Kansas Cities by neighborhood is shown, color-coded according to median household income. As noted above, a neighborhood having a higher median household income was associated with higher Google Fiber preregistration levels.

The demographic controls used were the neighborhood's median household income, the unemployment rate and the black population percentage. These variables are traditionally associated with crime, and were strongly correlated with demand (Figure 2.7). Therefore, it was important to control for this demographic variation across neighborhoods, or any effects observed from internet installation that could have been due to differences in income or race that were uncontrolled for by Google Fiber demand.

2.4. Model and empirical strategy

Below is the benchmark model (1):

$$Crime_{it} = \beta_0 + \beta_1 Install_{it} + \beta_2 state_i + X_i\Phi + preReg_i + \delta_t + \varepsilon_{it} \quad (1)$$

In model (1) the dependent variable $Crime_{it}$ was a measure of the crime rate per 1,000 inhabitants in a particular neighborhood *i* at a date *t*. The variable of interest in this study was $Install_{it}$, which was a dummy variable that records a "1" if Google Fiber installation by neighborhood *i* had occurred by time *t*. In the Google Fiber data, access was available to the installation start date. If the date was on or past the start date, $Install_{it}$ recorded a "1". As there were many time effects that were not able to be controlled for, δ_t included fixed effects for every date in the sample for each model. For model (1), $preReg_i$, $state_i$ and X_i were control variables that represented dummies for each distinct preregistration rate of Google Fiber by neighborhood, the state in which a neighborhood was located and various demographic controls. The model was identified through $Install_{it}$ which varied by neighborhood and by date net of controls. Standard errors were clustered at the neighborhood level throughout.

There did not appear to be any idiosyncratic differences between Kansas and Missouri with respect to various demographic characteristics and Google Fiber preregistration levels (Figure 2.7). While Fiber preregistration was strongly correlated with various measures of income; within the preregistration level, these relationships were similar for the two states. However, as noted in the results subsection below, Missouri had a higher crime rate than Kansas. Because of this, there may have been idiosyncratic differences across states, or police departments, that were unable to be controlled for in this model.

2.4.1. Preregistration versus neighborhood level controls

Each individual preregistration level was controlled in the model detailed below with a separate dummy variable. This freed neighborhoods that pre-registered for disparate levels of Google Fiber to differ non-linearly in average crime effects, allowing the model to control for differences across preregistration levels, rather than using neighborhood level dummies in the main specification.

There were 50 separate preregistration dummies utilized in the benchmark model. This was in contrast to the 203 dummy variables that would have been incorporated into the benchmark model if neighborhood dummies had instead been used. Therefore, by utilizing the preregistration controls, more variation was allowed in the variable of interest, $Install_{it}$, although the possibility of introducing omitted variable bias was increased, which would have been controlled for with the use of neighborhood dummies. However, as a robustness check, neighborhood level dummy variables were used instead of preregistration level dummies. Similar results were found from this specification; these are discussed in the robustness section.

2.4.2. Controls for potential omitted variable bias

Testing for the effects of increased internet availability is difficult because of the possible simultaneous effects of increased internet availability and other factors correlated with crime, such as income. For instance, it may be the case that neighborhoods with a high Google Fiber demand adopt technologies at a faster rate than the average. These neighborhoods therefore may have an increased willingness to pay for new technologies, such as a higher percentage of income that is not completely captured by median household income. The distribution of preregistration levels is shown in Appendix Figure B1. The most common preregistration level

(given a width of four percentage points) was between 8 and 12, which occupied 18 percent of the sample (Figure B1).

By controlling for every date in the sample period, from 2012 to 2013, any date effects were controlled for that may have been unable to be controlled for explicitly.

2.4.3. Internet roll out and temporal displacement

The model design described above has an advantage over previous crime research concerning incapacitation. The roll out of Google Fiber represented an infrastructure change that continued past the point of the installation date. This meant that individuals who may have been self-incapacitated as a result of the increased internet availability would permanently have the same and potentially a higher opportunity cost after the installation. Therefore, data regarding individuals who could temporally displace their criminal activities to a later date would not be potentially misconstrued as a reduction in crime but rather as an increase in the spread of crime over time.

This finding was relevant in comparison to Dahl and DellaVigna (2009), who found negative effects of daily violent movie attendance on aggravated assaults. As a larger number of people attended violent movies, the percent of aggravated assaults decreased. However, the authors did not find persistent medium-run effects of violent movies on behavior (positive or negative) several days or weeks after the movie had been shown. One explanation of this was that movies only incapacitated individuals for a relatively short amount of time; therefore, although individuals were able to temporally substitute crime for a violent movie, they would be involved in the same amount of violence later in the week. This finding was notable for showing that criminals can potentially temporally substitute and/or delay their behavior from one day to

the next. This research was directly related to this paper, as when Google Fiber had been installed, it was always in effect. This prevented the ability of a potential criminal to temporally substitute and/or delay their criminal activity, as they could have with watching a violent movie.

2.4.4. State control rationale

If Google decided to select Kansas City, Kansas to initially deploy Google Fiber because that city had a lower average crime rate, we could expect the average crime rate associated with a positive β_1 coefficient to be reduced, even within demand levels, as Kansas had Google Fiber installed earlier. This would then appear as though the installation itself lowered crime rates, but the effects observed would essentially be the differences in average crime rates between Kansas and Missouri, as one assumption of this model (without controlling for states) was that there were no difference between states, only within demand and demographics. This would therefore generate spurious results.

Given the relatively large difference in crime rates noted in the following section, it was important to control for differences across states. Some of the types of effects for which this enabled control included idiosyncratic police department effects, Kansas state law or Missouri state law related effects, and other omitted variables.

2.4.5. Weighted least squares

As the population of neighborhoods becomes smaller, the variance of any crime rate increases for two reasons. First, the crime rate is defined as the number of crimes divided by the population. As the population between neighborhoods is not constant, the variance of the crime rates increases as the population counts decrease (Appendix Figure B1 displays the inverse

relationship between crime rates and population). Second, any administrative level errors from the mapping of Census population data to Google Fiber neighborhoods becomes exacerbated as populations decline. One neighborhood may include several sections of other Census tracts; thus, the accuracy of the population of any neighborhood increases with population size. Approximately 30 percent of all neighborhoods have a population of < 1,000 and 70 percent of all neighborhoods in the sample had populations of < 2,000 (Figure B2). Both of these reasons introduce heteroscedasticity into the data. To account for this, weighted least squares were used, where the weights were the resident populations.

In the next section some selected descriptive statistics are discussed, results are shown, and alternative specifications and robustness checks are discussed.

2.5. Results

2.5.1. Descriptive statistics – crime

Crime counts and rates for each Kansas City are displayed in Tables 2.1 and 2.2.[25] Within the state, the year-to-year change in the rate of all types of crime is relatively consistent. However, there are large differences across the state. Kansas City, Missouri reported a homicide rate in 2012-2013 of 0.39 per 1,000, which was nearly double the rate of Kansas City, Kansas, and nearly ten times the national average. For every crime category (except for burglary), Kansas City, Missouri had a higher crime rate. This fact therefore increases the importance of controlling differences across states, as the crime rates are very different.

[25] The population counts reported here represent the Google neighborhoods only, and do not include the entire Kansas City, Kansas or Kansas City, Missouri populations.

2.5.2. Google Fiber installation plausibility

The variable of interest in this study, $Install_{it}$, represented the dates from which Google began to install Google Fiber in the respective neighborhoods. Therefore, one limitation of this study was the inability to ascertain the percentage of households which had Google Fiber installed by a certain date, as installation times varied depending on a number of unknown variables. However, the percentage of households by neighborhood which had pre-registered for Google Fiber was able to be ascertained from publicly provided data. By multiplying this percentage by the number of households reported by the American Community Survey to be in each neighborhood, the estimated total number of households that signed up for Google Fiber was able to be calculated. It was estimated to be approximately 28,938 households, which was similar to the only reported count of Google Fiber subscribers in each Kansas City combined (which was 27,000).[26] However, as this number was mined from the 2015 count, it is unclear how many households signed up for the service during the sample dates included in this study (2012 to 2013). To convert households to population, the number of households estimated to be subscribed to Google Fiber were multiplied by the average population per household (2.4). This resulted in an estimate of a population of approximately 70,000 that could be directly affected by Google Fiber.

As noted earlier, the temporal displacement effects in Dahl and DellaVigna (2009) are unable to be tested for in this study. This allowed for the testing of potential underlying causes of a permanent crime change, as opposed to a potentially transient change like the time spent in a movie theater. This was relevant for policy considerations, as if increased internet availability

[26] This figure originated from the U.S. Copyright Office as reported by BloombergBusiness. It is available at: http://www.bloomberg.com/news/articles/2015-03-12/google-fiber-s-threat-to-cable-is-like-ebola-analyst-says

was a potential cause of crime, this could lead to changes in the way high-speed internet is adopted or pursued.

However, there are two reasons why different crime rate effects would be expected to be identified in this paper rather than in a study such as that performed Dahl and DellaVigna (2009), which investigated the incapacitation effects of violent movies on aggravated assault levels. First, for the types of crime used in this paper, Kansas City had a crime rate that was approximately 2.8 times the national average (FBI 2012). Therefore, any effects observed from a national study may not be applicable to a relatively more dangerous place such as Kansas City. For instance, the incapacitation effects of any proposed intervention might be larger with increasing crime rates, as higher crime rates in a city like Kansas City could indicate that more criminals are committing multiple crimes than the average reported by other cities. Therefore, if an incapacitation strategy works, a greater percentage effect per person incapacitated might then be observed, as any incapacitated individual would be responsible for a larger percentage of the crime rate.

Second, the incapacitation effects hypothesized in this paper would immediately affect whole neighborhoods once Google Fiber was rolled out. Therefore, there could be additional neighborhood-based network incapacitation effects of increased internet availability. For instance, if one or more potential criminal/s in a neighborhood lived in households that received Google Fiber, and their crime network required the participation of all nearby members simultaneously, any crimes he and his network may commit may be severely diminished if one or more members did not participate. This has been noted in the literature, as an individual may commit more crimes if his neighbors are committing crimes (Glaeser et al. 1995, Freeman 1999). Therefore, as more neighbors spend additional time indoors, less criminal networks may form.

2.5.2.1. Google Trends figures – how Google Fiber could change internet behavior

While Google has not released official take-up figures for Google Fiber, it is important to know if, conditional on Google Fiber being installed, subscribers changed their internet behavior when compared to time periods prior to the Fiber roll out. To attempt to proxy this data, data available from Google Trends was used.[27] Google Trends provides search query data by time range, location and search terms in order to view the relative popularity of a particular search term over time. This is calculated by dividing the number of searches of a particular term in a location during a particular week over all searches conducted in that location over the entire specified timespan. This value is then normalized to values from 0 to 100 by dividing the above ratio by the maximum value over the sample period. The benefits afforded by this method are that by comparing two locations, the relative popularity of a search term over time can easily be judged, relative to the volume of searches in a particular location and to the maximum number of searches within the location and sample ranges.

In the following section, several figures of Google Trends curves of popular internet searches are presented. The search terms used were (and remain at the time of printing) among the most popular websites visited in 2013.[28] The search terms are presented alongside the mean populations of Google Fiber installations. This was calculated by multiplying the population of a neighborhood by the Google Fiber preregistration rate, and whether Google Fiber was installed that week. This is then averaged over all of the neighborhoods by week for a value of the average population of Google Fiber subscribers. This allowed for the presentation of the average number

[27]Google Trends can be accessed at https://www.google.com/trends/.
[28] See https://web.archive.org/web/20130815160826/http://www.alexa.com/topsites/countries/US for the top 500 most visited websites in the United States as would have been seen on August 15, 2013

of persons which had access to Google Fiber on a certain week, with the relative popularity of popular search terms in that week, compared to a baseline city. Next, the baseline city, Oklahoma City, Oklahoma is discussed, along with the motivations for choosing this as the baseline city.

2.5.2.1.1. Kansas City and Oklahoma City comparison

To make an apples-to-apples comparison of internet searches over time, the relative search activity in Kansas City was compared to a control city.[29] This is because underlying trends in search activity in Kansas City that were unrelated to Google Fiber roll out needed to be controlled for. For this purpose, Oklahoma City was used as the control. Oklahoma City is a Midwestern city of comparable size to and located near to Kansas City. The median household income was US$45,275 in Kansas City, Missouri compared to US$45,824 in Oklahoma City. The percentage of persons under 18 was 7.5 percent in Kansas City and 7.9 percent in Oklahoma City. Education levels were also similar. The percentage of the population with a high school education or higher was 31.3 and 28.1 percent in Kansas City and Oklahoma City, respectively. The cities were similar in the racial makeup of whites, with the percentage of individuals which identified as "white alone, not Hispanic or Latino" being 54.9 percent in Kansas City and 56.7 percent in Oklahoma City. A larger share of the population in Kansas City was black, 29 percent versus 15.1 percent; however, there was a comparably larger Hispanic or Latino population in Oklahoma City, 10.0 percent versus 17.2 percent. This resulted in the percentage of individuals

[29] Google Trends will only allow the selection of Kansas City, Missouri and not Kansas, so throughout this section, Kansas City, Missouri is used. Because of this, only the relevant Google Fiber installations on the Missouri side was used in the Google Trends plausibility section.

who were black, African, Hispanic or Latino being 39.9 percent in Kansas City and 32.3 percent in Oklahoma City. The full demographic comparison is presented in the Appendix Table B3.

2.5.2.1.2. Google Trends – search terms figures

The search terms presented in Figures 2.9.1 to 2.9.5 represented a large section of internet search term usage. These terms were selected for their plausible relation to internet usage, general popularity and internet bandwidth requirements. They included search terms for video sites or streaming services, social media, search engines and internet browsers. For each figure, the x-axis represents the weeks between January 2012 and January 2014, capturing the full sample range of this paper. The left-side x-axis measures the difference in relative popularity of the search terms between Kansas City and Oklahoma City. The right-side y-axis measures the average population in Kansas City that had Google Fiber installed. If Google Fiber installation had led to an increased usage of relevant search terms, then the two curves should be positively related.

Video search terms

Video-related searches included terms such as ‘youtube,’ ‘netflix,’ and the terms ‘streaming’ and ‘torrent,’ which represent large-sized files typically used for downloading pirated movies, television shows and other media. Video-related search terms were used because, in order to access and view streaming video, the internet user needs a connection capable of streaming or downloading the video. This is relevant to Google Fiber, as the speeds offered by Google Fiber are fast enough to plausibly change internet user behavior by causing more individuals to use the internet for more internet capacity demanding activities than previously. If

a new Google Fiber subscriber did not previously have internet access, or had used a service with slower speeds, the increase in speed offered by Google Fiber would allow the user to access video streaming services (Netflix, YouTube, the word 'streaming') and heavy download sites (torrents), as their internet speeds would be capable of that much activity. The installation of Google Fiber was visually heavily related to increased 'netflix,' 'youtube,' and 'torrent' searches, and weakly related to 'streaming' searches (Figure 2.9.1).

Internet browser search terms

Internet browsers allow individuals to access the internet. The most popular browsers are Chrome (Google), Firefox (Mozilla), Internet Explorer (Microsoft) and Safari (Apple). All internet browser-related search term figures are presented in Figure 2.9.2. Search engines are the gateway to the internet, allowing internet users to find whatever it is they are looking for. A faster internet connection via Google Fiber would allow the user to visit more pages and search for more terms online. Similarly to the previous figure, the search terms 'chrome', 'firefox', 'internet explorer' are visually related to the installation of Google Fiber while 'safari' appeared weakly related (Figure 2.9.2).

Search engine search terms

The search engine searches used were 'google,' 'yahoo' and 'bing.' These represented the three most popular search engines of 2013.[30] The reasoning for using these search terms was that increased Google Fiber access provided more individuals with the opportunity to use the

[30] The most popular search engines in 2013, by parent company, are available at: http://www.comscore.com/Insights/Press-Releases/2014/1/comScore-Releases-December-2013-US-Search-Engine-Rankings.

most popular search engines. This was relevant to this study, as the increased speed provided by Google Fiber allowed an increased number of searches to be performed in a given time frame; therefore, it was logical to hypothesize that if a household had Google Fiber installed, relatively more searches would be conducted per week than the amount conducted before the Google Fiber installation. While it is possible that an increased subscriber base would simply conform to the given average popularity of any search term, it is plausible to think any new user would use the most popular search terms over relatively obscure choices. A strong correlation was seen between the relative popularity of the search engines described above in Kansas City versus Oklahoma City, for the Google Fiber installation period (Figure 2.9.3). For the 'google' search term itself, 'google fiber'-related terms were returned due to the increase in awareness of the Google Fiber project in Kansas City. However, the increase in this search term would have occurred prior to the commencement of installation. The consistent visual pattern for all three search engines represented an increase in search engine use plausibly caused by Google Fiber (Figure 2.9.3).

Social media search terms

The social media searches used were 'facebook,' 'twitter,' 'instagram' and 'pinterest.' These represented popular social media websites, the most popular being Facebook, which a 2013 Pew survey reported was used by 71 percent of all adults and 84 percent of adults aged between 18 and 29 (Duggan et al. 2014). This was relevant as, if increased numbers of internet users are coming online and are younger, there is a greater likelihood of self-incapacitation and possibly the prevention of crime. The relative popularity of searches for 'facebook' in Kansas City increased substantially after Google Fiber was installed compared to Oklahoma City (Figure

2.9.4). There appeared to be a comparable correlation with the search term ‘pinterest’; however, ‘instagram’ and ‘twitter’ appeared unrelated.

Information and shopping search terms

For information and shopping related searches the terms used were ‘wikipedia,’ ‘amazon,’ ‘craigslist’ and ‘ebay.’ These represented Wikipedia, the online encyclopedia (one of the most popular websites), and the most popular shopping websites. These terms were relevant to this study for reasons similar to those stated above. As new users came online with the internet capability to perform an increased number of searches and to access a larger share of content faster, users may have demanded a greater share the most popular content as it was easier to access. The relative popularity of the searches ‘wikipedia,’ ‘amazon’ and ‘ebay’ appeared to be strongly related to the average population who had Google Fiber installed (Figure 2.9.5).

2.5.2.2. Google Fiber trends context

In Figures 2.9.1 to 2.9.5, and through the discussion above, it has been shown that Google Fiber installation plausibly influenced overall internet activity in Kansas City during the relevant time period. As the public data from Google Fiber only shows the percentage of households at a particular time which were to have Google Fiber installed, and not how much the service was used over time, the preceding section sheds light on how the internet was increasingly utilized following Google Fiber adoption.

2.5.3. Benchmark results

Benchmark results are presented in Table 2.3. Across all specifications, no statistically significant effects were identified of Google Fiber installation, $Install_{it}$, on any of the crime rate types used. Each column of the Table represents a new specification for the crime type listed below. Seven crime type rates were used: overall crime, homicide, rape, robbery, aggravated assault, theft and burglary. All crime types were examined as crime rates, with the corresponding neighborhood population in the denominator.

For the control coefficients, the expected signs were found. The coefficient of median household income was negative across crime types, as higher incomes were associated with lower amounts of crime. The unemployment rate and the black population percentage were associated with higher rates of crime, which is consistent with previous studies. The Kansas dummy was negative, which was expected given the high crime rates in Missouri relative to Kansas, as discussed earlier (Table 2.1 and Table 2.2).

Next, two other specifications will be presented that use different interaction terms, such as median household income and demand level. The motivations and results of these specifications are discussed below.

2.5.4. Alternative specification – median household income interaction

In this section an alternative specification is presented with an interaction term, de-meaned median household income (2.1).

$$Crime_{it} = \beta_0 + \beta_1 Install_{it} + \beta_2 Install_{it} * dMHI_i + \beta_3 state_i + X_i\Phi + demand_i + \delta_t + \varepsilon_{it} \quad (2.1)$$

In model (2.1) the installation date for Google Fiber interacts with de-meaned median household income, denoted $dMHI_i$. The baseline $dMHI_i$ was included in the vector Φ. If individuals in a poorer neighborhood are unable to purchase Google Fiber due to budget constraints, then as median household income rises, larger, more noticeable effects of internet availability on crime rates may be observed. Median household income was de-meaned, which involved subtracting the mean median household income of the entire sample from each neighborhood's reported value. The motivation for this was that income is never zero in any neighborhood. By de-meaning, the interpretation of the β_1 coefficient was transformed into the effect of internet installation when median household income is at the average, rather than when it is zero. No statistically significant effects of Google Fiber internet availability were found, including in its interaction with de-meaned median household income, with one exception. Aggravated assault, which was positive, was approximately one fourth of the size of the base income variable and was statistically significant at the 10 percent level (Table 2.4).

2.5.5. Alternative specification – demand interaction specification

Below is an alternative specification where the variable of interest, $Install_{it}$ is interacted with a de-meaned continuous neighborhood demand level variable (2.2).

$$Crime_{it} = \beta_0 + \beta_1 Install_{it} + \beta_2 Install_{it} * dDemand_i + \beta_3 state_i + X_i\Phi + demand_i + \delta_t + \varepsilon_{it} \quad (2.2)$$

This specification allowed $Install_{it}$ to vary linearly across de-meaned demand, instead of in the benchmark specification, where internet installation was assumed to be identical across demand levels. The motivation for this specification came from the hypothesis that $Install_{it}$ may not have the same effect across demand levels. Neighborhoods that demanded Google Fiber at higher rates may already have had access to the internet at faster speeds because these neighborhoods are prone to being heavy users of technology compared to lower-demand households. If this is true, then higher-demand neighborhoods may see no effect of $Install_{it}$, as they already have significant levels of internet service. Demand was de-meaned because there was no possible effect of $Install_{it}$ when demand equals zero, as in that case Google Fiber was never installed. No statistically significant effects of either of the internet installation variables of interest for any crime rate type were found (Table 2.5.1).

2.5.6. Alternative specification – young male share, black share, young black share

In Tables 2.5.2, 2.5.3 and 2.5.4 the alternative de-meaned interaction specifications in the 15-24 year old male share of the population, the de-meaned black share, and the de-meaned 15-24 year old black share of the population are presented. These alternative specifications were designed to test whether Google Fiber roll out and any possible incapacitation effects were

related to the share of the population that was more likely to be arrested or to become a victim of a crime, namely, young males and black individuals. No statistically significant effects were identified other than one coefficient corresponding to burglary (Table 2.5.2) which was negative and significant at the 10 percent level.

However, the any potential effects of internet availability related to age share might vary non-linearly. Therefore, another alternative specification was conducted where $Install_{it}$ was interacted with five equal quantiles of 15-24 year old male share of the population. This specification allowed for the test of effects for very high young male shares or low, independent of each other. Consistent with the rest of the findings of this paper, no statistically significant effects were found across all crime types (unpublished data).

2.5.7. Alternative specification – count models

Another alternative specification could be the use of the Poisson or negative binominal models traditionally used with count data. This may not have been the optimal modal for use in this study, as the dependent variable of interest was the crime rate, not the crime count. However, no statistically significant effects were found by changing the model type to any of the traditional count models and utilizing crime counts instead of crime rates (unpublished data).

Next, a series of robustness checks are presented with different controls or sample restrictions.

2.6. Robustness

The robustness results are presented below. These included restricting the demand range to include demand levels with the most variation, using neighborhood-level fixed effects instead of the demand and demographic controls, and collapsing the unit of time from the date to the week.

2.6.1. Robustness – demand range restriction

By including all the possible demand responses in the benchmark model, the non-linear effects for each demand level were controlled for, and the effect of internet installation was assumed to be the same across demand levels. This may not have been the case for very high or very low demand levels. For instance, extremely high demand levels may have experienced no effect from installation if these households already had access to high quality internet services. There was also little variation in $Install_{it}$ at the end of 2013 between states (Figure 2.5). With little variation between installation dates, and with minimal treatment days in 2013, any crime effects of internet installation may have been small or non-existent. To mitigate this and to test the samples with the widest across-state variation in internet installation, demand lower or equal to 56 percent and greater than 17 percent was used. This range excluded the highest demanders that had representation only in Missouri and not in Kansas, and the lowest demanders that had extremely limited variation across both states. Similarly to all other specifications, no statistically significant effects of internet installation were found (Table 2.6.1).

2.6.2. Robustness – neighborhood fixed effect controls

In the benchmark model, state, demand level, and several demographic characteristics were able to be controlled for. This model was designed to control for differences across neighborhoods by the above controls, with the assumption that there were no other differences by neighborhood that were related to internet installation. In this robustness check, the assumption of the above controls was relaxed and neighborhood level dummy variables for each neighborhood were included. The motivation for this robustness check was that the benchmark design may have suffered from omitted variable bias. Neighborhoods may have been intrinsically different despite having identical demand levels and other characteristics. The magnitude and sign of the coefficient of the variable of interest, $Install_{it}$, within crime type were similar to the benchmark results presented earlier (Table 2.6.2). This showed that model (1) above was able to control for idiosyncratic differences across neighborhoods in a way that was similar to neighborhood level-fixed effects, without losing as much potential variation as in this robustness check.

2.6.3. Robustness – collapse by week

In this specification, the benchmark specification was tested but the unit of time was collapsed from the date to the week. This potentially reduced the noise in the sample at the cost of fewer observations. Net of the controls in the benchmark specification, the effect of internet installation, was assumed to be identical every day after installation began. Therefore, it was plausible to expect similar results after collapsing by week, with the added benefit of potentially reducing day-to-day noise. No statistically significant effects of installation by week were identified, similar to all other specifications (Table 2.6.3).

2.6.4. Robustness – event study and within state tests, within state trends

In the following specification, an event study framework was used, where relatively short time intervals were examined before and after the date of Google Fiber installation in each neighborhood. This allowed comparison of the potential effects of Google Fiber installation in a way that was similar to Dahl and DellaVigna (2009), and to potentially observe the temporal displacement of crime as individuals initially adjusted their behavior upon receiving Google Fiber. This was accomplished by shortening the sample ranges to seven, 14, 28, and 35 days before and after the installation date rather than the entire sample. In this way, potentially noisy and unrelated crime effects occurring unrelated to the installation 'event' were able to be removed, at the cost of observations. No statistically significant effects were identified regardless of date window chosen (unpublished data).

Additionally, as the variation of Google Fiber within the states allowed the identification of any potential effects of internet availability, robustness versions of the benchmark regressions were conducted, but run separately within Kansas and Missouri. This specification is designed to check for potentially different effects within Kansas or Missouri separately. For instance, it may be the case that one state but not the other experienced effects of Google Fiber installation, or each state may have had opposite effects. Also, there may have been unobserved state-specific time trends that previous specifications were unable to control for. To mitigate this, I conduced another set of specifications which included state-specific time trends. No statistically significant effects were found across all crime types within each state or using the state-specific time trends (unpublished data). This implied that Missouri and Kansas behaved similarly, in that neither state

felt any crime effects from Google Fiber installation, nor did they experience opposite and potentially cofounding crime effects.

2.6.5. Overall results - distribution of coefficient signs and power

In the preceding sections, several tables have been presented which showed that no evidence of crime effects of increased internet availability was found. This is despite the Google Trends evidence from section 5.2.1 which showed that internet behavior appreciably changed after Google Fiber adoption. Therefore, these results indicated that no crime effects were observed despite the adoption of Google Fiber.

Additionally, across any given specification, the coefficient on $Install_{it}$ changed within crime type, appearing random. If it was the case that the standard errors on the coefficient of interest were too large, then consistent signs from the coefficients would still be expected. However, the coefficient on the overall crime rate changed almost evenly across every specification, as did many other types of crime.

However, the null results of this paper could have been due to low power, and as a result would have been unable to detect sufficiently small effects. The standard errors, if taken at face value, implied that the coefficient for the overall crime rate in the benchmark specifications would require a magnitude that represented a nearly nine percent decrease in the overall daily crime rate. From the literature, Dahl and DellVigna (2009) find a one million increase in movie theater attendance caused a decrease in same-day assaults of two percent. However, this figure used the data for the United States as a whole, while Kansas City has different demographic profile. For instance, Kansas City (in the sample used in this paper, overall across both cities) had an average black population per neighborhood of 38 percent. The United States had a black

population share of 12.8 percent in 2004, the last year in the Dahl and DellaVigna (2009) study. The arrest rate for black individuals was 6.6 percent, 2.2 for the rest of the population. As Kansas City had approximately three times the black population share as the rest of the United States, and black individuals have been arrested at a higher rate, the comparable "violent movie" effect for Kansas City would have been 2.9 times larger. Also, of the million in movie theater attendance, 28 percent are estimated to have been in the 18 to 29 year old range for mildly violent movies. This group represented 0.58 percent of that same age cohort in the United States in 2004, the last year in the sample used in Dahl and DellaVigna. However, unlike their research, the study conducted in this paper was unable to have directly observed how many individuals used Google Fiber. If that same percentage of young individuals used Google Fiber, and the crime effects were appreciably larger due to the black shares this paper could have potentially arrived at similar observed effects.

2.7. Discussion

This study found no effect of increased internet availability on crime rates. One explanation for this result is that criminals or victims can rearrange their schedules and stay in criminally risky situations even in the face of potentially attractive options, such as internet access. The null finding here suggested that even if individuals are self-incapacitating online and indoors, the overall crime effect is zero if the individuals only shift their criminal activities to a future time. This important null finding improves our understanding of the complex causes of criminal activity.

It may also be the case that the available data limited the ability of this research to find significant effects. Google Fiber may have been too expensive to reach the most relevant and

criminally risky population, the poor. Although Google offered a substantially subsidized internet plan in addition to its more expensive gigabit plan, as mentioned earlier in this paper, it is not known how many individuals signed up to either plan at each date.[31]

As discerned from the Google Trends figures, it was clear that behavior changed along with Google Fiber expansion. It was also clear that salient time-intensive activities, such as watching movies on Netflix or videos on YouTube, increased substantially during the relevant Google Fiber installation time frame. Therefore, while time spent indoors utilizing the internet may have increased, the total time indoors may not have, as any potential crime effects may have simply been displaced to a future time, potentially taking the place of another activity instead of avoiding crime altogether. This would have resulted in no change in the crime rate while allowing the simultaneous observation of the effects of internet behavior.

Another explanation is that for those who could afford Google Fiber, crime may have been unaffected if individuals only shifted their time between activities that would have been spent indoors already, e.g., from watching television, to the internet. This would have resulted in no net change in time spent outdoors from indoors, and as a result no change in crime would have been observed.

However, the importance of this data set must not be discounted. Another contribution of this study to the literature beyond the null finding was the variation of increased internet availability. While internet availability expanded rapidly across the United States in the 1990s and 2000s, it is rare and notable to have access to daily exogenous variation. Internet availability is provided by internet service providers, who presumably would only invest in the large amount of infrastructure needed to develop an internet service if there was significant untapped demand

[31] Google has thus far refused to release this information, due to proprietary concerns.

in the applicable area. As previously mentioned, there could be many reasons for an untapped demand for internet access in an area. Therefore, the construction of the data set used in this paper is notable. This variation in internet availability could be used in future studies seeking to investigate other potential effects on further important outcome variables besides crime, such as educational achievement, income, health and unemployment, among many others

Chapter 3

Earned Prizes: Group versus Individual Prize Valuations and Effort

3.1. Introduction

The value an individual places on a physical good depends on the context involved, such as whether the good was earned or received randomly (Loewenstein et al. 1994). For instance, some businesses frame the first dollar bill earned on a wall even though that same bill is still only worth one dollar. That dollar bill symbolizes the start of a new business, which is an important context to the individual business owner. In general, goods that are earned have higher individual valuations than randomly received goods.

Another possible valuation context is whether the good was earned as a solo competitor or as part of a group. This context is felt whenever individuals might need to group together to solve a problem, such as in academia, business and sports. In tennis the most prestigious awards come from the solo competitions despite the existence of extensive doubles tournaments. In a singles match what the tennis player does reflects directly on herself. In doubles matches it may not always be clear who is responsible for the result of the competition. Therefore, the valuation of a prize awarded to the winner of a singles competition could differ significantly than a member of a winning doubles team. In addition the effort spent in a group competition may differ from an individual competition.

Solo or group competition structure is relevant to firms deciding whether to hold a competition (e.g. sales) with the option of grouping employees together or letting them compete as separate individuals. The firm will be interested if the effort expended by its employees in a

group is any different than competing individually. Some members of a group might realize the potential to shirk and still receive a prize, which could result in less effort expended for the firm. Alternatively, group members might enjoy increasing returns to effort if they are involved with a team from increased potential cooperation and knowledge sharing opportunities. Also, employees who participate in the competition might value the prizes awarded differently depending on whether they were solo or part of a group. This is especially relevant to firms who want to make sure that all competing employees, win or lose, still see the prize as a relevant and worthy goal. If post-competition, the losing solo competitors value identical prizes less than the losing employees in a group competition, the firm might avoid potential unhappy employees by switching to a group structure.

This paper presents results from an experiment designed to test whether the valuations and efforts of competitively awarded prizes differ between groups and solo individuals. I utilize the Kahneman et al. (1990) experimental design, which awards prizes to participants and elicits valuations in a real money market. Three separate conditions were tested: a solo competition, a group competition, and a random lottery assignment.

This paper reports two new findings as a result of a novel experimental competition structure. First, individuals value prizes and work as hard on average in the group competition as in the individual competition. This implies that group structure matters little to prize valuation or effort produced in the competition. Second, prize valuations change dramatically as a result of winning or losing the competition, and change with the results of being in a competition versus having a prize randomly assigned. This is especially important given the first finding, as effort across groups was very similar, therefore any potential confounding changes in effort from competition structure could not have caused any potential observed changes in valuation, only

the win or loss result of the competition.[32] Additionally, what is driving the difference in valuations between winners and losers is the competition itself, as I find no comparable differences in random assignment. I find that losers of the individual and group competitions value the prize lower than half of its retail value.

3.2. Background

This paper relates to three literatures. The first literature is on the endowment effect. The endowment effect refers to a larger than expected gap in the difference in willingness to sell and willingness to buy between sellers and buyers in a market for a particular good. Under the endowment effect, an individual as a seller values the good more than the same individual would as a buyer, when the individual does not own the good. One reason the endowment effect might exist is that loss aversion is altering the buyers and sellers willingness to buy and pay. By owning the good, sellers would rather avoid losing the good to a potential low-ball offer, than potentially make gains from trade. Similarly, those who do not own the good, the buyers, do not want to incur losses by paying too much for it and bid lower than expected on the good. The endowment effect was first tested experimentally by Knetsch (1989), and again by Kahneman, Knetsch and Thaler (1990). They found existence of the endowment effect with random assignment of the prize, even after accounting for learning. Kahneman et al. (1990) postulate that under an efficient market the ex-ante valuations of prizes should be distributed evenly given a fifty percent probability of receiving a mug through a lottery. The groups that won the mug in the lottery, assuming the distribution of valuations is equal across individuals, should have a similar

[32]This experimental design improves upon Loewenstein et al. (1994) by including a straightforward way to measure effort and by using a competitive framework instead of a non-competitive test.

willingness to sell as the group that lost the lottery. They expect, with no endowment effect, that the median willingness to sell and the median willingness to buy should be similar, and roughly half of the mugs won should be traded. However in their experiments, Kahneman et al. (1990) find an endowment effect, where much less than half the mugs were traded, as a result of a relatively large difference in the willingness to sell and willingness to buy.

The endowment effect is disputed by authors such as Shogren (1994), List (2004) and Plott and Zeiler (2005, 2007). Shogren (1994) found that the substitutability of the endowed goods mattered a great deal, in that if the goods were close substitutes, the willingness to pay and willingness to accept of the participants converged. However, if the goods involved had no clear substitutes, then there persisted a gap between willingness to pay and accept. List (2004) found during laboratory and field tests that psychological based theories, such as the endowment effect, are only relevant for inexperienced traders. Experienced traders, on the other are much more willing to part with their goods and trade. Others, such as Plott (2004) see the endowment effect as resulting from subject misconceptions on the relative value of the prizes involved. Plott shows that the willingness to sell and willingness to buy differences could be caused by how the good's transition to the owner is framed. The language that the experimenter uses could induce relative information asymmetries in the minds of the participants, therefore influencing their valuations.

The second literature examines the difference between earning prizes and receiving unearned prizes. In previous experiments, it has been shown that the manner in which a good is obtained can matter to the individual. For example, earning the good as opposed to receiving it randomly appears to have an effect on valuation, as it has a direct influence on how the prize is framed and interpreted by the subject (Levin 1998). Loewenstein et al. (1994) showed that individuals valued their prizes more when they received them as part of doing well at a task, as

opposed to students who got them randomly assigned. Many studies have shown that playing with so called 'house money' in public goods games can change the outcomes involved. In several experiments, the researchers found that those who earned money legitimately in the experiment contributed less to a public goods game than those who did not (Cherry 2002; Muehlbacher 2009; Carlsson 2010). Clark (2002) found no effect of house money on the amount of total money contributed in public goods games; however this is disputed by Harrison (2007) as improper use of statistical testing. Cherry (2002, 2005) found that the accrual of house money resulted in larger donations to a public good, and observes house money effects when the endowments were heterogeneous among the individuals.

The third literature involves groups and their effect on competitions. The difference between individual and group behavior has been of interest to social sciences, as groups are prevalent in firms and other decision making bodies. Hammond (1961) found that working in small groups produced better answers to undergraduate psychology problems rather than individuals. Kocher (2005) finds that while individuals are not "smarter" players than groups initially, the groups learn much faster than the individuals when competing in the same game. In a set of competition experiments, Harbring et al. (2003) found that average effort increased as the number of individuals awarded prizes increased, and the variability of effort is lower.

This paper adds to the current literature by testing how prize valuation changes when group competitions are used instead of solo competitions. This is the first study that adapts the Kahneman et al. (1990) experimental design to include group competition. In addition, this paper adds to the literature by accurately observing effort by the competitors. Also, this paper replicates the Kahneman et al. (1990) experiment and finds no evidence of the endowment effect when prizes are awarded randomly.

3.3. Experiment Design

These experiments were conducted at University of California, Irvine. The subjects were recruited from the Experimental Social Science Laboratory (ESSL) subject pool who are UCI students. The students became aware of ESSL by on-campus advertising. After signing up with ESSL, potential subjects were emailed about a particular experimental trial's date and time. The subjects could choose to sign up and reserve a spot via the ESSL webpage. No individual subject could participate in more than one trial.

3.3.1. Experiment Design: Random Assignment

In this experimental condition I replicate an endowment effect experiment similar to Kahneman et al. (1990) which will serve as a comparison of later treatments. This is important to determine what, if any, of the valuation effects I observe in my competition tests are due to competition itself or simply owning the prize. This condition involves random assignment of a prize which serves as a control for individual and group competition later.

In the random assignment condition, participants arrive in the room and are seated in a staggered fashion by arrival time. The subjects are shown a mug purchased at the local bookstore, with the University of California, Irvine seal. The participants are told the value, $6.95, shown the bar code on the bottom of the mug, and told that they may exchange the mug at the bookstore for credit. They are instructed that they will win a mug if they win a random lottery. They were told that this mug was theirs to keep if they win. Each subject receives an envelope with a number on it when they enters the room. A die-roll generator was placed on the projector, and if an odd number was rolled, the participants with odd envelopes would win a mug. If there was an even roll, then those with even envelopes would get a mug. The die is rolled

and the mugs are awarded. Afterwards, the participants receive a survey depending on whether they won or did not win a mug. Mug winners will receive a survey that shows their willingness to sell the mug, ranging from $12.00 to $0.50, decreasing in $0.50 increments. Buyers receive a similar survey, except they show their willingness to buy, ranging from $12.00 to $0.50. These valuations are tabulated to form supply and demand curves for the participants, with the market price determined by the intersection of supply and demand. After the market price was determined, trades were conducted with the appropriate individuals with real money. Those who sell a mug will add the market price to their show up fee of $15. Those who buy a mug will subtract the market price from their show up fee and receive the appropriate seller's mug.

3.3.2. Experiment Design: Solo Competition

For the solo competition condition, instead of a random die roll, participants are involved in a competition. This design establishes a control that I can use to test whether a group or solo competition structure is influential in individual prize valuations. This is necessary to observe how individuals value prizes after earning them, and to examine the similarities and differences between individuals earning alone versus individuals earning as a group. Each person had a stack of envelopes and University of California, Irvine experimental lab flyers. The competition was to fill as many of the envelopes as possible in a three minute time span. The top fifty percent of envelope fillers would receive an official University of California Irvine mug[33], the rest would not. After the competition, the number of envelopes filled by each subject was recorded and the same willingness to pay and willingness to sell surveys from the random assignment trial were

[33] These experiments took place over two separate years, 2011 and 2015. As a result, the original mug used in 2011 was not available for the second run in 2015. However, a similar mug, which cost one dollar more was used in 2015. I test whether this difference influences the results of this experiment in the robustness section. I find no influence of the different mug on the results presented in this paper.

distributed. Afterwards, supply and demand curves were tabulated from the surveys, and trades were made for the appropriate individuals.

3.3.3. Experiment Design: Group Competition Condition

For this group competition condition, I use a group-based competition instead of the solo competition used earlier. This expands on the previous condition by observing whether individuals grouped together value identical prizes differently than solo individuals. The subjects were partnered with the people sitting next to them, determined randomly by subject arrival time. Each subject's effort, in the form of filled envelopes, was counted separately. Each group's envelope total was derived from adding each group member's contribution. The competition was the same as earlier, though instead of the top fifty percent of individuals receiving mugs, the top fifty percent of groups received identical mugs. Each member of the group received a separate, identical mug. Each subject then received a willingness to sell and willingness to buy survey. Supply and demand curves were tabulated from the surveys and corresponding trades were executed for real money.

3.3.4. Experiment Design: Why Filling Envelopes with Flyers?

A major issue in experimental competitions is the perception of fairness. For example, if instead of filling envelopes, the competition described in this paper involved performing math problems, the individuals with a stronger math background would predictably do better than those with a weak math background. This could result in an "unfair" framing effect among those subjects with a comparatively weak math background. Those that lost the math competition would perceive the competition as unfair and deliberately rigged towards students who practice

their math skills more often, and might give deliberately poor valuations for any prize offered by the experimenter. Therefore, any difference in valuations between sellers and buyers would be due to an implicit unfairness. It was important to choose a competition that nearly any able-bodied individual could perform adequately at. Filling envelopes with flyers was designed to be one such task. The goal was to choose an activity that would be perceived by the subjects as fair as possible.

Additionally, Experimental Social Science Laboratory flyers were used in the experiment instead of a random piece of paper. These flyers were deliberately chosen to avoid a "pointless" frame in the subjects mind. This framing effect could potentially influence valuations if the subjects felt that the filling of envelopes was a needless task outside of wining the competition. By using the flyers, the participants could hopefully avoid thinking in terms of any "pointless" frame and focus on the competition and valuation elicitation. A summary table of my conditions tested and their descriptions is constructed in Table 3.1.

3.4. Empirical Model

Below I show and describe three empirical models which are designed to test for: (1) valuation effects of winners and losers of solo and group competitions, (2) effort effects across the solo and group competition structures, and (3) valuation effects of competition, winners and losers versus random assignment.

3.4.1. Empirical Model (1) – Group vs. Individual Valuations

$$Value_i = \beta_0 + \beta_1 * Seller_i + \beta_2 * Group_i + \beta_3 * Group_i * Seller_i + e_i \quad (1)$$

The empirical model above uses data from the solo competition trials and the group condition trials only. In this model and all that follow, the *i* subscript represents an individual. This model captures potential valuation effects between buyers and sellers (those who lost or won the competition), those in the group competition versus those in the individual competition, and the interaction between the two. The variable $Seller_i$ is a dummy variable that records a "1" if the subject won the competition, as part of a group or solo. The variable $Group_i$ is a dummy variable that records a "1" if the subject was part of a group, "0" if they were solo. The interaction variable $Seller_i * Group_i$ will only record a "1" if the subject won the group competition.

The purpose of formulating the empirical model in this way is to test for the potential valuation effects of being in a group versus solo, when controlling for the effects of winning or losing the competition. In addition, potential interaction effects of winning the competition as a group or as a solo individual. This is important in lieu of the extensive endowment effect literature documenting higher valuations for those that own, or in the case of this experiment, earned the prize in a competition. Therefore I must control for differences in winning or losing the competition, and net of those effects, determine if there are statistically significant differences across solo or group competitions.

The coefficient β_1 represents the effect of winning the solo competition versus losing the solo competition. The coefficient β_2 represents the effect of losing the group competition versus losing the solo competition. Whereas β_3 represents the differential effect of being in the group structured competition and winning the competition simultaneously.

3.4.2. Empirical Model (2) – Group vs. Individual Effort

$$Effort_i = \beta_0 + \beta_1 * Group_i + e_i \quad (2)$$

The model above (2) represents a test of whether *Effort*$_i$, differs across solo or group competition structures. *Effort*$_i$ is measured as the number of envelopes a subject manages to fill during the competition. This measure is recorded for each individual in the solo competition, and each group member separately of the group competition. *Group*$_i$ is estimated on its own to test for overall effort effects between the competitions. This is an important test of one of the main questions of the paper, whether group structure contributes to changes in effort output. The coefficient β_1 represents the average difference in envelopes filled by a subject in a group competition versus an individual competition.

3.4.3. Empirical Model (1) – Competition vs. Random Valuations

$$Value_i = \beta_0 + \beta_1 * Seller_i + \beta_2 * Comp_i + \beta_3 * Comp_i * Seller_i + e_i \quad (3)$$

The empirical model above uses data from the solo, group and random assignment conditions. This model captures potential valuation effects between buyers and sellers (those who lost or won the competition), those in any type of competition, *Comp*$_i$, versus those in the random assignment condition, and their interaction.[34] As above, the variables *Seller*$_i$ is a dummy variable that records a "1" if the subject won the competition, as part of a group or solo. The variable *Comp*$_i$ is a dummy variable which records a "1" if the subject was a part of either the

[34] To avoid redundancy, the author does not present an alternative specification to (3), where competition is a categorical variable with different values for random assignment, solo competition, and group competition. As the interpretation of the equation in (3) is simpler, easier to extrapolate from, and produces very similar results to values shown in this paper, I present the aggregated competition model and results in this paper instead.

solo or group competitions, "0" if they were in random assignment condition. The interaction variable $Seller_i$* $Comp_i$ will only record a "1" if the subject won either type of competition.

The purpose of formulating the empirical model in this way is to test for potential valuation effects of competition itself versus random assignment, when controlling for the effects of winning or losing the competition, as well as the interaction. Similar to the benchmark test, I must control for differences in winning or losing, as there could be asymmetrical valuation effects of the results of the competition or random draw.

Using the three models above, this paper tests three separate questions. One, whether valuations of prizes awarded in competition depend on group structure. Two, whether effort expended depends on group structure. Three, whether valuations of prizes awarded in any competition are comparable to those awarded randomly. Below in the following section I discuss my results. I find that prize valuation does not depend on group structure but only on the result of the competition. Next, I find that effort is statistically indistinguishable between the group and solo trials. Also, I find that competition produces higher valuations from winners and lower valuations from losers versus the random assignment condition.

3.5. Results

Figure 3.1 displays a supply and demand graph for the group and individual competition conditions. The y-axis is the elicited subject valuation over the retail mug price, and the x-axis represents the quantity. To construct demand, buyers (those who lost the competition) are ordered from highest elicited valuation to lowest. To construct supply, sellers (those who won the competition and now own a mug) are ordered from lowest elicited valuation to highest. Each supply and demand curve is separately constructed for the solo and group competitions. Sellers

(supply) are shown with diamond markers, and buyers (demand) are shown with circles. The dark lines represent the solo competition, and the lighter gray lines represent the group competition.

Quantity in this context represents the number of subjects in order of their valuations, for supply or demand. For instance, in Figure 3.1, if we examine the valuation ratio of 0.5 (an elicited valuation of half the retail price of the mug), only one individual seller (solo competition), across conditions has a valuation that low. The interpretation here is that only one mug would be sold if the market price equaled a valuation of 0.5, which equals a quantity of one. Likewise, if the market price in the solo competition equaled a valuation ratio of 1.5 (a price 1.5 times the retail price of the mug), every seller on the supply curve would sell their mug, resulting in a quantity of 26 (the total number of sellers in the solo competition).

From Figure 3.1 we can visually see that demand for the group competition shifted to the right and supply for the group competition shifted to the left. This resulted in an aggregate valuation ratio that is higher for the group subjects than the solo subjects, with minimal change in quantity. However, on the right portion of the graph, the sections of high valuations for supply and low valuations for demand are very visually similar across solo and group competitions.

3.5.1. Descriptive Results – Random prize Assignment

In this section I will describe the results of the random prize assignment, as would be tested under a traditional endowment effect experiment. This allows for the formation of a baseline expectation for what types of valuations and quantities are expected without a competition setting. In the Random Assignment column of Table 3.2, I display descriptive statistics from the random assignment trial. The market price was $5.75 for a mug worth

approximately $7 at the campus bookstore. Out of a total of 25 participants, 12 were involved in a trade at that market price. The average acceptable price sellers and buyers reported were $5.7 and $5.4 respectively. The median prices were $7 and $5.75 respectively. Please see Appendix Figure C1 for a supply and demand figure displaying the results of this trial.

3.5.2. Descriptive Results Valuations – Solo Competition

As shown in Table 3.2, in the solo competition trials, 49 subjects participated. The average market price was $4.65, with an average market price valuation ratio of 0.62. The differences between sellers and buyers were significant, sellers valued their mugs nearly double the buyers on average, using either dollars or the valuation ratio as a metric. Only seven trades occurred, resulting in 29 percent of subjects trading. This is well below the expected average amount of traders as found in the random assignment results (48 percent). The mean and median willingness to sell valuation ratio was 0.89 and 0.87 for sellers, and 0.45 and 0.43 for buyers, respectively.

3.5.3. Descriptive Results Valuations – Group Competition

In the third set of columns in Table 3.2, in the group competition, 48 subjects participated. The average market price was $5.85, with a mean market price valuation ratio of 0.78. The mean and median willingness to sell valuation ratio was 0.95 and 0.93 for sellers, and 0.54 and 0.44[35] for buyers respectively. Of the 48 subjects, eight trades occurred resulting in 16

[35] The power calculation for this experiment was calculated after observing large effects in buyer valuations from the solo to the group competition, with valuations rising by 70 percent in the group structure. To potentially replicate this effect with conventional power levels, 96 subjects were required (with roughly half appearing as buyers and the other half sellers).

subjects participating in trade. This resulted in 33 percent of group competition subjects trading, significantly below 50 percent as found in the random prize assignment but higher than the solo competition trials.

3.5.4. Results: Effort Descriptive Statistics

In Figure 3.2, I present a plot of the valuation ratio versus envelopes filled for the solo and group competition. This figure shows potential effects of effort on valuation. Visually, there does not seem to be any discernable correlations between valuations and effort produced. In Table 3.3, I present descriptive statistics on effort produced from the solo and group competitions. As expected after viewing Figure 3.2, the mean effort produced (envelopes filled) is nearly identical across treatments. The mean envelopes filled for the solo competition was 12.5 and 12.0 for the group competition. There was a difference between buyers and sellers, however this is to be expected given that the sellers must have filled more envelopes in order to win, and become sellers. The mean envelopes filled for sellers in the solo competition was 14.4 and 13.3 in the group competition. For buyers, the mean was 10.4 in the solo competition and 10.8 in the group competition. The maximum envelopes filled in three minutes was 18 in the solo competition, and 17 in the group competition. The minimum was three in the solo competition and six in the group competition.

3.5.5. Benchmark Regression Results - Valuation

In Table 3.4 I present my benchmark ordinary least squares regression results using model (1) described above. Of my variables of interest, only $Seller_i$ has a coefficient that is statistically significant at conventional levels. The magnitude of the coefficient on $Seller_i$ is 0.44

and it is statistically significant at the one percent level. The interpretation of this coefficient is that if a subject won the competition and became a seller, their valuation ratio would increase by 44 percent of the retail value of the mug. The insignificant coefficients on $Group_i$ and the interaction $Seller_i$ * $Group_i$ show that there was no statistically significant difference in valuations between the solo and group competitions and no differences between buyers and sellers across solo or group competitions. However, while insignificant, the coefficient on $Group_i$ is positive at 0.09, showing what we would expect after visually interpreting Figure 3.1 and the descriptive statistics presented earlier.

3.5.6. Benchmark Regression Results - Effort

Table 3.5 displays the effort ordinary least squares results from model (2) described above. This model was designed to test whether forming a competition structure in a group or solo manner will result in the participants producing more or less effort. The coefficient on my variable of interest, $Group_i$, is statistically insignificant at conventional levels. The sign is negative and the magnitude is -0.51 which is expected given the slight decrease in mean envelopes filled across conditions that was described earlier.

The results of this test show that across both types of competition, effort was indistinguishable. This result makes the interpretation of the coefficients in the valuation regressions simpler to understand. As effort appears to be unrelated to competition type, any observed differences across conditions would have to be due to the effects of the competition structure itself and the results of the competition, and not how much the subject gave in effort. Also, this result shows that competition structure does not contribute to effort produced. This is

notable as previous literature such as Harbring (2003) has found increasing effort levels as group size expands.

3.5.7. Competition versus Random Assignment – OLS Results

Table 3.6 displays the ordinary least squares results from model (3). This model was designed to test whether forming any competition structure will affect prize valuations versus randomly receiving the prize. The coefficient on *Competition* is statistically significant at the one percent level, the sign is negative and the value is -0.27. This is interpreted as a 27 percent (relative to the retail price) decrease in valuation if the subject lost any type of competition versus random assignment. The coefficient on *Seller*Competition* is equal to 0.38, which is positive and statistically significant at the one percent level. The interpretation is that 38 percent increase in valuation (relative to the retail price) if the subject won any type of competition versus random assignment. The coefficient on *Seller* is positive but statistically insignificant.

The results of this test show that across any type of competition, valuations are much higher or lower depending on whether the subject won or lost the competition. The act of simply owning the prize, which the *Seller* variable captures, is insignificant. This shows that what matters is winning or losing a competition, not simply owning a good.

3.5.8. Descriptive Results – Endowment Effect Predictions

The endowment effect literature such as shown by Kahneman et al. (1990) predicts that in a trial where prizes are awarded randomly, those that end up owning the prize will value it higher than would be predicted by an efficient market. Both hypotheses start with the scenario that we are given a pool of individuals that have an equal proportion of types: those who have

high internal valuations on the potential prize and those that have low internal valuations on the potential prize. If the prize is awarded randomly, then on average half of the high-value types will win a prize and half will not, and half of the low-value types will win a prize and half will not. Therefore, if the experimenter creates a market for the prizes, under an efficient market the low-value half of the group that won the prize will trade with the half of the high-value group that did not win the prize. In an equally proportioned market this would on average result in 50 percent of the prizes sold between subjects.

However, the endowment effect literature predicts that by simply owning a prize, some proportion of low-value types become high-value types, as well as by not owning a prize, some high-value types become low-value types. This results in a portion of high-value and low-value types who would have traded without the endowment effect, now refuse to trade. This effectively reduces the trading percentage among subjects to below 50 percent.

In the baseline random assignment trial performed in this paper, 48 percent of subjects were involved in a trade. Thus the endowment effect in this trial was not observed in a discernable way. Participants traded almost exactly half of the mugs available, corresponding almost directly to predictions of a uniform and equally proportioned distribution of prize appreciation among participants. As this experiment was designed to replicate the results of Kahneman et al. (1990), the finding of no endowment effect whatsoever goes against their results. Therefore, the singular act of owning a good may not be enough to induce an endowment effect result, given the lack of trading observed in this experiment. This could be due to the inevitable language differences between this experiment and Kahneman et al. (1990). If true, this suggests that the way the ownership of the prize is framed to the subject is important as posited by Plott (2004), not solely the act of owning the prize.

3.5.9. Descriptive Endowment Effect Results – Earned Prizes

As noted earlier, the percent of traders out of the total participants is a useful metric for measuring the existence of the endowment effect. If the number of trades is below the 50 percent predicted by the literature, we might suspect that some type of endowment effect is limiting the number of trades. This was observed in the solo and group competition trials, as only 29 percent and 33 percent of able participants respectively participated in the market. This shows evidence of a type of "earned" endowment effect that was also observed by Loewenstein et al. (1994). Subjects who perceived that they were rewarded for performing well, as opposed to performing poorly, valued the prizes higher.

3.5.10. Robustness – Valuation Levels Versus Valuation Ratio

As noted earlier, a result of performing the trials used in this experiment in different years is that the mugs sold by the University of California, Irvine bookstore changed. In 2011, when the experiment began the official mug was 11 ounces, white in color, had a picture of the University of California at Irvine seal, and it cost $6.95. In 2015, when the experiments finished, the mug was still white and 11 ounces, however it featured the school mascot of the anteater in a cartoon style, with no University of California at Irvine seal, and the price increased to $7.95, an increase of $1. If the retail price of the mug has risen, then the valuations from 2011 to 2015 might not be directly comparable. As a result, I used the valuation ratio, which is the elicited valuation of the subject divided by the price of the mug at the time of the experiment. In this way, I control for differences in mug price across trials.

In Table 3.7, I present two robustness specifications where I use dollars instead of the valuation ratio [R1] while using the benchmark specification outlined in model (1). Following

this I utilized the same specification but include a year 2015 dummy variable to control for any other omitted differences across years [R2]. The results are nearly identical across both robustness checks and very similar to my benchmark results presented earlier. The coefficient on $Seller_i$, is positive, significant at the one percent level and equal to 3.36 in both robustness checks. The interpretation of this is that upon winning the competition, regardless of solo or group competition structure, results in a valuation that is $3.36 higher on average. This is unsurprisingly 48 percent of the value of the 2011 mug and 42 percent of the value of the 2015 mug. The average of both of these measures equals 44 percent, identical to the 0.44 coefficient on $Seller_i$ observed in the benchmark results.

3.6. Discussion

This paper examines and finds no effects of the context of competition structure on valuation of prizes or effort produced. This is notable as the literature on competitions has found some evidence of returns to effort from participation in group activity. Therefore this finding on valuation and effort represents a significant departure from the literature. However, I do find large effects of competition itself on valuations. This is a notable extension of the endowment effect literature, and this research shows that simply owning a good has a negligible effect on valuation compared to the effect of losing or winning a competition. This is relevant to firms deciding whether to base potential employee competitions on solo or group structure, or to have competitions at all. As I find no effects of competition structure and no effects on effort, the most cost-saving competition method would be the best choice. Though statistically insignificant, I find limited evidence of higher valuations among the losers of competitions in the group versus

the solo competition structure. This implies that all else equal, firms may be better off pairing up employees to prevent a possible post-competition spite effect on prize valuation.

However, another issue is whether to use competitions at all. As I find large valuation swings between winners and losers of competitions and random prize assignments, firms that seek to reward the most productive employees relatively more would be better off by having competitions. Conversely, firms may seek to avoid potential employee instability by not using a competition. Random assignment dampens the variance of high and low employee attitudes, which perhaps could prevent future potential problems.[36]

This research is also notable for the competition used in the experiment design. By choosing envelope filling as the basis for the competition, anyone who was able-bodied could perform this task adequately. This has two interesting consequences. One, participants were not able to distinguish themselves very much in effort output as nearly all subjects were similarly talented at filling envelopes. Two, by choosing a competition that almost anyone can do, any effect of effort on valuation is effectively controlled for. So while the experiment loses some ability to measure effort given a relatively narrow range, I gain the ability to adequately control for effort expended effects on valuation across competition types, as effort variation was very similar across all conditions.

This paper also contributes to the "endowment effect" literature in a notable way. I observe strong evidence of an earned endowment effect in both types of competition. The percentage of subjects that participated in trading was almost half of the value that that would have been predicted under no endowment effect. This result is in contrast to the random prize assignment in which I find nearly 50 percent of subjects traded, what would have been predicted

[36] Effort may well change as a result of instituting random assignment instead of a competition, but this beyond the scope of this paper.

under no endowment effect. This result differs from the endowment effect literature, which would have predicted a much lower trading percentage than I observe in the random assignment trial. Given that I observe the endowment effect in the earned trials, this supports a version of the endowment effect that applies only when subjects earn their prizes and not when they receive it randomly. By working at a task and earning a prize, the subjects may feel more attached to the prize than those who received it randomly. This may cause the winning subjects to "overvalue" their prizes in relation to their retail price, and in relation to the other subjects who lost.

The losers of the competition, having worked but received nothing in return, may report lower valuations out of a type of post-competition spite effect. This could manifest itself as disappointment, as individuals who tried hard and failed may disregard the experiment and the valuations afterward as unimportant, given that they lost. This would minimize any potential psychological effects of losing the competition in any subject's mind.

A possible future experimental extension of this research includes utilizing comparatively large-sized groups with the same style of competition, to test the effects of increasing group size on effort and valuation. In a similar vein, if future researchers are more interested in the potential of group members to shirk, lengthening the competition or introducing more choices to the subjects could give the subjects more opportunities, and thus more variation in effort expended.

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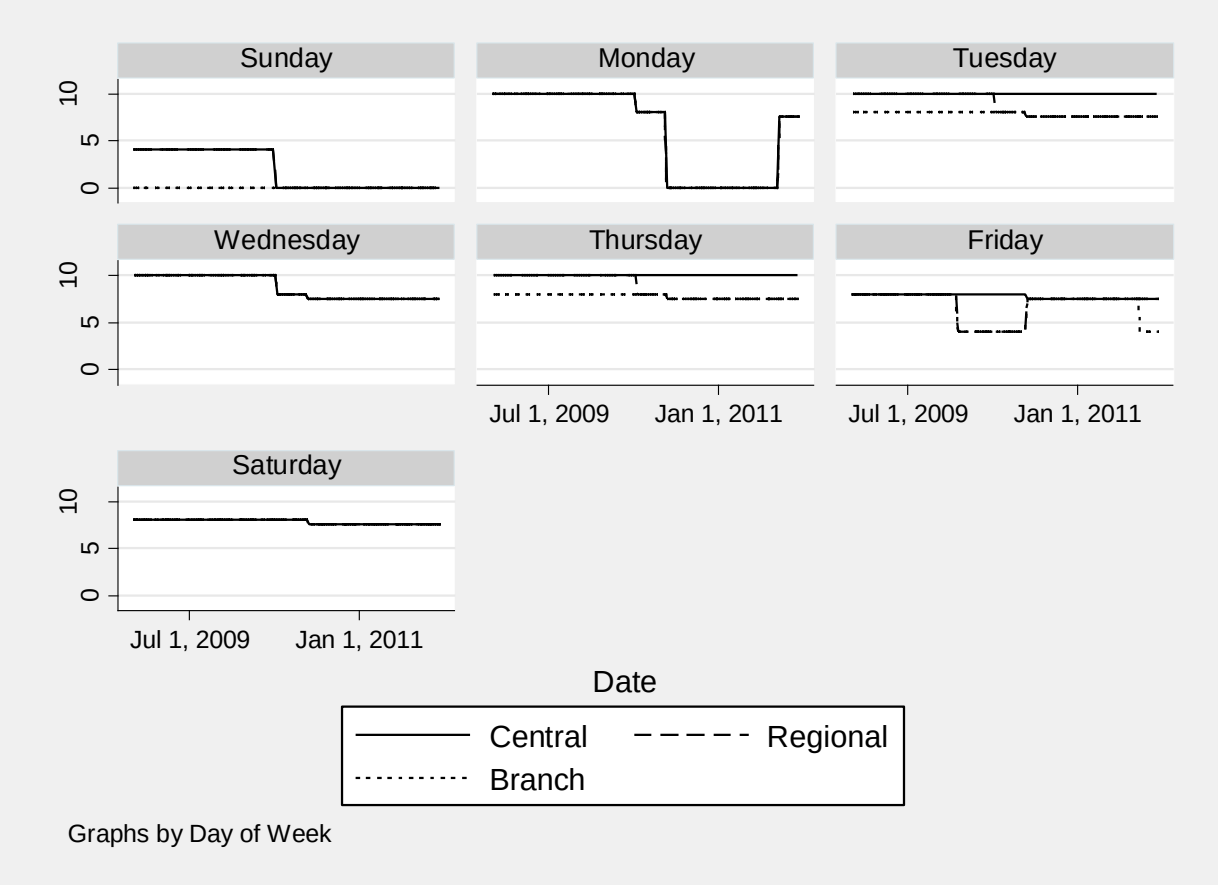
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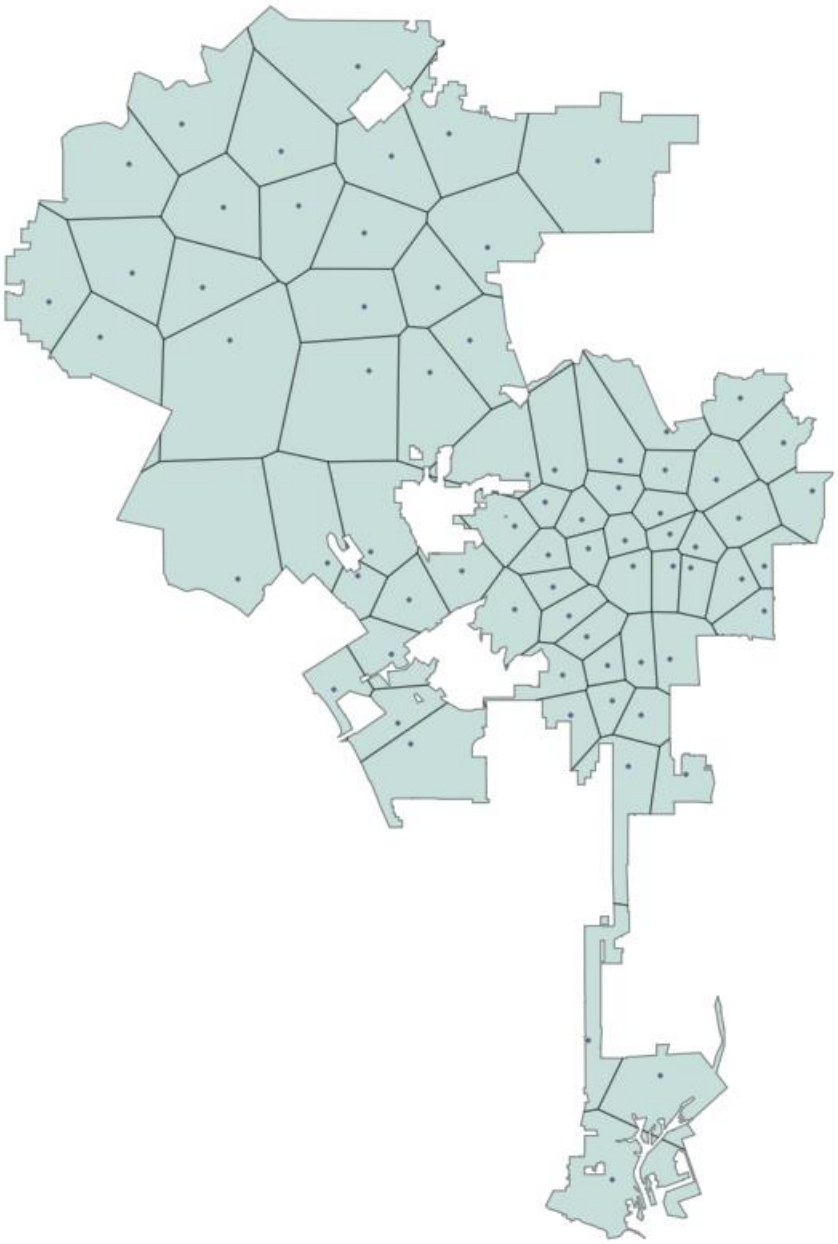
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Figure 1.1—Variation in library hours by library type,
January 2009 to September 2011



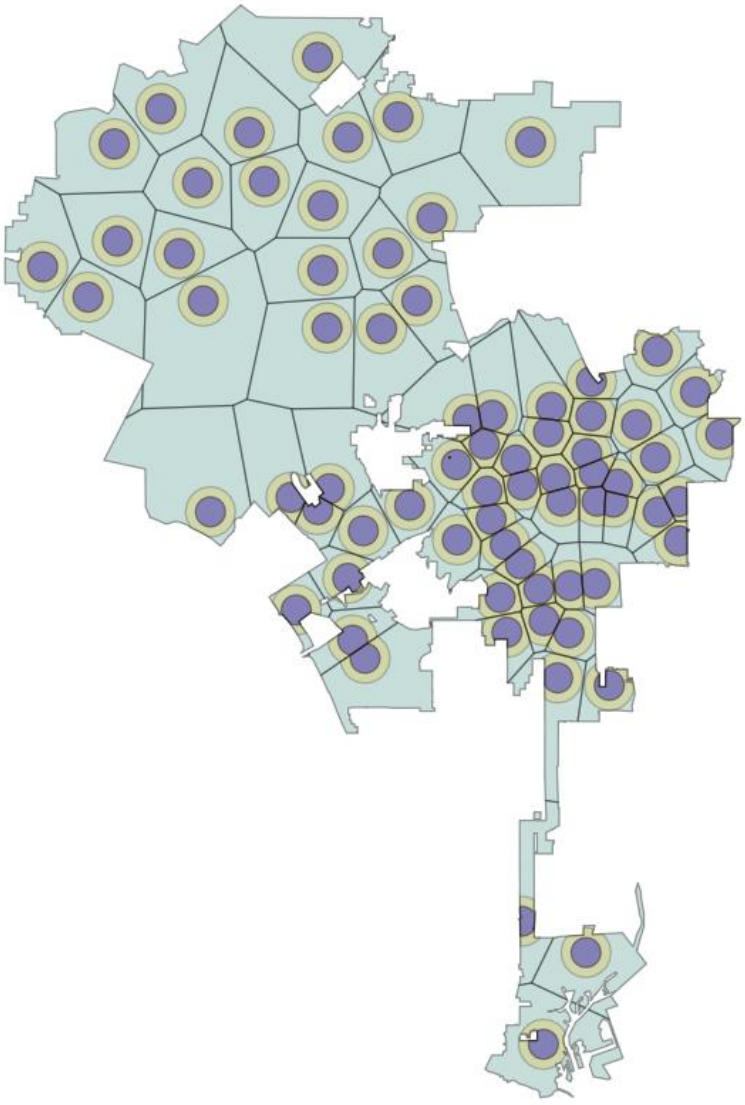
Notes. This figure shows the variation in library hours during the sample period, January 2009 through September 2011, by day of the week and type of library. Library holidays were removed from the figure to cleanly show the intended variation. In the Los Angeles Public Library system there are one central, eight regional, and 64 branch libraries. Holidays were removed to make the figure easier to understand.

Figure 1.2—Los Angeles libraries and library-zones



Notes. This is a map of Los Angeles divided into library zones. Each dot represents a Los Angeles public library. A library's catchment area is calculated as a voronoi polygon, where each library is responsible for all of the area where it is the closest library "as the crow flies".

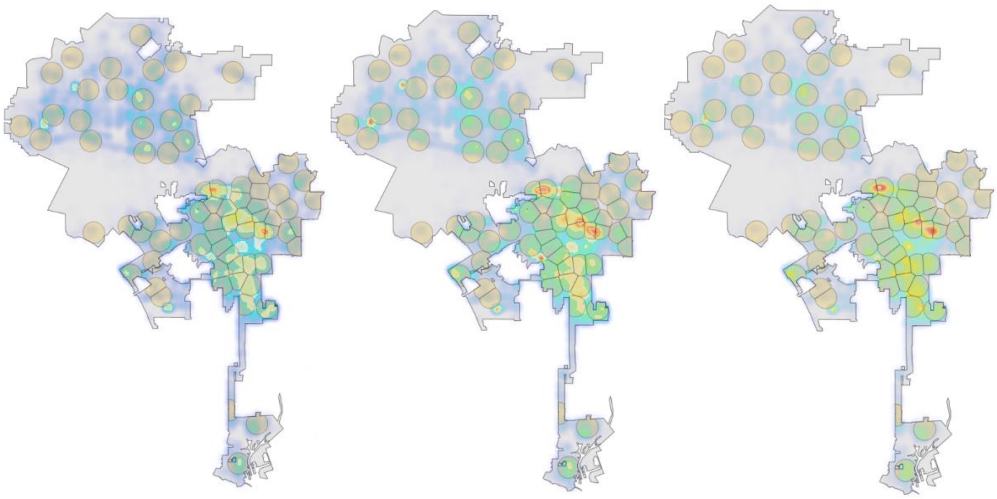
Figure 1.3—Los Angeles by library radii-zones



Notes. This is a map of Los Angeles divided into library zones. A library's catchment area is calculated as a voronoi polygon, where each library is responsible for all of the area where it is the closest library. Within each library zone, two library radii-zones, 1.0 miles and 0.6 miles are illustrated.

Figure 1.4—Los Angeles crime heatmaps (2009 – 2011) with 1.0 mile library radii-zones

A.2009 B. 2010 C.2011 (partial)



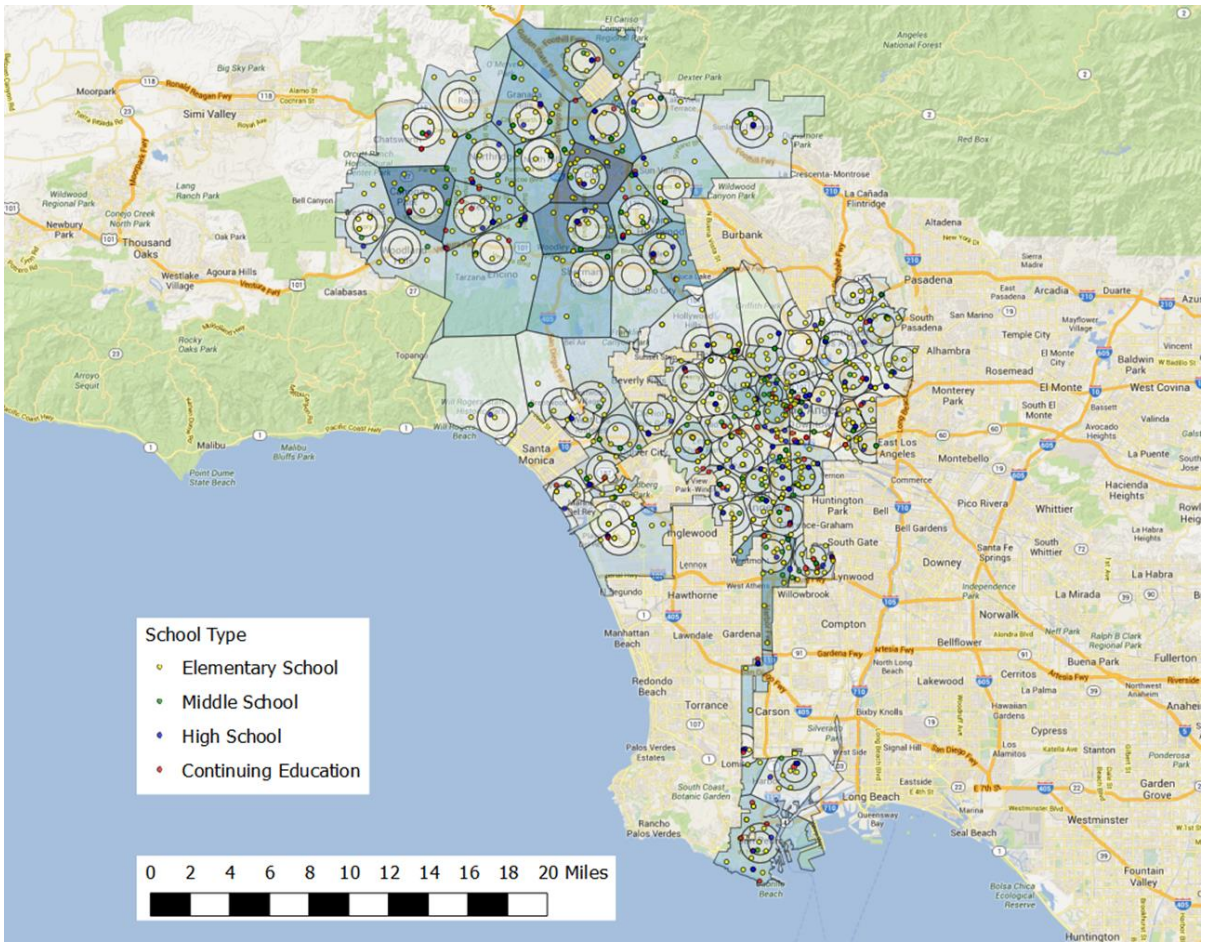
Notes. Each figure is a crime heatmap of Los Angeles for all three years in the sample. 2009, 2010 are complete, while 2011 only extends to September 17th. The heatmap transitions from "cold" colors of blue and green, to "hot" colors of yellow and red. Hotter colors indicate larger crime counts.

Figure 1.5—De-meaned car burglary rate and library hours by date, by day of the week, areas within 0.6 miles of a library



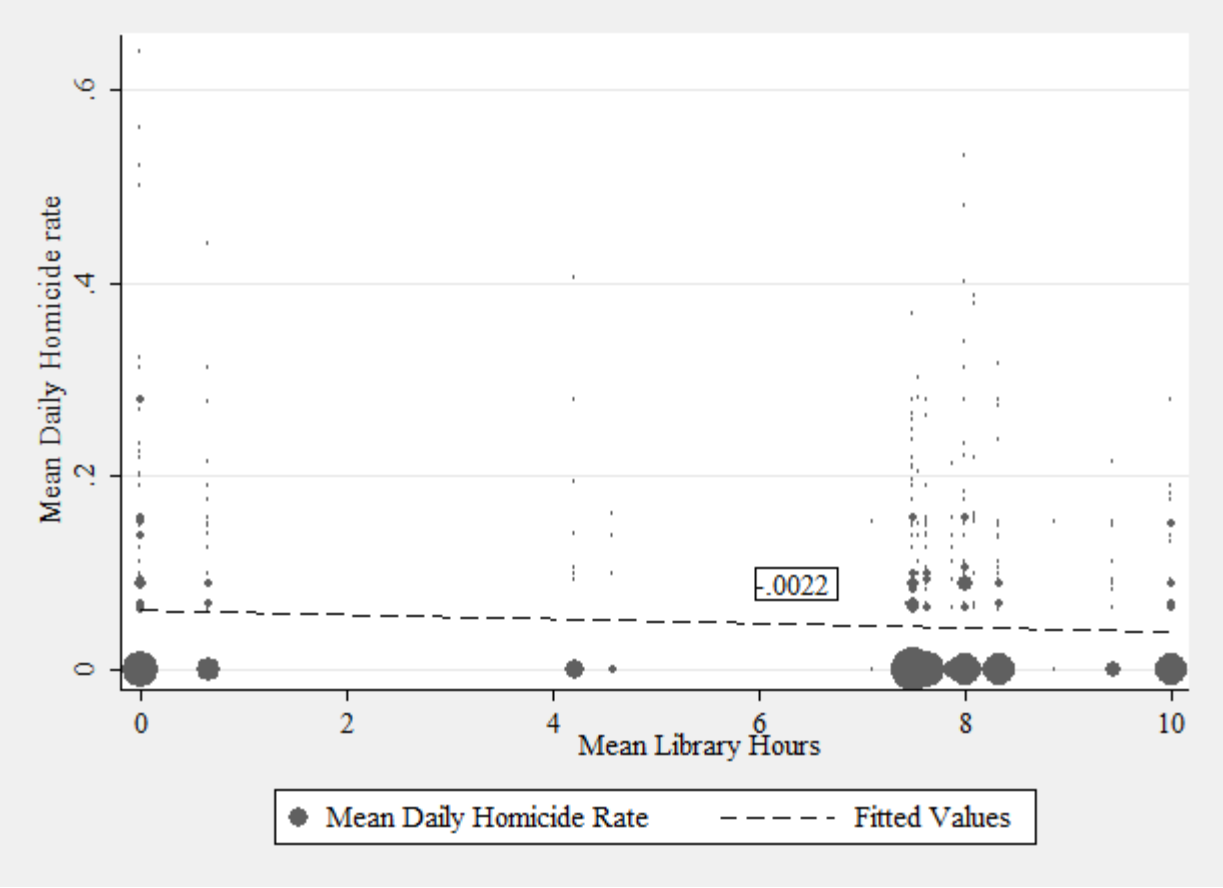
Notes. This graph shows relationship between daily de-meaned car burglary rates by day of the week, for areas within 0.6 of mile of a library, for branch libraries, without holidays and for only the libraries that didn't undergo renovations or grand openings over the entire sample period. The reason for the restrictions is to clearly show the variation in library hours by date and day of the week, without the visually confusing variation by renovations, holiday and library type. In the figure, on Monday and Wednesday, de-meaned car burglary rates rise when the library hours were cut.

Figure 1.6—Map of Los Angeles and surrounding areas, by library radii-zones and Los Angeles Unified School type



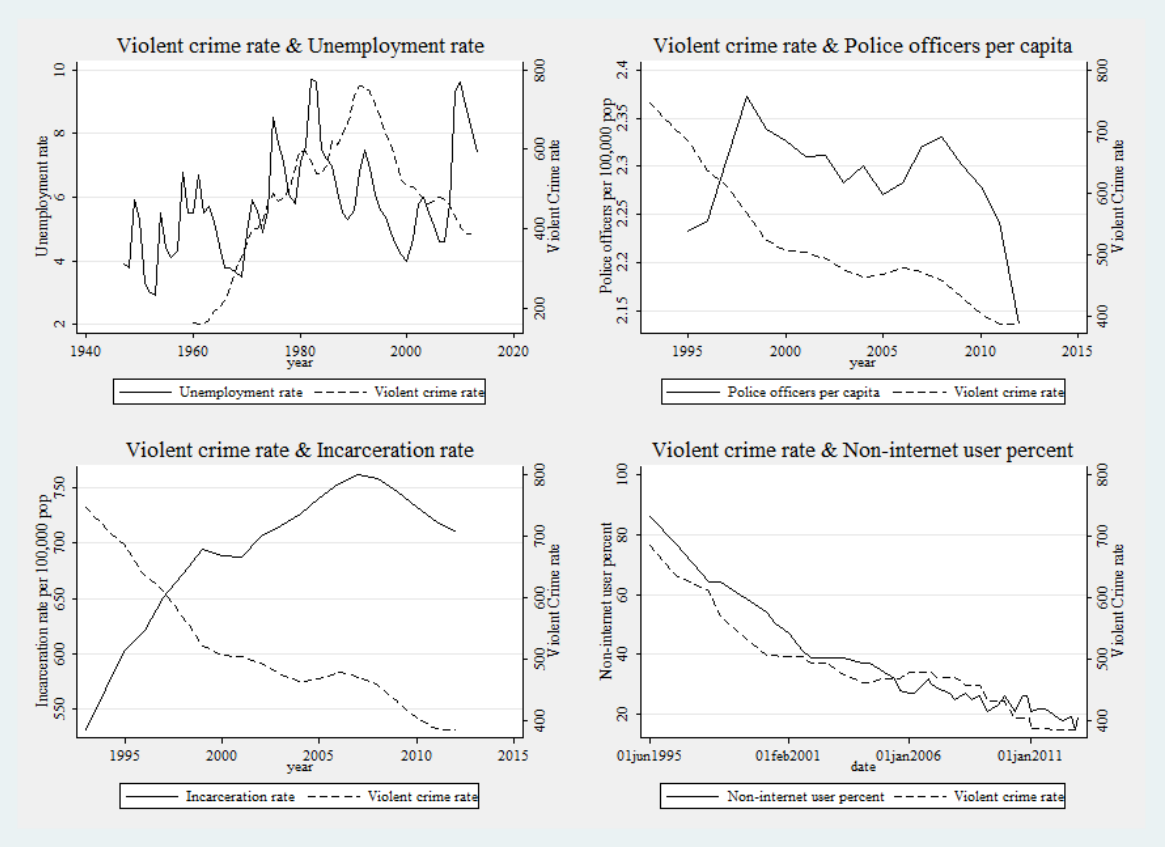
Notes. The above map represents the Los Angeles Public Library radii-zones, overlain over a map of the surrounding Los Angeles County. The dots on the map refer to Elementary Schools, Middle Schools, High Schools, and Continuing Education schools in Los Angeles Unified School district. Darker areas indicate higher levels of population.

Figure 1.7—Mean daily homicide rate versus mean library hours,
January 2009 – September 2011



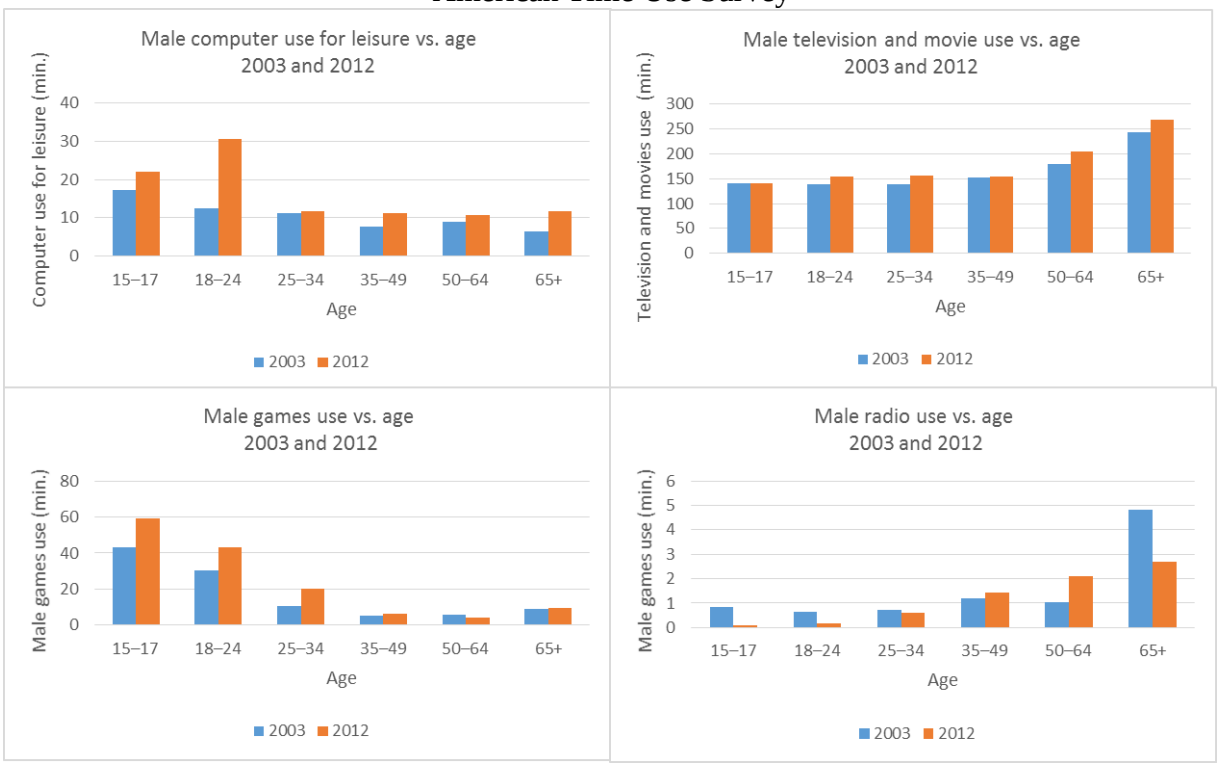
Notes. This graph shows the negative relationship between daily homicide rates for high violent crime areas and library hours. High violent crime areas are those above the 75th percentile of violent crime rates during the pre-treatment period. Marker size denotes how many observations are located at each homicide and library hours coordinate, larger size means more observations. The slope of the fitted line is -.02.

Figure 2.1—Time series plots of the possible explanations of crime and violent crime rate over time



Sources: Bureau of Justice Statistics, Federal Bureau of Investigation, Pew Research
Notes. This figure plots the violent crime rate and various proposed crime determinants. Violent crime includes homicide, rape, robbery, and aggravated assault reported per 100,000 population. Police officers per capita represent the number of police officers in the United States per 100,000 population per year. The incarceration rate represents the number of inmates in Federal, State, local or private prisons per year per 100,000 population. The non-internet user percent is the percentage of the population of the United States that does not use the internet.

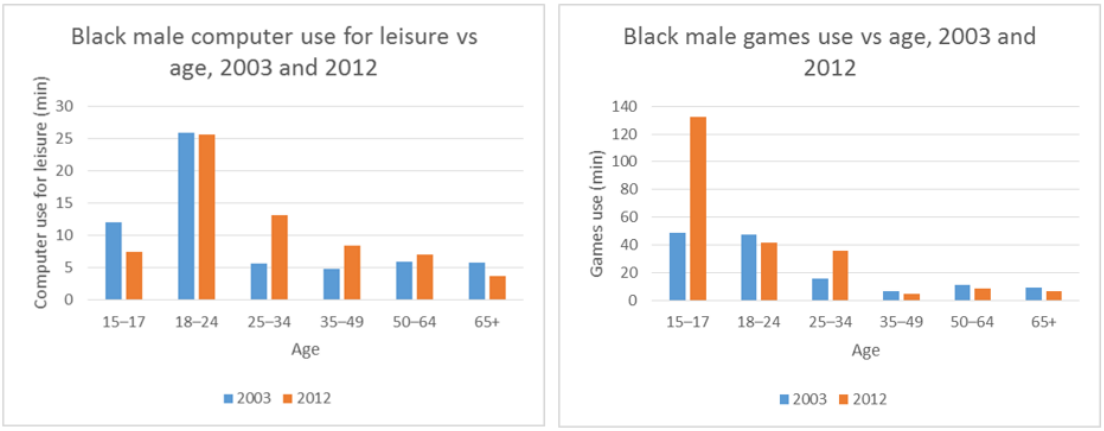
Figure 2.2—Computer, games, television, and radio use by males by age, 2003 and 2012, American Time Use Survey



Source: American Time Use Survey

Notes. This figure shows the usage of computers for leisure, television, games, and radio by age for males during 2003 and 2012, from the American Time Use Survey. As shown in the figure, large increases in the share of time spent using computers and playing games is shown. Computer use for leisure includes any time spent using the computer that does not include games or television use.

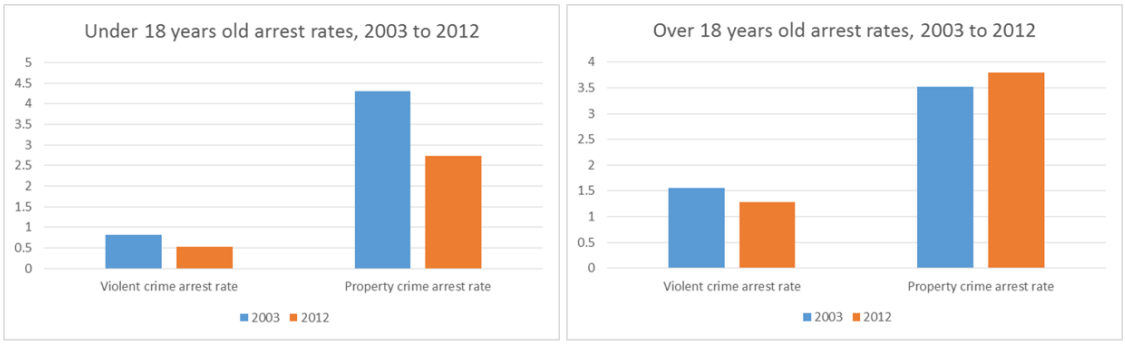
Figure 2.3—Computer and game use by black males by age, 2003 and 2012, American Time Use Survey



Source: American Time Use Survey

Notes. This figure shows black male computer use for leisure and games usage for 2003 and 2012, as reported by the American Time Use Survey. Computer use for leisure includes any time spent using the computer that does not include games or television use. Time spent playing games approximately tripled between 2003 and 2012 for black males. The usage of computers for leisure by black males increased for the age groups 25-34 and 35-49, while remaining the same for 18-24, and declined for those aged 15-17. As computer use does not include games, these figures do not rule out the possibility that young black males increased their time spent using games on the computer. Additionally, these figures provide context and support for the idea that the use of technology as a whole has dramatically expanded from 2003 through 2012 among young black males.

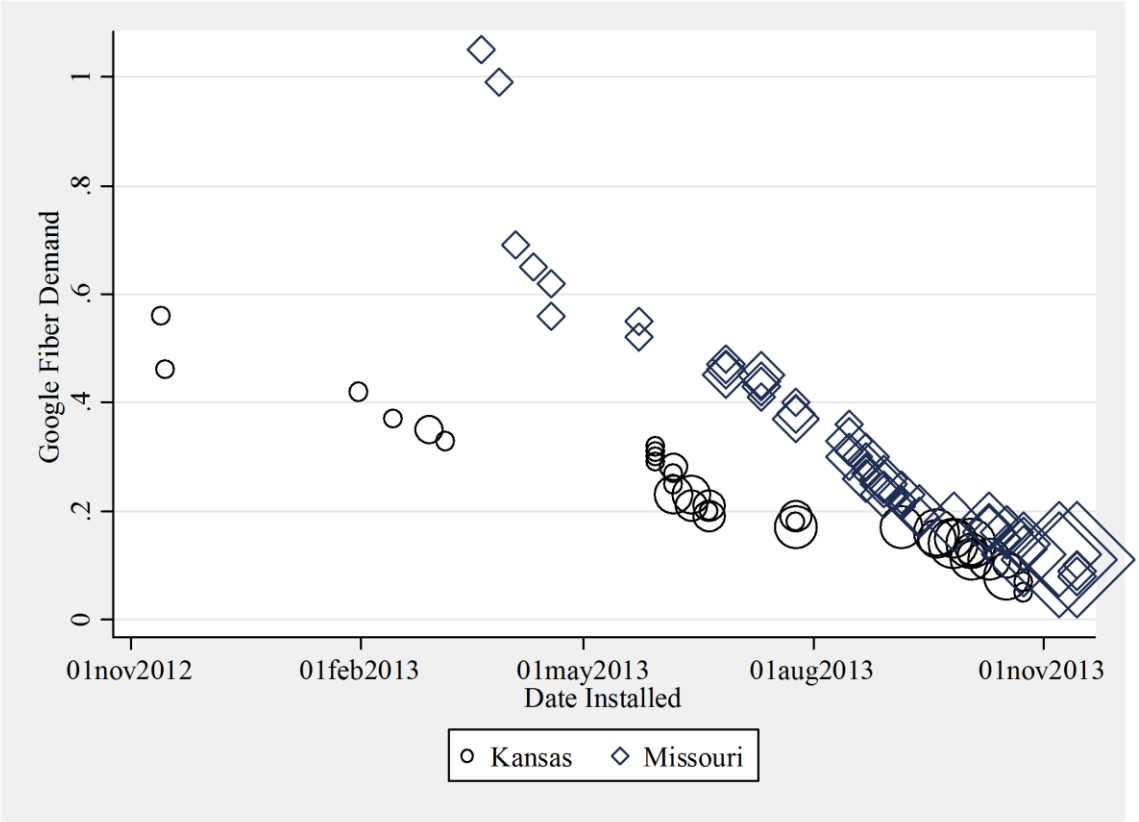
Figure 2.4—Arrest rates by age, type of crime, for 2003 and 2012



Source: American Time Use Survey

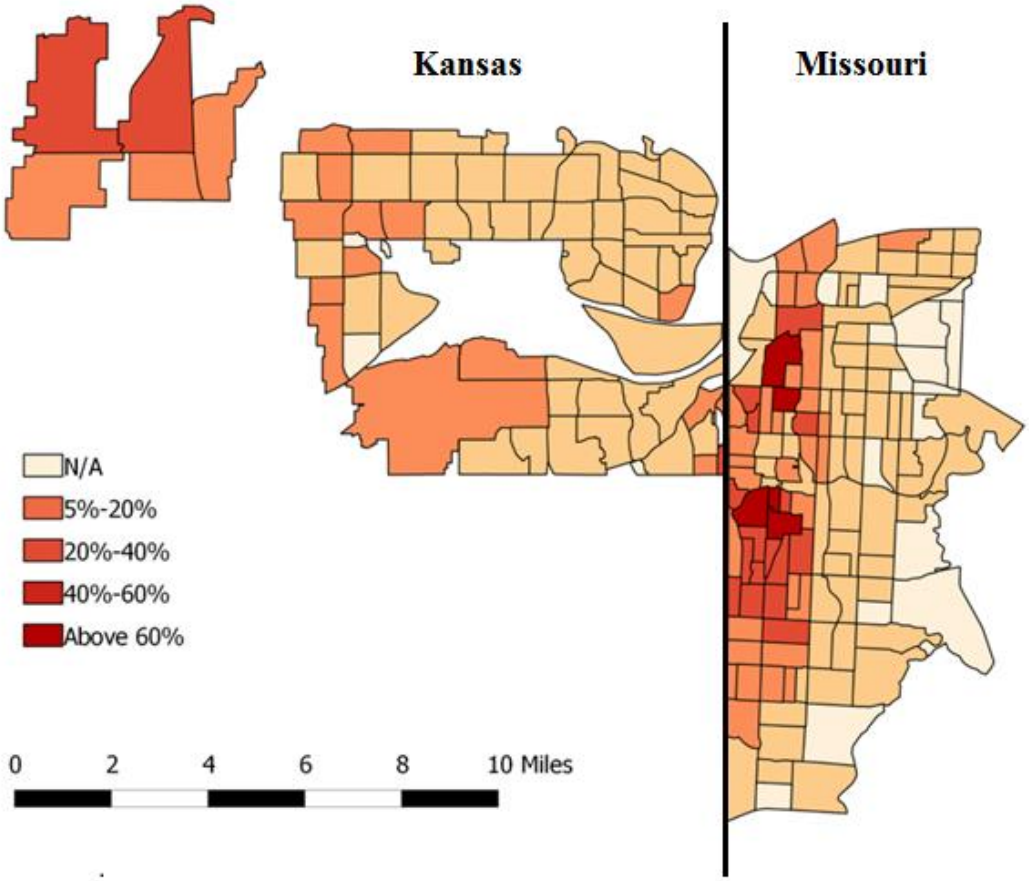
Notes. This figure shows arrest rates by age and violent and property crime rates for 2003 and 2012. The source of the data is the Federal Bureau of Investigation Uniform Crime Reports. As shown in the figure, large percentage changes in violent and property crimes are reported for under 18 year olds. There are decreases shown for those older than 18 years, but not as dramatic as those for under 18 years.

Figure 2.5—Google Fiber demand level vs. date installed, by state



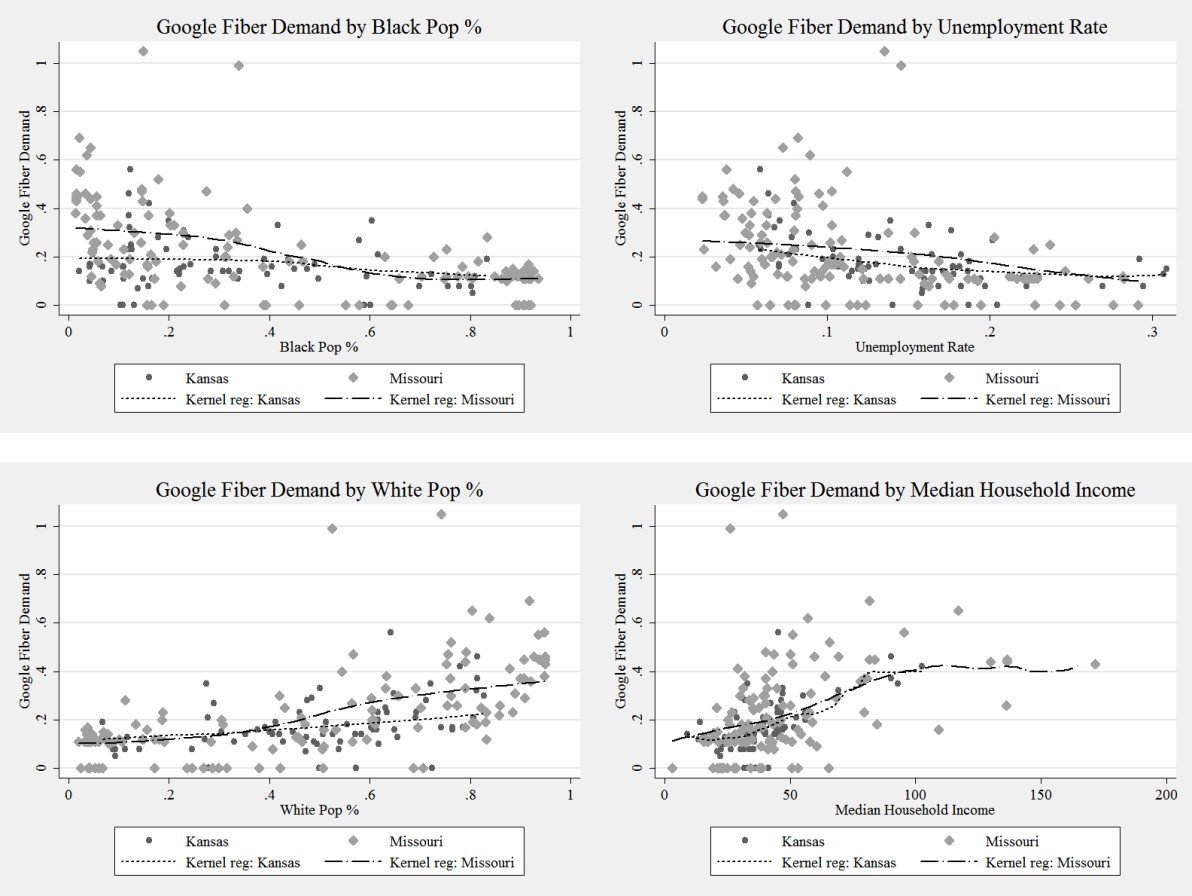
Source: Google Inc.
Notes. This figure plots Google Fiber preregistration rates by the date Google Fiber installation began, by state, showing the variation in internet availability. Preregistration rates are defined as the number of households per neighborhood that signed up for Google Fiber. The circles represent Kansas and the diamonds represent Missouri. The size of the objects refer to the number of neighborhoods at that demand level, per state. For instance, the larger diamonds and circles at the end of 2013 refer to relatively larger numbers of neighborhoods at the lower demand range.

Figure 2.6—Map of Kansas City, Kansas and Kansas City, Missouri, by Google Fiber neighborhood and Google Fiber preregistration level



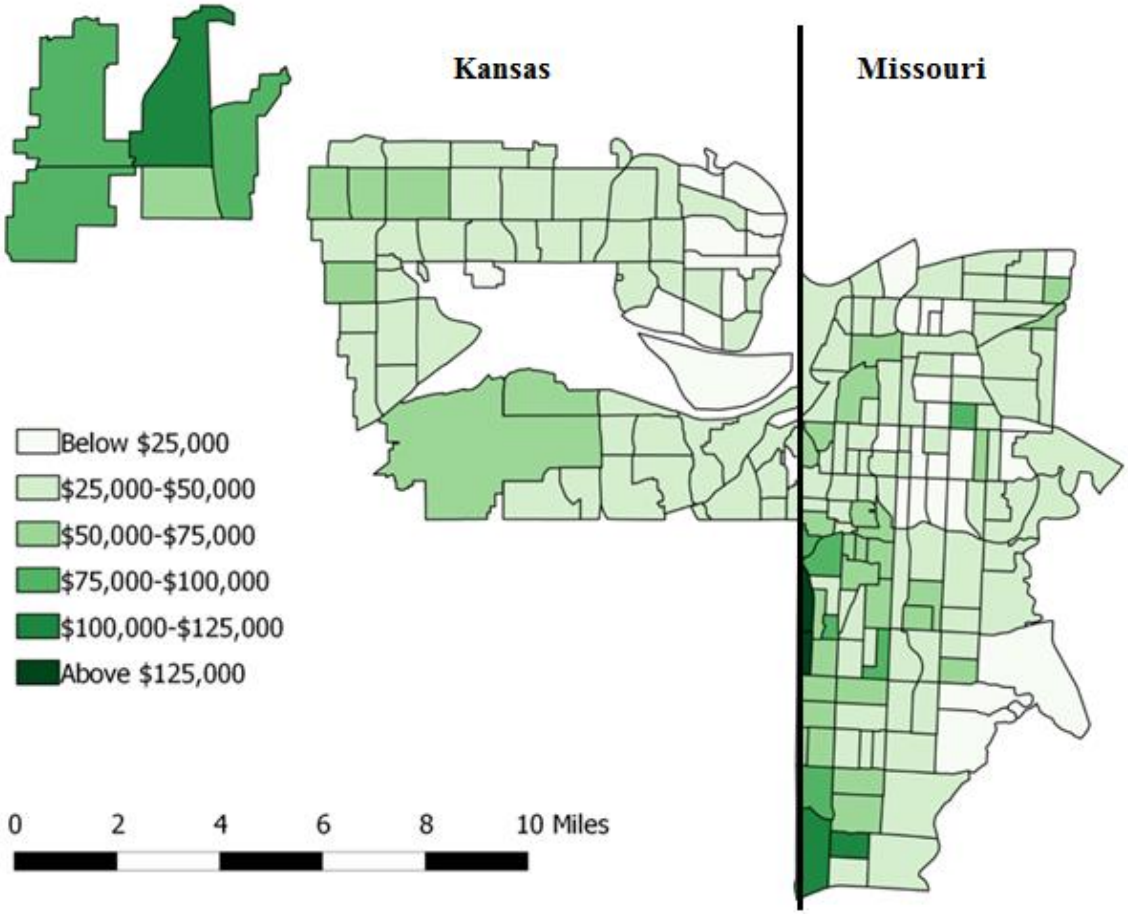
Sources: Google Inc.
Notes. This map shows the variation in Google Fiber demand rates by location, across Kansas and Missouri. Google Fiber demand rates are defined as the number of households per neighborhood that signed up for Google Fiber. Missouri is shown on the right, and Kansas is shown on the left. Empty areas represent places that were not selected to potentially receive Google Fiber as of 2013.

Figure 2.7—Google Fiber demand level by various demographics



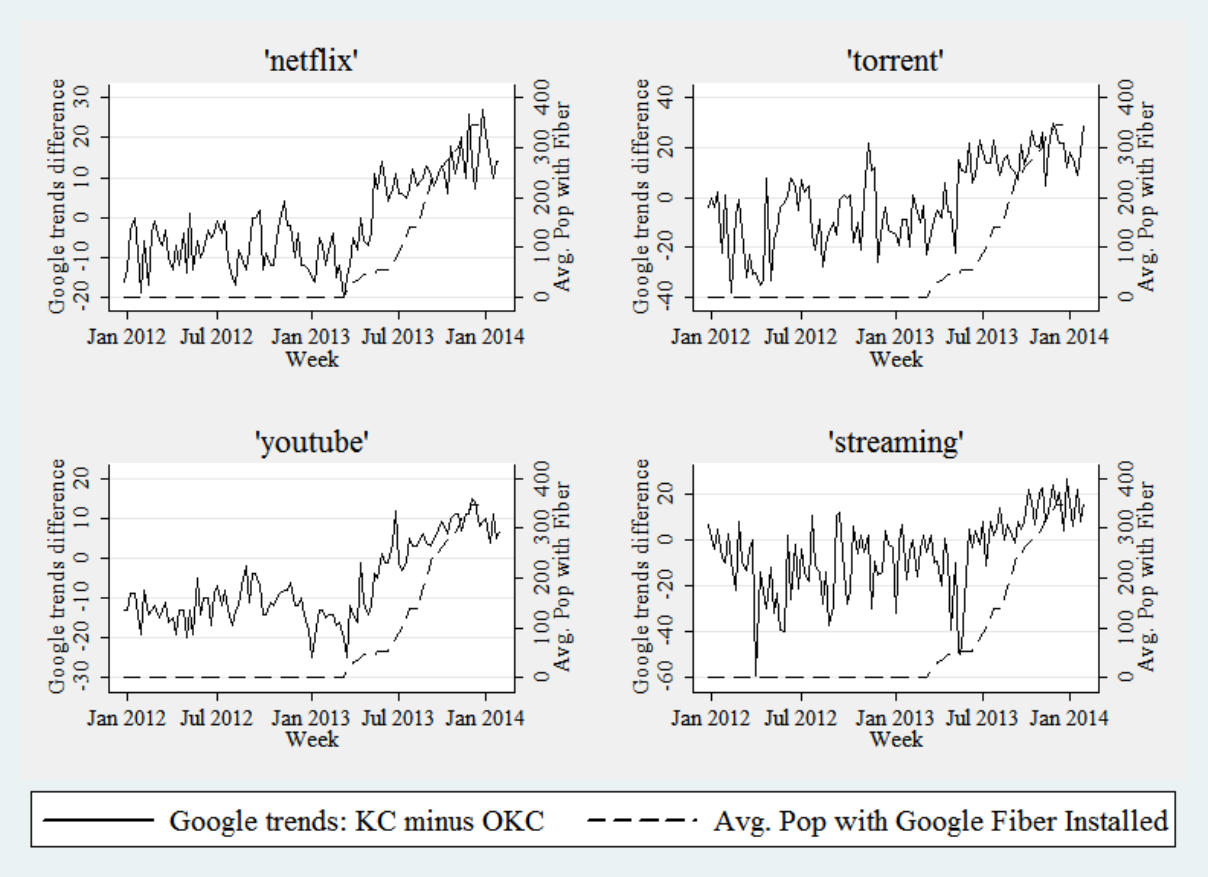
Sources: Google Inc. and the Census Bureau
Notes. This figure plots Google Fiber demand rates by various demographic characteristics and by state. Google Fiber demand rates are defined as the number of households per neighborhood that signed up for Google Fiber. Also, within-state kernel-weighted local polynomial regression curves were used to more easily show the relationships described above.

Figure 2.8—Map of Kansas City, Kansas and Kansas City, Missouri, by Google Fiber neighborhood and median household income



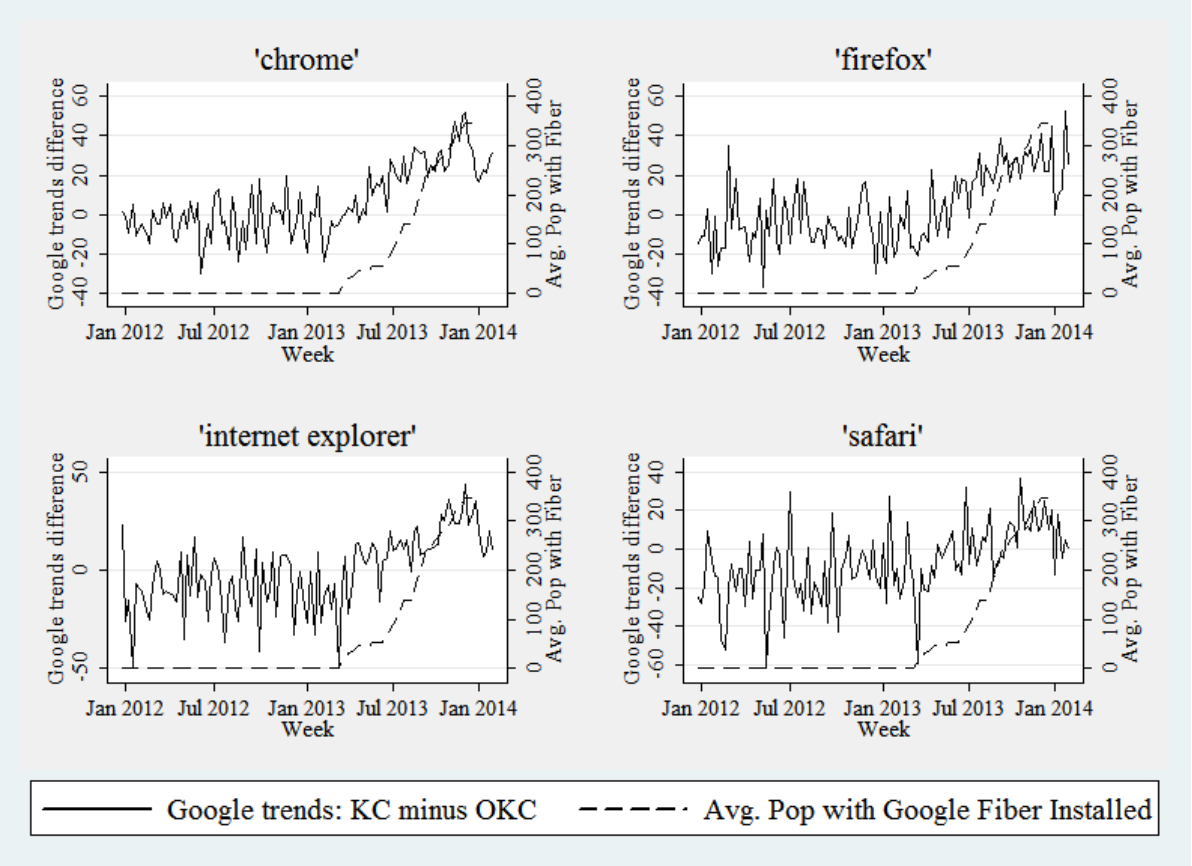
Sources: Google Inc. and the Census Bureau
Notes. This map shows the variation in median household income by location, across Kansas and Missouri. Missouri is shown on the right, and Kansas is shown on the left. Empty areas represent places that were not selected to potentially receive Google Fiber as of 2013.

Figure 2.9.1—Google Trends Kansas City, Oklahoma City comparison, streaming video or heavy download terms



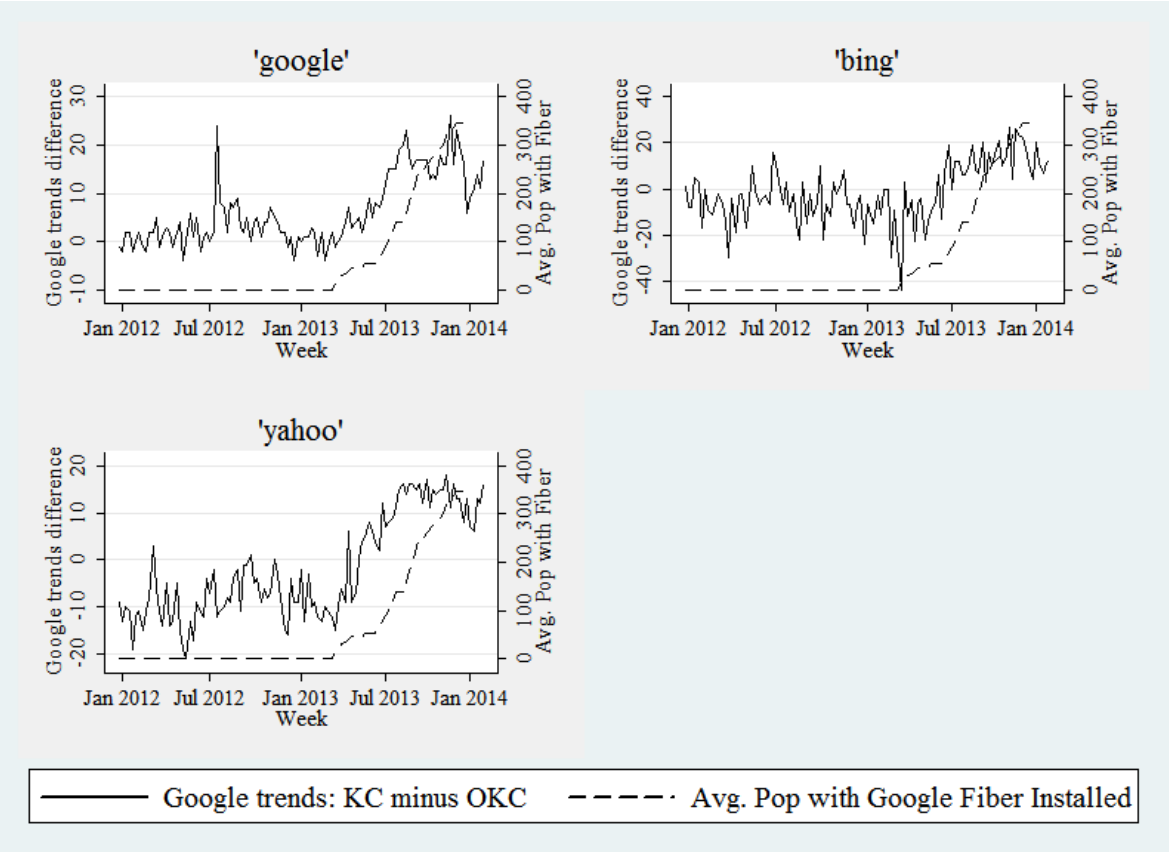
Notes. This figure shows the difference in Google Trends search term popularity between Kansas City, Missouri and Oklahoma City, Oklahoma each search term listed above each figure. Google Trends measures the relative popularity of search terms relative to overall Google searches, and again relative to the maximum value of the search term over a set time range and in a specific location. The time range chosen is January 2012 through January 2014, the time range relevant to the sample used in this paper. The left y-axis represents the Kansas City, Missouri Google Trends value minus the value from Oklahoma City, Oklahoma. The right y-axis represents the average population in Missouri that has Google Fiber installed, calculated by multiplying the neighborhood specific preregistration rate by the population and again by if the neighborhood has had Google Fiber installed. The solid line represents the Google Trends difference and the dotted line represents the average population with Google Fiber installed.

Figure 2.9.2—Google Trends Kansas City, Oklahoma City comparison, internet browser search terms



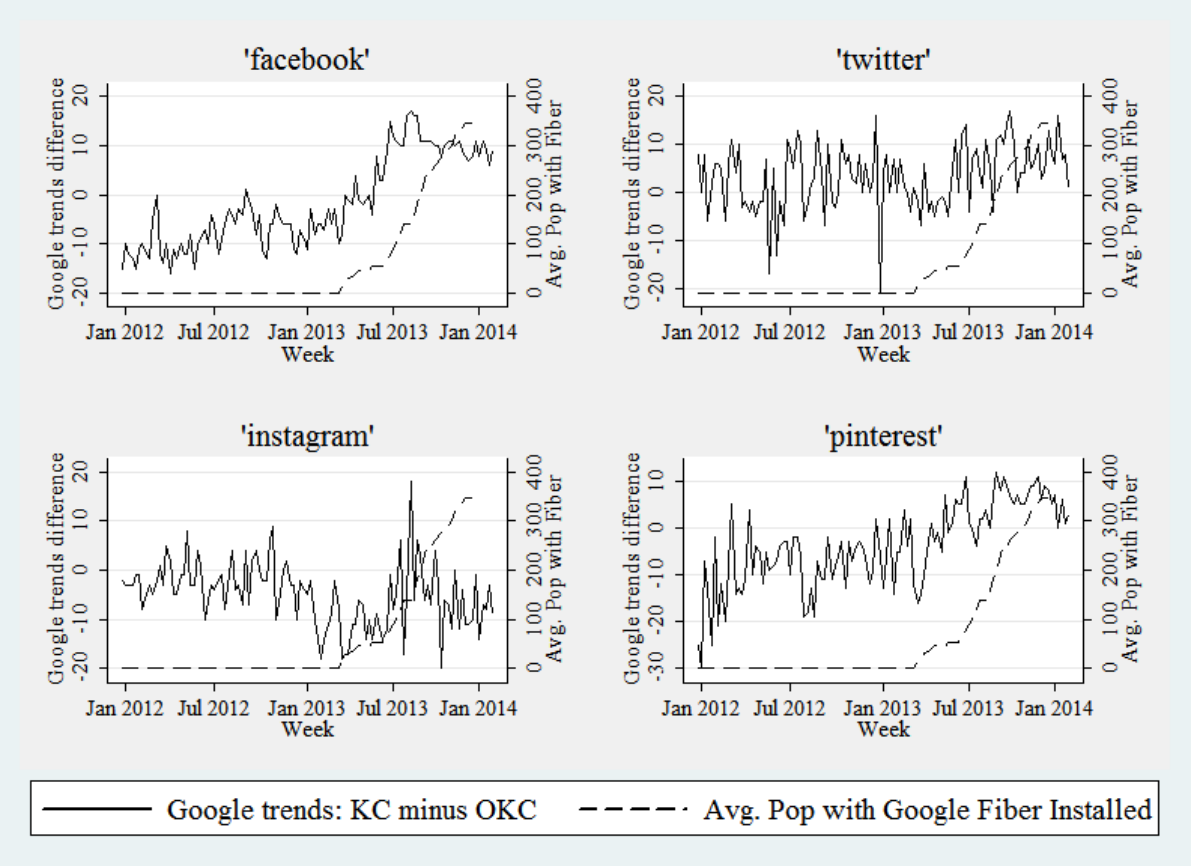
Notes. This figure shows the difference in Google Trends search term popularity between Kansas City, Missouri and Oklahoma City, Oklahoma each search term listed above each figure. Google Trends measures the relative popularity of search terms relative to overall Google searches, and again relative to the maximum value of the search term over a set time range and in a specific location. The time range chosen is January 2012 through January 2014, the time range relevant to the sample used in this paper. The left y-axis represents the Kansas City, Missouri Google Trends value minus the value from Oklahoma City, Oklahoma. The right y-axis represents the average population in Missouri that has Google Fiber installed, calculated by multiplying the neighborhood specific preregistration rate by the population and again by if the neighborhood has had Google Fiber installed. The solid line represents the Google Trends difference and the dotted line represents the average population with Google Fiber installed.

Figure 2.9.3—Google Trends Kansas City, Oklahoma City comparison, search engine search terms



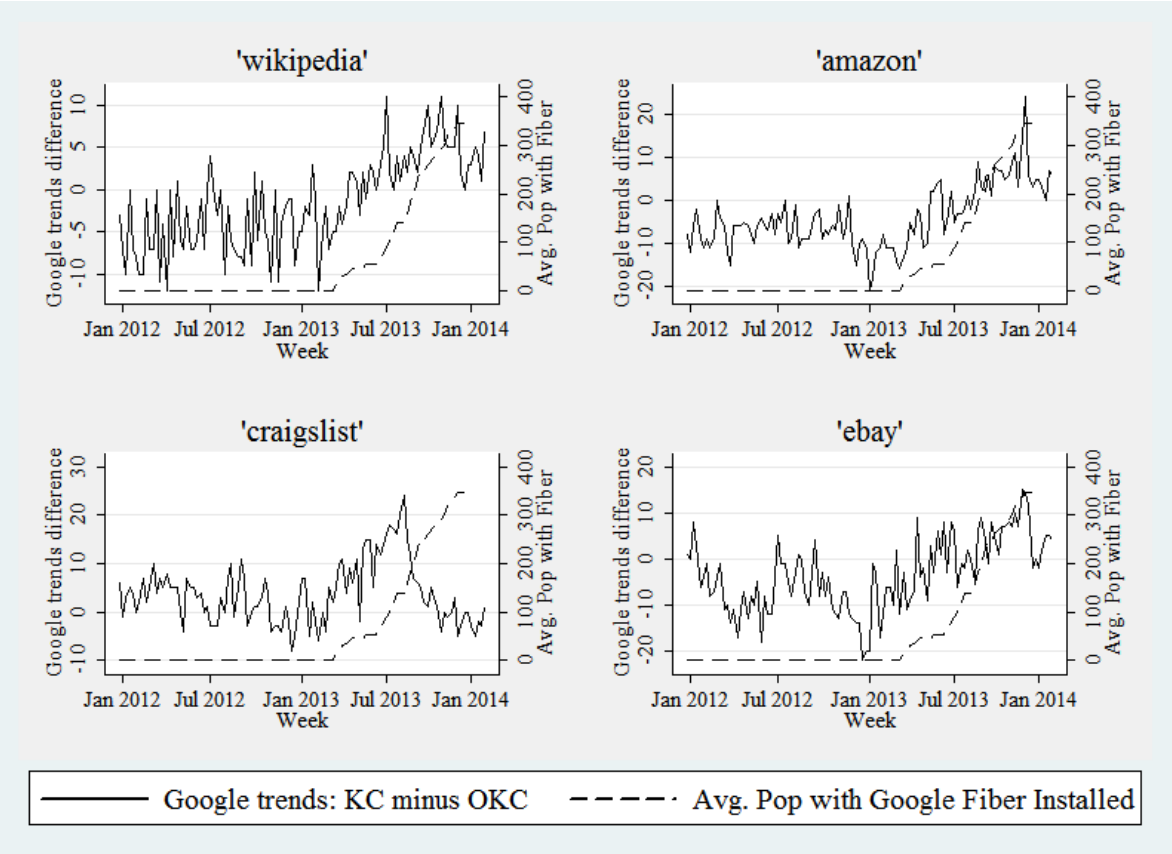
Notes. This figure shows the difference in Google Trends search term popularity between Kansas City, Missouri and Oklahoma City, Oklahoma each search term listed above each figure. Google Trends measures the relative popularity of search terms relative to overall Google searches, and again relative to the maximum value of the search term over a set time range and in a specific location. The time range chosen is January 2012 through January 2014, the time range relevant to the sample used in this paper. The left y-axis represents the Kansas City, Missouri Google Trends value minus the value from Oklahoma City, Oklahoma. The right y-axis represents the average population in Missouri that has Google Fiber installed, calculated by multiplying the neighborhood specific preregistration rate by the population and again by if the neighborhood has had Google Fiber installed. The solid line represents the Google Trends difference and the dotted line represents the average population with Google Fiber installed.

Figure 2.9.4—Google Trends Kansas City, Oklahoma City comparison, social media search terms



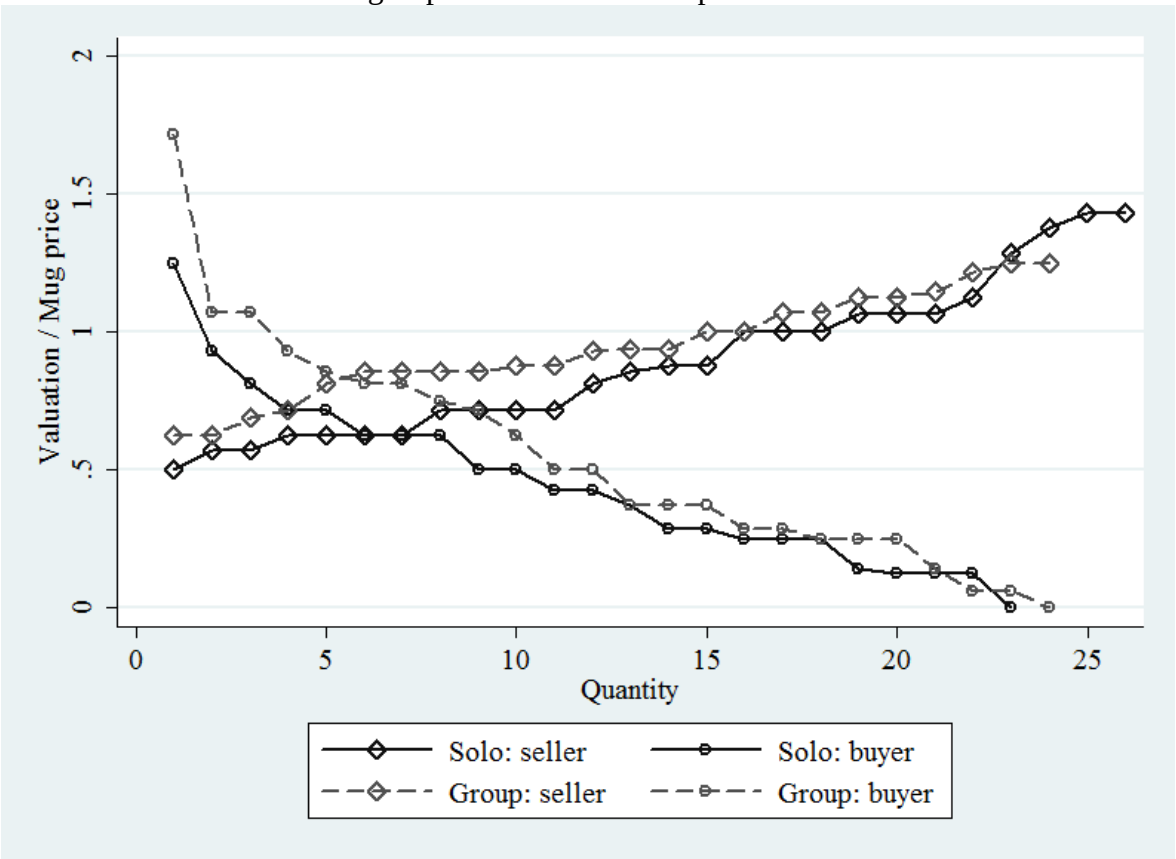
Notes. This figure shows the difference in Google Trends search term popularity between Kansas City, Missouri and Oklahoma City, Oklahoma each search term listed above each figure. Google Trends measures the relative popularity of search terms relative to overall Google searches, and again relative to the maximum value of the search term over a set time range and in a specific location. The time range chosen is January 2012 through January 2014, the time range relevant to the sample used in this paper. The left y-axis represents the Kansas City, Missouri Google Trends value minus the value from Oklahoma City, Oklahoma. The right y-axis represents the average population in Missouri that has Google Fiber installed, calculated by multiplying the neighborhood specific preregistration rate by the population and again by if the neighborhood has had Google Fiber installed. The solid line represents the Google Trends difference and the dotted line represents the average population with Google Fiber installed.

Figure 2.9.5—Google Trends Kansas City, Oklahoma City comparison, popular information and shopping related search terms



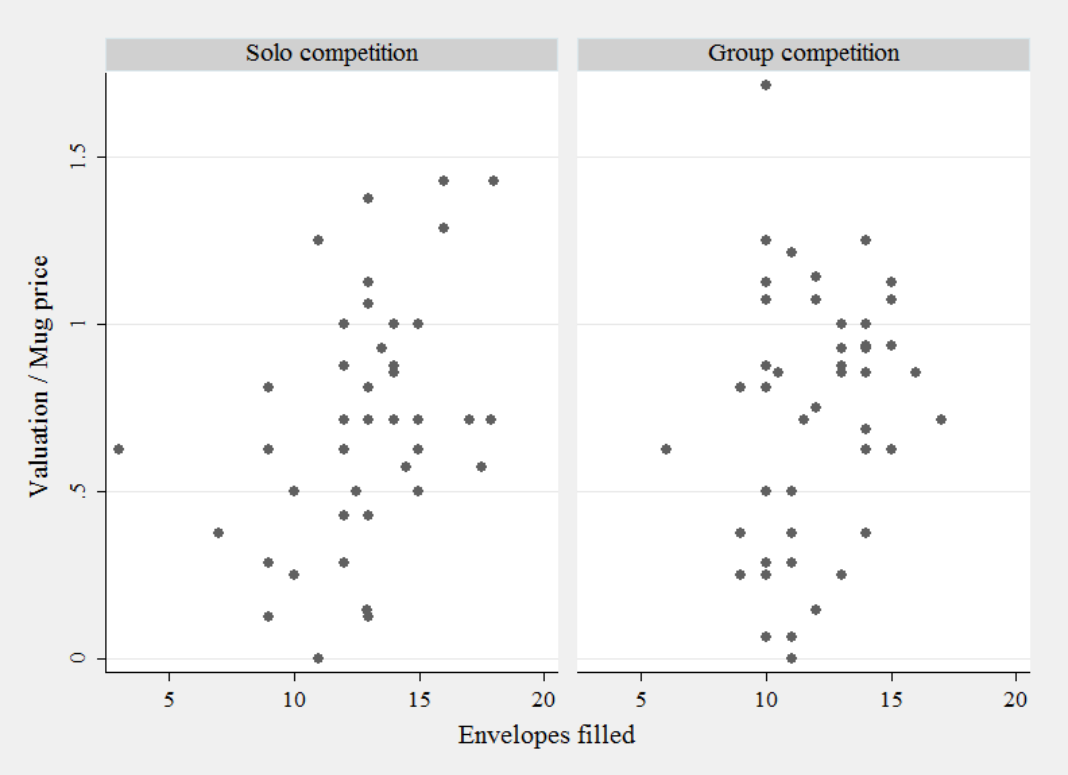
Notes. This figure shows the difference in Google Trends search term popularity between Kansas City, Missouri and Oklahoma City, Oklahoma each search term listed above each figure. Google Trends measures the relative popularity of search terms relative to overall Google searches, and again relative to the maximum value of the search term over a set time range and in a specific location. The time range chosen is January 2012 through January 2014, the time range relevant to the sample used in this paper. The left y-axis represents the Kansas City, Missouri Google Trends value minus the value from Oklahoma City, Oklahoma. The right y-axis represents the average population in Missouri that has Google Fiber installed, calculated by multiplying the neighborhood specific preregistration rate by the population and again by if the neighborhood has had Google Fiber installed. The solid line represents the Google Trends difference and the dotted line represents the average population with Google Fiber installed.

Figure 3.1—Supply and demand of mug market:
group and individual competitions



Notes. This figure displays the valuation ratio (elicited valuation divided buy the mug retail purchase price) versus the quantity available in the market. The lighter colored and dashed lines represented the group competition. The darker colored and solid lines represent the solo competition. The diamond markers represent the sellers (supply) in the market. The circles represent the buyers (demand) in the market. The valuation ratio is used instead of the raw price data to control for a $1 increase in mug price between the first set of trials and the second. The figure shows a rightward increase in demand and a leftward increase in supply in the group competition, the combined effects result in overall average market valuation ratio that is higher than the solo competition or control.

Figure 3.2—Valuation vs. effort across for solo and group competitions



Notes. This figure plots the valuation ratio (valuation divided by the mug price) versus effort produced, the number of envelopes filled. As we can see in the figure, there does not appear to be any association between effort and valuation between solo and group competitions. To statistically test whether a group competition structure, has any effect on effort produced, I present results from an OLS regression shown in Table 3.5.

Table 1.1—Los Angeles library hours variation, January 2009 through September 2011

Type	Date Affected	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Central	1/1/2009 - 12/10/2009	1pm-5pm	10am-8pm	10am-8pm	10am-8pm	10am-8pm	10am-6pm	10am-6pm
	12/11/2009 - 4/10/2010	1pm-5pm	10am-8pm	10am-8pm	10am-8pm	10am-8pm	10am-6pm	10am-6pm
	4/11/2010 - 7/18/2010	*CLOSED*	*10am-6pm*	10am-8pm	*10am-6pm*	10am-8pm	10am-6pm	10am-6pm
	7/19/2010 - 7/17/2011	CLOSED	*CLOSED*	10am-8pm	*10am-5:30pm*	10am-8pm	*10am-5:30pm*	*10am-5:30pm*
	7/18/2011 - 10/15/2012	CLOSED	*10am-5:30pm*	10am-8pm	10am-5:30pm	10am-8pm	10am-5:30pm	10am-5:30pm

	Date Affected	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Regional	1/1/2009 - 12/10/2009	1pm-5pm	10am-8pm	10am-8pm	10am-8pm	10am-8pm	10am-6pm	10am-6pm
	12/11/2009 - 4/10/2010	1pm-5pm	10am-8pm	10am-8pm	10am-8pm	10am-8pm	*1:30pm-5:30pm*	10am-6pm
	4/11/2010 - 7/18/2010	*CLOSED*	*10am-6pm*	*12pm-8pm*	*10am-6pm*	*12pm-8pm*	1:30pm-5:30pm	10am-6pm
	7/19/2010 - 7/17/2011	CLOSED	*CLOSED*	*12:30pm-8pm*	*10am-5:30pm*	12:30pm-8pm	*10am-5:30pm*	*10am-5:30pm*
	7/18/2011 - 10/15/2012	CLOSED	*10am-5:30pm*	12:30pm-8pm	10am-5:30pm	12:30pm-8pm	10am-5:30pm	10am-5:30pm

	Date Affected	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Branch	1/1/2009 - 12/10/2009	CLOSED	10am-8pm	12pm-8pm	10am-8pm	12pm-8pm	10am-6pm	10am-6pm
	12/11/2009 - 4/10/2010	CLOSED	10am-8pm	12pm-8pm	10am-8pm	12pm-8pm	*1:30pm-5:30pm*	10am-6pm
	4/11/2010 - 7/18/2010	CLOSED	*10am-6pm*	12pm-8pm	*10am-6pm*	12pm-8pm	1:30pm-5:30pm	10am-6pm
	7/19/2010 - 7/17/2011	CLOSED	*CLOSED*	*12:30pm-8pm*	*10am-5:30pm*	*12:30pm-8pm*	*10am-5:30pm*	*10am-5:30pm*
	7/18/2011 - 10/15/2012	CLOSED	*10am-5:30pm*	12:30pm-8pm	10am-5:30pm	12:30pm-8pm	*1:30pm-5:30pm*	10am-5:30pm

Notes. This table lists all the changes to the library hours during the sample period. As seen above, there are many changes for each type of library, branch, regional and the Central library. There is one central library, eight regional libraries and 64 branches. The major cuts to library hours occurred on in April, 2010 and July, 2010 when Sunday and Monday service were cut. Monday service was partially restored the following year. The Robertson branch removed eight Saturday hours during the week of April 10th 2011, and replaced them with four Sunday hours.

Table 1.2—Closures and openings

Library Name	Closed	Open
Angeles Mesa	8/1/2009	9/10/2009
Cahuenga	1/22/2009	1/26/2009
Felipe de Neve	2/7/2009	6/15/2009
John C. Freemont	4/6/2009	5/11/2009
Malabar	5/18/2009	5/18/2009
Silverlake (grand opening)	-	11/17/2009
Vermont Square	7/26/2009	10/13/2009

Notes. This table lists the library branches that closed for repairs, renovations or opening a new library (Silverlake) during the sample period, January 2009 through September 2011. These closures and openings add additional variation and provide a counterfactual as there were no library hours and they were on irregular days.

Table 1.3—Crime totals by date range, radii, type, and gang identifier, January 2009 through September 2011

Radii	Population	Gang and Non-gang			Gang			Non-gang		
		All crime	Property	Violent	All crime	Property	Violent	All crime	Property	Violent
Table 3A. Any date range										
0.6 or less	1,172,730	97,375	75,922	21,453	5,197	317	4,880	92,178	75,605	16,573
1.0 - 0.6	1,226,670	91,070	71,721	19,349	4,896	250	4,646	86,174	71,471	14,703
1.0 or greater	1,395,648	110,164	94,461	15,703	3,499	245	3,254	106,665	94,216	12,449
Total	3,795,049	298,609	242,104	56,505	13,592	812	12,780	285,017	241,292	43,725
Table 3B. Date range equal to one or zero days										
0.6 or less	1,172,730	90,612	69,190	21,422	5,177	304	4,873	85,435	68,886	16,549
1.0 - 0.6	1,226,670	84,111	64,786	19,325	4,882	240	4,642	79,229	64,546	14,683
1.0 or greater	1,395,648	100,403	84,730	15,673	3,489	237	3,252	96,914	84,493	12,421
Total	3,795,049	275,126	218,706	56,420	13,548	781	12,767	261,578	217,925	43,653

Source: Los Angeles Police Department

Notes. This table reports crime and population totals for each radii of 0.6, 1.0 and 1.0 or greater miles, for property, violent and all crimes, by date range, and by gang identifier Every crime is recorded with a date range over which the crime is alleged to occur. The date range is the difference between the reported beginning date of a crime, and the end date. Table 3.A reports all crimes regardless of the date range. Table 3.B reports only crimes which have a date range of one or zero

Table 1.4.1—Benchmark population-weighted least squares by crime type, crime dated at beginning date

Beginning Date	(1) All crime	(2) Violent	(3) Assault	(4) Homicide	(5) Robbery	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
LibHours	0.0545*	0.0276*	0.0211*	-0.0012	0.0077	0.0269	0.0149	0.0006	0.0041	0.0203*	-0.0129
	(0.0291)	(0.0150)	(0.0110)	(0.0015)	(0.0073)	(0.0272)	(0.0126)	(0.0034)	(0.0108)	(0.0121)	(0.0105)
(r=0.6) * LibHours	-0.0292*	-0.0069	-0.0080	-0.0000	0.0011	-0.0223*	0.0045	0.0000	0.0064	-0.016***	-0.017***
	(0.0152)	(0.0070)	(0.0055)	(0.0006)	(0.0031)	(0.0125)	(0.0069)	(0.0013)	(0.0050)	(0.0050)	(0.0064)
(r=1.0) * LibHours	-0.0456***	-0.0135**	-0.0084*	-0.0000	-0.0051	-0.032***	-0.0072	-0.0001	0.0019	-0.0105*	-0.016***
	(0.0119)	(0.0055)	(0.0045)	(0.0006)	(0.0035)	(0.0110)	(0.0063)	(0.0010)	(0.0046)	(0.0056)	(0.0047)
Elementary school days	-0.0363	-0.0145	-0.0136*	0.0007	-0.0015	-0.0218	-0.0102	0.0036	-0.0042	0.0117	-0.023***
	(0.0225)	(0.0102)	(0.0073)	(0.0008)	(0.0070)	(0.0165)	(0.0099)	(0.0031)	(0.0060)	(0.0076)	(0.0074)
Middle school days	0.0842**	0.0206	0.0252**	-0.0016	-0.0029	0.0636*	0.0336*	0.0024	0.0009	0.0132	0.0134
	(0.0422)	(0.0149)	(0.0115)	(0.0013)	(0.0095)	(0.0327)	(0.0180)	(0.0029)	(0.0115)	(0.0142)	(0.0148)
High school days	0.0460	0.0055	-0.0030	-0.0002	0.0086	0.0405	0.0394**	0.0056	-0.0148	-0.0047	0.0149
	(0.0437)	(0.0179)	(0.0119)	(0.0014)	(0.0114)	(0.0341)	(0.0173)	(0.0037)	(0.0123)	(0.0124)	(0.0142)
Other school days	0.1559**	0.0255	0.0125	-0.0032	0.0162	0.1304**	0.0179	-0.0071	0.0342*	0.0402	0.0451*
	(0.0677)	(0.0237)	(0.0207)	(0.0039)	(0.0201)	(0.0535)	(0.0254)	(0.0126)	(0.0202)	(0.0279)	(0.0251)
Constant	10.7123***	1.5852***	1.1695***	0.0609	0.3548**	9.1271***	4.4051***	0.0176	0.7219***	1.3021***	2.6804***
	(0.5003)	(0.2727)	(0.2056)	(0.0578)	(0.1661)	(0.4816)	(0.3077)	(0.0496)	(0.1698)	(0.1511)	(0.2606)
Time FE (day)	X	X	X	X	X	X	X	X	X	X	X
Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.203	0.120	0.055	0.010	0.085	0.174	0.154	0.029	0.052	0.046	0.073

*Standard in parentheses, clustered at the library level, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This is the first of two benchmark difference in differences, weighted least squares results, for the full sample. Of note are the negative and statistically significant at the one percent level estimates of burglary from vehicle as well as the overall null estimate from overall crime. Additional controls include daily fixed effects and library-radii-zone fixed effects. Weights are by population by library-radii-zone.

Table 1.4.2— Population-weighted least squares by crime type, date range equal to one day or zero, beginning date

Beginning Date	(1) All crime	(2) Violent	(3) Assault	(4) Homicide	(5) Robbery	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
LibHours	0.0462*	0.0272*	0.0208*	-0.0012	0.0077	0.0190	0.0099	0.0006	0.0014	0.0196*	-0.0126
	(0.0268)	(0.0151)	(0.0110)	(0.0015)	(0.0072)	(0.0248)	(0.0125)	(0.0034)	(0.0107)	(0.0099)	(0.0106)
(r=0.6) * LibHours	-0.0236	-0.0068	-0.0079	-0.0000	0.0012	-0.0168	0.0060	-0.0001	0.0064	-0.0130**	-0.0162**
	(0.0151)	(0.0069)	(0.0055)	(0.0006)	(0.0031)	(0.0121)	(0.0067)	(0.0013)	(0.0046)	(0.0051)	(0.0064)
(r=1.0) * LibHours	-0.041***	-0.0134**	-0.0083*	0.0000	-0.0051	-0.0271**	-0.0054	-0.0001	0.0013	-0.0080	-0.015***
	(0.0117)	(0.0055)	(0.0044)	(0.0006)	(0.0035)	(0.0104)	(0.0061)	(0.0010)	(0.0047)	(0.0055)	(0.0046)
Elem. Sch. days	-0.0372*	-0.0143	-0.0134*	0.0008	-0.0017	-0.0229	-0.0108	0.0036	-0.0032	0.0098	-0.022***
	(0.0210)	(0.0102)	(0.0072)	(0.0008)	(0.0070)	(0.0150)	(0.0093)	(0.0030)	(0.0053)	(0.0072)	(0.0071)
Mid. Sch. days	0.0702*	0.0205	0.0251**	-0.0017	-0.0028	0.0497*	0.0287*	0.0023	-0.0047	0.0084	0.0150
	(0.0387)	(0.0149)	(0.0114)	(0.0013)	(0.0095)	(0.0292)	(0.0160)	(0.0029)	(0.0099)	(0.0134)	(0.0148)
High Sch. days	0.0324	0.0053	-0.0032	-0.0001	0.0086	0.0270	0.0293*	0.0059	-0.0120	-0.0048	0.0085
	(0.0426)	(0.0179)	(0.0120)	(0.0014)	(0.0113)	(0.0325)	(0.0161)	(0.0037)	(0.0111)	(0.0122)	(0.0134)
Other Sch. days	0.1536**	0.0254	0.0125	-0.0033	0.0162	0.1282***	0.0302	-0.0074	0.0233	0.0328	0.0492*
	(0.0615)	(0.0239)	(0.0207)	(0.0039)	(0.0202)	(0.0471)	(0.0241)	(0.0125)	(0.0165)	(0.0251)	(0.0267)
Constant	7.7500***	1.5353***	1.1186***	0.0612	0.3555**	6.2147***	2.4531***	0.0178	0.5492***	0.9627***	2.2319***
	(0.4689)	(0.2698)	(0.2050)	(0.0578)	(0.1661)	(0.4046)	(0.1996)	(0.0496)	(0.1777)	(0.1403)	(0.2374)
Time (date) and Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.1960	0.1197	0.0552	0.0097	0.0854	0.1640	0.1534	0.0280	0.0495	0.0423	0.0701

*Standard in parentheses, clustered at the library level, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This is the second of two benchmark difference in differences, weighted least squares results, for a subsample of only crimes reported within one or two days. Of note are the negative and statistically significant at the one percent level and five percent level estimates of burglary from vehicle as well as the overall null estimate from overall crime. Additional controls include daily fixed effects and library-radii-zone fixed effects. Weights are by population by library-radii-zone.

Table 1.5— Population-weighted least squares by crime type, gang and not gang-related, date range equal to one day or zero, beginning date

	Gang-related					Not Gang-related				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Beginning Date	All crime	Violent	Assault	Homicide	Robbery	All crime	Violent	Assault	Homicide	Robbery
LibHours	0.0037	0.0048	0.0041	-0.0006	0.0013	0.0425	0.0224*	0.0167	-0.0006	0.0064
	(0.0063)	(0.0059)	(0.0034)	(0.0008)	(0.0038)	(0.0259)	(0.0132)	(0.0103)	(0.0009)	(0.0052)
(r=0.6) * LibHours	0.0014	0.0019	0.0017	-0.0001	0.0003	-0.0250*	-0.0087	-0.0096**	0.0001	0.0009
	(0.0028)	(0.0027)	(0.0023)	(0.0004)	(0.0014)	(0.0144)	(0.0059)	(0.0044)	(0.0004)	(0.0029)
(r=1.0) * LibHours	-0.0015	-0.0017	0.0005	-0.0003	-0.0020*	-0.0390***	-0.0117**	-0.0089***	0.0003	-0.0031
	(0.0026)	(0.0025)	(0.0022)	(0.0004)	(0.0012)	(0.0113)	(0.0044)	(0.0033)	(0.0004)	(0.0032)
Elementary school days	-0.0049	-0.0044	-0.0050	-0.0004	0.0009	-0.0323	-0.0098	-0.0084	0.0012***	-0.0026
	(0.0042)	(0.0042)	(0.0037)	(0.0007)	(0.0023)	(0.0194)	(0.0085)	(0.0057)	(0.0004)	(0.0059)
Middle school days	0.0066	0.0073	0.0109	-0.0009	-0.0027	0.0636*	0.0132	0.0141	-0.0009	-0.0001
	(0.0089)	(0.0090)	(0.0071)	(0.0010)	(0.0031)	(0.0376)	(0.0126)	(0.0087)	(0.0009)	(0.0088)
High school days	0.0024	0.0015	-0.0055	0.0000	0.0070*	0.0299	0.0038	0.0023	-0.0002	0.0017
	(0.0058)	(0.0056)	(0.0053)	(0.0010)	(0.0036)	(0.0412)	(0.0162)	(0.0093)	(0.0008)	(0.0111)
Other school days	-0.0023	-0.0015	0.0039	-0.0021	-0.0033	0.1558***	0.0269	0.0087	-0.0012	0.0195
	(0.0147)	(0.0141)	(0.0126)	(0.0033)	(0.0054)	(0.0565)	(0.0205)	(0.0133)	(0.0015)	(0.0181)
Constant	0.0444	0.0318	-0.0091	-0.0128***	0.0538	7.7056***	1.5035***	1.1278***	0.0740	0.3017**
	(0.0894)	(0.0897)	(0.0627)	(0.0034)	(0.0632)	(0.4573)	(0.2600)	(0.2091)	(0.0580)	(0.1429)
Time (date) and Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X
Observations	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.0417	0.0417	0.0263	0.0103	0.0238	0.1955	0.1031	0.0452	0.0078	0.0752

*Standard in parentheses, clustered at the library level, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This is a gang and not gang-related crime specification table. Here I disaggregate violent crime into gang and not gang related and test using the benchmark specification. Additional controls include daily fixed effects and library-radii-zone fixed effects. Weights are by population by library-radii-zone.

Table 1.6.1.1—Robustness specifications, burglary from vehicle, benchmark

	(1)	(2)	(3)	(4)	(5)	(6)
LibHours	-0.0126 (0.0106)	-0.0018 (0.0124)	-0.0129 (0.0105)	-0.0043 (0.0128)	-0.0208* (0.0112)	-0.0094 (0.0125)
(r=0.6) * LibHours	-0.0162** (0.0064)	-0.0221*** (0.0077)	-0.0172*** (0.0064)	-0.0208*** (0.0078)	-0.0126* (0.0064)	-0.0194** (0.0077)
(r=1.0) * LibHours	-0.0148*** (0.0046)	-0.0132** (0.0053)	-0.0162*** (0.0047)	-0.0112** (0.0056)	-0.0113** (0.0049)	-0.0109* (0.0057)
Time fixed effects (day)	X	X	X	X	X	X
Library-radii fixed effects	X	X	X	X	X	X
Library fixed effects						
Library by date fixed effects						
Radii fixed effects						
Beg date, one day elapsed	X					
End date, one day elapsed		X				
Beg date, all			X			
End date, all				X		
Beg date, farthest less than 1.5 miles					X	
End date, farthest less than 1.5 miles						X
Population weighted	X	X	X	X	X	X
Observations	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.0701	0.0694	0.0731	0.0718	0.0612	0.0606

Notes. This is a robustness specification table, testing the benchmark model of burglary from vehicle against many different specification. The full set of tests by crime type of specification (7), the library by date fixed effect controls, is found in Table A2 in the Appendices.

Table 1.6.1.2—Robustness specifications, burglary from vehicle, additional

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
LibHours	-	-0.0110	-0.0001	-0.0114	-0.0025	0.0360	-0.0190
	-	(0.0109)	(0.0125)	(0.0110)	(0.0130)	(0.0337)	(0.0529)
(r=0.6) * LibHours	-0.0201*	-0.0172**	-0.0232***	-0.0183**	-0.0220**	-0.0385**	0.0310
	(0.0098)	(0.0073)	(0.0083)	(0.0073)	(0.0086)	(0.0170)	(0.0564)
(r=1.0) * LibHours	-0.0187**	-0.0165***	-0.0150***	-0.0178***	-0.0129**	-0.0315**	0.0428
	(0.0060)	(0.0048)	(0.0055)	(0.0049)	(0.0058)	(0.0150)	(0.0556)
Time fixed effects (day)		X	X	X	X	X	X
Library-radii fixed effects						X	X
Library fixed effects		X	X	X	X		
Library by date fixed effects	X						
Radii fixed effects	X						
Beg date, one day elapsed	X	X				X	
End date, one day elapsed			X				X
Beg date, all				X			
End date, all					X		
Beg date, farthest less than 1.5 miles							
End date, farthest less than 1.5 miles							
Population weighted	X	X	X	X	X		
Observations	214,830	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.3126	0.0360	0.0357	0.0382	0.0374	0.0897	0.0911

Notes. This is a robustness specification table, testing the benchmark model of burglary from vehicle against many different specification. The full set of tests by crime type of specification (7), the library by date fixed effect controls, is found in Table A2 in the Appendices.

Table 1.6.2—Overall summed library zones: no size limit specification, 1.5 mile radii size limit specification, population-weighted least squares, date range equal to one day or zero, beginning date

Beginning Date	(1) All crime	(2) Violent	(3) Assault	(4) Homicide	(5) Robbery	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
1) LibHours, full library zone	0.0203	0.0187	0.0140	-0.0013	0.0059	0.0016	0.0099	0.0005	0.0046	0.0111	-0.0245**
	(0.0244)	(0.0136)	(0.0100)	(0.0015)	(0.0070)	(0.0240)	(0.0113)	(0.0033)	(0.0101)	(0.0088)	(0.0099)
2) LibHours, 1.5 mile total radii zone	0.0163	0.0176	0.0130	-0.0013	0.0059	-0.0013	0.0123	-0.0003	0.0059	0.0106	-0.0298***
	(0.0267)	(0.0142)	(0.0105)	(0.0016)	(0.0070)	(0.0260)	(0.0112)	(0.0037)	(0.0104)	(0.0087)	(0.0105)
Time fixed effects (day)	X	X	X	X	X	X	X	X	X	X	X
Library fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270

*Standard in parentheses, clustered at the library level. School day variables not displayed, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This Table uses two specifications of the summed library zones. This takes the library-zones as they were before cutting them into radii, or summing the zones into one larger zone per library. Doing so shrinks the available observations to one third of their previous total as two thirds of the radii observations are missing. The second specification also does not use radii but instead limits the total reach of the library zone to 1.5 miles. Both specifications are to test whether the earlier results are driven by an artifact of the radii map construction or a result of the library hours sphere of influence. Additional controls include daily fixed effects and library-zone fixed effects. Weights are by population by library-zone.

Table 1.7.1—High violent crime areas interaction above 75th percentile,
date range equal to one day or zero, beginning date, overall library zones, summed library zones

Beginning Date	(1) All crime	(2) Violent	(3) Assault	(4) Homicide	(5) Robbery	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
LibHours	0.0252	0.0210	0.0179	-0.0008	0.0039	0.0042	0.0100	-0.0003	0.0039	0.0133	-0.0227**
	(0.0251)	(0.0142)	(0.0108)	(0.0014)	(0.0069)	(0.0239)	(0.0116)	(0.0036)	(0.0102)	(0.0086)	(0.0099)
LibHours*V75th	-0.0230	-0.0109	-0.018***	-0.002**	0.0093	-0.0121	-0.0004	0.0037**	0.0031	-0.0102	-0.0084
	(0.0189)	(0.0070)	(0.0057)	(0.0008)	(0.0057)	(0.0149)	(0.0054)	(0.0018)	(0.0059)	(0.0081)	(0.0063)
Elem Sch. days	-0.0312	-0.0029	-0.0063	0.0011	0.0023	-0.0283*	0.0003	0.0025	-0.0113**	0.0012	-0.021***
	(0.0189)	(0.0071)	(0.0050)	(0.0007)	(0.0040)	(0.0146)	(0.0070)	(0.0022)	(0.0052)	(0.0081)	(0.0044)
Middle Sch. days	0.0211	-0.0084	-0.0012	-0.0014	-0.0059	0.0295	0.0133	0.0011	-0.0139*	0.0152	0.0137
	(0.0332)	(0.0121)	(0.0090)	(0.0016)	(0.0075)	(0.0253)	(0.0128)	(0.0022)	(0.0072)	(0.0152)	(0.0135)
High Sch. days	0.0025	-0.0025	-0.0035	0.0001	0.0010	0.0050	0.0051	0.0003	-0.0042	-0.0120	0.0159
	(0.0411)	(0.0170)	(0.0105)	(0.0017)	(0.0103)	(0.0292)	(0.0132)	(0.0051)	(0.0086)	(0.0139)	(0.0128)
Other Sch. days	0.0526	-0.0075	-0.0056	-0.0031	0.0012	0.0601	0.0137	-0.0023	0.0127	0.0178	0.0182
	(0.0494)	(0.0200)	(0.0148)	(0.0032)	(0.0119)	(0.0368)	(0.0149)	(0.0039)	(0.0121)	(0.0209)	(0.0177)
Constant	7.92***	1.5844***	1.1143***	0.0562	0.4138**	6.3308***	2.1921***	0.0429	0.6263***	1.0256***	2.4439***
	(0.4709)	(0.2723)	(0.2053)	(0.0581)	(0.1685)	(0.4080)	(0.1971)	(0.0513)	(0.1794)	(0.1467)	(0.2396)
Time (date) and Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270
R-squared	0.3218	0.2759	0.1442	0.0248	0.2025	0.2554	0.2627	0.0697	0.0952	0.0979	0.1122

*Standard in parentheses, clustered at the library level, population weighted least squares, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This is the first of two interaction specifications, the specifications in this table interact library hours with an indicator variable describing whether the library-zone (not library radii-zones) is in the top 75th percentile of violent crime rates. Of special note are the negative and statistically significant estimates of homicide and aggravated assault. Additional controls include daily fixed effects and library-radii-zone fixed effects. Additional controls include daily fixed effects and library-zone fixed effects. Weights are by population by library-zone.

Table 1.7.2—Above 25th percentile crime rate interaction,
date range equal to one day or zero, beginning date, overall library zones, summed library zones

Beginning Date	(1) All crime	(2) Violent	(3) Assault	(4) Homicide	(5) Robbery	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
LibHours	-0.0080	0.0187	0.0162	-0.0006	0.0031	-0.0268	0.0021	-0.0022	0.0096	-0.0034	-0.033***
	(0.0299)	(0.0168)	(0.0118)	(0.0015)	(0.0079)	(0.0263)	(0.0123)	(0.0035)	(0.0116)	(0.0092)	(0.0105)
LibHours*C25th	0.0320*	-0.0000	-0.0024	-0.0008**	0.0032	0.032***	0.0089*	0.0030***	-0.0057	0.0163***	0.0095*
	(0.0162)	(0.0090)	(0.0059)	(0.0004)	(0.0040)	(0.0120)	(0.0048)	(0.0009)	(0.0049)	(0.0058)	(0.0049)
Elem Sch. days	-0.0328	-0.0035	-0.0072	0.0010	0.0028	-0.0294*	0.0001	0.0026	-0.011**	0.0005	-0.022***
	(0.0198)	(0.0074)	(0.0056)	(0.0007)	(0.0041)	(0.0149)	(0.0069)	(0.0022)	(0.0052)	(0.0082)	(0.0043)
Middle Sch. days	0.0197	-0.0089	-0.0020	-0.0014	-0.0055	0.0286	0.0132	0.0012	-0.014*	0.0146	0.0133
	(0.0333)	(0.0125)	(0.0099)	(0.0017)	(0.0076)	(0.0249)	(0.0127)	(0.0023)	(0.0072)	(0.0152)	(0.0134)
High Sch. days	-0.0001	-0.0031	-0.0045	-0.0000	0.0014	0.0031	0.0048	0.0004	-0.0038	-0.0133	0.0150
	(0.0419)	(0.0174)	(0.0113)	(0.0018)	(0.0102)	(0.0292)	(0.0132)	(0.0050)	(0.0086)	(0.0137)	(0.0131)
Other Sch. days	0.0517	-0.0071	-0.0048	-0.0030	0.0007	0.0587	0.0132	-0.0026	0.0129	0.0172	0.0180
	(0.0508)	(0.0209)	(0.0163)	(0.0033)	(0.0119)	(0.0371)	(0.0149)	(0.0039)	(0.0118)	(0.0212)	(0.0177)
Constant	4.6550***	1.1683***	0.84***	0.0535	0.274	3.487***	1.457***	0.0467	0.1132	0.2508	1.6189***
	(0.4770)	(0.2750)	(0.2087)	(0.0580)	(0.1676)	(0.4123)	(0.1959)	(0.0512)	(0.1812)	(0.1550)	(0.2398)
Time (date) and Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270
R-squared	0.3219	0.2758	0.1439	0.0246	0.2024	0.2555	0.2628	0.0697	0.0952	0.0980	0.1122

*Standard in parentheses, clustered at the library level, population weighted least squares, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This is the second of three interaction specifications, the specifications in this table interact library hours with an indicator variable describing whether the library-zone (not library radii-zones) is in the 25th percentile of overall crime rates. Of special note are the negative and statistically significant estimates of homicide and the positive and statistically significant estimates of property crime. Additional controls include daily fixed effects and library-zone fixed effects. Weights are by population by library-zone.

Table 2.1—Crime totals by state, 2012-2013

Totals	Year	Pop.	Homicide	Rape	Robbery	Agg. Assault	Burglary	Theft
Kansas	2012	127,431	20	74	272	433	2,339	4,602
	2013		28	65	228	348	2,143	4,562
Missouri	2012	226,759	88	187	1,721	2,939	5,023	12,077
	2013		88	163	1,760	2,724	4,202	11,343

Table 2.2—Crime rates per 1,000 population by state, 2012-2013

Rates	Year	Pop.	Homicide	Rape	Robbery	Agg. Assault	Burglary	Theft
Kansas	2012	127,431	0.16	0.58	2.13	3.40	18.36	36.11
	2013		0.22	0.51	1.79	2.73	16.82	35.80
Missouri	2012	226,759	0.39	0.82	7.59	12.96	22.15	53.26
	2013		0.39	0.72	7.76	12.01	18.53	50.02

Notes. These table shows the counts and rates of homicide, rape, robbery, aggravated assault, burglary and theft for Kansas City Missouri and Kansas in 2012 and 2013. The counts are reported by the police departments of Kansas City, Kansas and Kansas City, Missouri. Population data is from the Census. For Table 2, the crime rates are reported per 1,000 population. The population counts are for relevant Google neighborhoods and do not represent the entirety of either Kansas City.

Table 2.3—Benchmark model by crime type, population-weighted least squares, demand-level controls

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	0.0004	-0.0001	-0.0003	-0.0033	0.0014	-0.0011	0.0038
	(0.0120)	(0.0004)	(0.0004)	(0.0048)	(0.0014)	(0.0077)	(0.0036)
Median Household Income ($1000s)	-0.0029***	-0.0000**	-0.0000**	-0.0004**	-0.0002**	-0.0019***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
Black population percentage	0.0256	0.0017***	0.0010	0.0349**	-0.0006	-0.0435	0.0321**
	(0.0719)	(0.0005)	(0.0011)	(0.0158)	(0.0105)	(0.0454)	(0.0133)
Unemployment rate	-0.2226	0.0001	0.0008	-0.0771	-0.0347	-0.0554	-0.0563
	(0.2528)	(0.0028)	(0.0044)	(0.0591)	(0.0373)	(0.1499)	(0.0540)
Kansas dummy	-0.1331***	-0.0005**	-0.0010**	-0.0254***	-0.0218***	-0.0798**	-0.0046
	(0.0435)	(0.0002)	(0.0004)	(0.0049)	(0.0048)	(0.0335)	(0.0064)
Constant	0.3909***	0.0008	0.0030**	0.0543***	0.0352***	0.2273***	0.0704***
	(0.0711)	(0.0005)	(0.0014)	(0.0150)	(0.0127)	(0.0361)	(0.0139)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	146,566	146,566	146,566	146,566	146,566	146,566	146,566
R-squared	0.073	0.004	0.007	0.019	0.017	0.084	0.015

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the benchmark model results. Each column represents a specification using the crime rate type (per 1,000 persons) listed below as the dependent variable. My variable of interest is $Install_{it}$, which is a dummy variable that records a "1" if Google Fiber installation by neighborhood *i* has occurred by date *t*. The controls for these regressions include the demographic variables listed above, time fixed effects by date, and a demand level dummy variable for each demand level. I find no statistically significant effects of Google Fiber internet installation for any crime type used. Predictably, I find the expected signs for the demographic variables. Median household income (measured is thousands of dollars) is uniformly negative, as higher income neighborhoods typically have fewer crimes committed.

Table 2.4—Alternative specification, interaction with de-meaned median household income, by crime rate type, population-weighted least squares, demand-level controls

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	-0.0015	0.0000	-0.0004	-0.0040	0.0011	-0.0014	0.0032
	(0.0121)	(0.0004)	(0.0004)	(0.0050)	(0.0014)	(0.0079)	(0.0036)
Install * dMHI ($1000s)	0.0003	-0.0000	0.0000	0.0001*	0.0000	0.0000	0.0001
	(0.0003)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0002)	(0.0001)
dMHI ($1000s)	-0.0030***	-0.0000*	-0.0000**	-0.0004***	-0.0002**	-0.0019***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
Black population percentage	0.0256	0.0017***	0.0010	0.0349**	-0.0006	-0.0435	0.0321**
	(0.0719)	(0.0005)	(0.0011)	(0.0158)	(0.0105)	(0.0454)	(0.0133)
Unemployment rate	-0.2248	0.0002	0.0007	-0.0779	-0.0350	-0.0558	-0.0569
	(0.2529)	(0.0028)	(0.0045)	(0.0592)	(0.0373)	(0.1499)	(0.0540)
Kansas dummy	-0.1330***	-0.0005**	-0.0010**	-0.0254***	-0.0218***	-0.0798**	-0.0046
	(0.0435)	(0.0002)	(0.0004)	(0.0049)	(0.0048)	(0.0335)	(0.0064)
Constant	0.2649***	0.0003	0.0018*	0.0369***	0.0250***	0.1466***	0.0543***
	(0.0528)	(0.0004)	(0.0011)	(0.0106)	(0.0096)	(0.0254)	(0.0115)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.0743	0.0036	0.0069	0.0185	0.0166	0.0850	0.0156

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the results of an alternative specification, interacting de-meaned median household income (dMHI in the table) with Install. A discussion of the motivations for this interaction occurs in section 5.4. Each column represents a separate regression using the crime rate type listed below as the dependent variable. Each regression uses weighted least squares, weighted by population.

Table 2.5.1—Alternative specification, interaction with de-meaned demand level, by crime rate type, population-weighted least squares, demand-level controls

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	0.0038	0.0000	-0.0002	-0.0030	0.0021	0.0003	0.0046
	(0.0108)	(0.0004)	(0.0004)	(0.0052)	(0.0014)	(0.0063)	(0.0036)
Install * Demand	-0.0004	-0.0000	-0.0000	-0.0000	-0.0001	-0.0002	-0.0001
	(0.0012)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0009)	(0.0001)
Median Household Income	-0.0029***	-0.0000**	-0.0000**	-0.0004**	-0.0002**	-0.0019***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
Black population percentage	0.0257	0.0017***	0.0010	0.0349**	-0.0006	-0.0434	0.0321**
	(0.0720)	(0.0005)	(0.0011)	(0.0158)	(0.0105)	(0.0454)	(0.0133)
Unemployment rate	-0.2228	0.0001	0.0008	-0.0771	-0.0347	-0.0555	-0.0563
	(0.2528)	(0.0028)	(0.0044)	(0.0591)	(0.0373)	(0.1500)	(0.0540)
Kansas dummy	-0.1333***	-0.0005**	-0.0010**	-0.0254***	-0.0218***	-0.0799**	-0.0047
	(0.0435)	(0.0002)	(0.0004)	(0.0049)	(0.0048)	(0.0334)	(0.0064)
Constant	0.5721***	0.0007	0.0048***	0.0739***	0.0483***	0.3768***	0.0675***
	(0.0663)	(0.0005)	(0.0013)	(0.0141)	(0.0116)	(0.0345)	(0.0128)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.0743	0.0036	0.0069	0.0185	0.0166	0.0850	0.0156

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the results of an alternative specification, interacting de-meaned demand level as a continuous variable with Install. A discussion of the motivations for this interaction occurs in section 5.5. Each column represents a separate regression using the crime rate type listed below as the dependent variable. Each regression uses weighted least squares, weighted by population. Median household income is reported in thousands of dollars.

Table 2.5.2—Alternative specification, interaction with 15-24 male share of population, population-weighted least squares

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	-0.0004	-0.0001	-0.0003	-0.0034	0.0014	-0.0015	0.0035
	(0.0121)	(0.0004)	(0.0004)	(0.0048)	(0.0014)	(0.0078)	(0.0036)
Install * d15-24 age share	-0.4025	-0.0065	0.0028	-0.0589	0.0404	-0.1870	-0.1933*
	(0.3229)	(0.0148)	(0.0085)	(0.0516)	(0.0724)	(0.2631)	(0.1079)
d15-24 age share	1.0470*	0.0251	0.0030	0.1542	0.1146	0.5728	0.1774
	(0.6203)	(0.0193)	(0.0090)	(0.0988)	(0.0879)	(0.4846)	(0.1310)
Median Household Income	-0.0030***	-0.0000**	-0.0000**	-0.0004***	-0.0002**	-0.0019***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
Black population share	0.0364	0.0019***	0.0010	0.0365**	0.0007	-0.0375	0.0338**
	(0.0734)	(0.0006)	(0.0011)	(0.0160)	(0.0107)	(0.0466)	(0.0134)
Unemployment rate	-0.2481	-0.0005	0.0007	-0.0808	-0.0377	-0.0695	-0.0603
	(0.2579)	(0.0028)	(0.0045)	(0.0595)	(0.0378)	(0.1555)	(0.0544)
Kansas dummy	-0.1315***	-0.0004**	-0.0010**	-0.0252***	-0.0216***	-0.0789**	-0.0043
	(0.0439)	(0.0002)	(0.0004)	(0.0049)	(0.0048)	(0.0337)	(0.0064)
Constant	0.3890***	0.0007	0.0030**	0.0540***	0.0350***	0.2262***	0.0701***
	(0.0709)	(0.0005)	(0.0014)	(0.0149)	(0.0127)	(0.0357)	(0.0140)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.0749	0.0037	0.0069	0.0186	0.0168	0.0854	0.0157

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.

*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the results of an alternative specification, interacting the de-meaned share of the population that is male and between the ages of 15 through 24 (shown as d15-24 age share above) as a continuous variable with Install. Each column represents a separate regression using the crime rate type listed below as the dependent variable. Each regression uses weighted least squares, weighted by population. Median household income is reported in thousands of dollars. Date and demand level dummies are used in each specification.

Table 2.5.3—Alternative specification, interaction with black share of population, population-weighted least squares

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	-0.0005	-0.0001	-0.0002	-0.0047	0.0014	0.0001	0.0030
	(0.0126)	(0.0004)	(0.0004)	(0.0051)	(0.0016)	(0.0080)	(0.0039)
Install * dBlack pop. share	-0.0070	-0.0005	0.0006	-0.0107	0.0001	0.0095	-0.0059
	(0.0320)	(0.0007)	(0.0010)	(0.0076)	(0.0051)	(0.0211)	(0.0110)
Median Household Income	-0.0029***	-0.0000**	-0.0000**	-0.0004**	-0.0002**	-0.0019***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
dBlack pop. share	0.0263	0.0017***	0.0009	0.0360**	-0.0006	-0.0444	0.0327**
	(0.0731)	(0.0005)	(0.0011)	(0.0163)	(0.0104)	(0.0459)	(0.0137)
Unemployment rate	-0.2229	0.0001	0.0008	-0.0775	-0.0347	-0.0550	-0.0565
	(0.2529)	(0.0028)	(0.0045)	(0.0592)	(0.0372)	(0.1499)	(0.0540)
Kansas dummy	-0.1331***	-0.0005**	-0.0010**	-0.0254***	-0.0218***	-0.0799**	-0.0046
	(0.0435)	(0.0002)	(0.0004)	(0.0049)	(0.0048)	(0.0335)	(0.0064)
Constant	0.4007***	0.0014**	0.0034**	0.0677***	0.0350***	0.2107***	0.0827***
	(0.0688)	(0.0006)	(0.0014)	(0.0158)	(0.0123)	(0.0337)	(0.0135)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.0749	0.0037	0.0069	0.0186	0.0168	0.0854	0.0157

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the results of an alternative specification, interacting the de-meaned black share of the population (shown as dBlack pop. share above) as a continuous variable with Install. Each column represents a separate regression using the crime rate type listed below as the dependent variable. Each regression uses weighted least squares, weighted by population. Median household income is reported in thousands of dollars. Date and demand level dummies are used in each specification.

Table 2.5.4—Alternative specification, interaction with black share of population aged 15-24, population-weighted least squares

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	0.0011	-0.0000	-0.0003	-0.0028	0.0017	-0.0021	0.0046
	(0.0122)	(0.0004)	(0.0004)	(0.0049)	(0.0014)	(0.0077)	(0.0036)
Install * d15-24 black share	-0.0033	0.0022	-0.0020	-0.0553	0.0449	-0.0167	0.0236
	(0.2660)	(0.0064)	(0.0086)	(0.0787)	(0.0399)	(0.1615)	(0.0973)
d15-24 black share	-0.0475	0.0056	-0.0006	0.0829	-0.0418	-0.2756	0.1821*
	(0.4797)	(0.0056)	(0.0081)	(0.1310)	(0.0693)	(0.2822)	(0.1044)
Median Household Income	-0.0029***	-0.0000**	-0.0000**	-0.0004**	-0.0002**	-0.0018***	-0.0004***
	(0.0007)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0004)	(0.0001)
Unemployment rate	-0.1533	0.0025	0.0032	-0.0151	-0.0275	-0.0917	-0.0248
	(0.2375)	(0.0026)	(0.0042)	(0.0555)	(0.0347)	(0.1425)	(0.0537)
Kansas dummy	-0.1402***	-0.0008***	-0.0012***	-0.0338***	-0.0220***	-0.0711***	-0.0113**
	(0.0374)	(0.0002)	(0.0004)	(0.0061)	(0.0043)	(0.0272)	(0.0057)
Constant	0.3937***	0.0013**	0.0031**	0.0636***	0.0336***	0.2087***	0.0833***
	(0.0714)	(0.0006)	(0.0015)	(0.0174)	(0.0125)	(0.0359)	(0.0149)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.0749	0.0037	0.0069	0.0186	0.0168	0.0854	0.0157

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are the results of an alternative specification, interacting the de-meaned share of the population that is male, black, and between the ages of 15 through 24 (shown as d15-24 black share above) as a continuous variable with Install. Each column represents a separate regression using the crime rate type listed below as the dependent variable. Each regression uses weighted least squares, weighted by population. Median household income is reported in thousands of dollars. Date and demand level dummies are used in each specification.

Table 2.6.1—Robustness, restricted demand range, crime rate type, population-weighted least squares, demand-level controls

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	-0.0057	0.0005	-0.0006	0.0014	-0.0019	0.0016	-0.0067
	(0.0149)	(0.0006)	(0.0005)	(0.0026)	(0.0023)	(0.0127)	(0.0051)
Median Household Income	-0.0017**	-0.0000	-0.0000	-0.0001	-0.0000	-0.0013**	-0.0003**
	(0.0007)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0005)	(0.0001)
Black population percentage	0.2002*	0.0018*	0.0042***	0.0330***	0.0113	0.0928	0.0570*
	(0.1143)	(0.0009)	(0.0014)	(0.0122)	(0.0125)	(0.0831)	(0.0296)
Unemployment rate	-0.0209	-0.0006	-0.0017	0.0372	0.0121	-0.0450	-0.0230
	(0.5614)	(0.0058)	(0.0082)	(0.0550)	(0.0474)	(0.4358)	(0.0803)
Kansas dummy	-0.0656	-0.0004	-0.0006	-0.0135***	-0.0137***	-0.0455	0.0081
	(0.0431)	(0.0003)	(0.0006)	(0.0041)	(0.0036)	(0.0323)	(0.0092)
Constant	0.1444*	0.0000	0.0009	0.0001	0.0086	0.1076*	0.0272*
	(0.0797)	(0.0007)	(0.0012)	(0.0092)	(0.0082)	(0.0624)	(0.0147)
Time fixed effects (date)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	61,404	61,404	61,404	61,404	61,404	61,404	61,404
R-squared	0.1171	0.0055	0.0135	0.0275	0.0240	0.1295	0.0212

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. This is a robustness check, restricting the demand range allowed in the sample to less than 57 percent and greater than 17 percent. By doing this, I isolate the neighborhoods across states with comparable demand ranges with the largest amount of variation. Still, I find no statistically significant effects across all crime types. Please see the discussion in section

Table 2.6.2—Robustness, by crime rate type, population-weighted least squares, neighborhood-level controls

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	-0.0214	0.0001	-0.0005	-0.0079	0.0011	-0.0169	0.0027
	(0.0287)	(0.0004)	(0.0005)	(0.0069)	(0.0019)	(0.0201)	(0.0042)
Constant	0.2941***	0.0029***	0.0010***	0.0324***	0.0241***	0.1982***	0.0355***
	(0.0021)	(0.0000)	(0.0000)	(0.0005)	(0.0001)	(0.0015)	(0.0003)
Time fixed effects (date)	X	X	X	X	X	X	X
Neighborhood level fixed effects	X	X	X	X	X	X	X
Observations	148,393	148,393	148,393	148,393	148,393	148,393	148,393
R-squared	0.1656	0.0060	0.0140	0.0409	0.0446	0.1730	0.0310

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.

*** p<0.01, ** p<0.05, * p<0.10

Notes. These are a robustness check of my benchmark results. Each column represents a specification using the crime rate type listed below as the dependent variable. The controls for these regressions include time fixed effects by date and neighborhood level fixed effects. These specifications do not include the demographic variables as they do not vary over time. I find no statistically significant effects of Google Fiber internet installation for any crime type used. The signs and magnitude of all the specifications in this table are nearly identical with Table 2, suggesting that controlling for demand, state and demographic variables control for much of the idiosyncratic differences between neighborhoods. Please see the discussion in section 6.

Table 2.6.3—Robustness, by crime rate type, population-weighted least squares, collapse by week

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
	All crime	Homicide	Rape	Agg. Assault	Robbery	Theft	Burglary
Install	0.0011	-0.0008	-0.0023	-0.0249	0.0111	-0.0149	0.0329
	(0.0864)	(0.0026)	(0.0026)	(0.0338)	(0.0111)	(0.0560)	(0.0256)
Median Household Income	-0.0000***	-0.0000**	-0.0000**	-0.0000**	-0.0000**	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Black population percentage	0.1801	0.0116***	0.0069	0.2455**	-0.0042	-0.3052	0.2254**
	(0.5062)	(0.0038)	(0.0076)	(0.1114)	(0.0737)	(0.3191)	(0.0938)
Unemployment rate	-1.5645	0.0006	0.0055	-0.5418	-0.2437	-0.3896	-0.3954
	(1.7788)	(0.0199)	(0.0313)	(0.4156)	(0.2622)	(1.0550)	(0.3800)
Kansas dummy	-0.9354***	-0.0033**	-0.0068**	-0.1785***	-0.1532***	-0.5606**	-0.0330
	(0.3057)	(0.0015)	(0.0029)	(0.0343)	(0.0335)	(0.2350)	(0.0449)
Constant	2.7476***	0.0054	0.0209**	0.3815***	0.2473***	1.5976***	0.4949***
	(0.5001)	(0.0038)	(0.0101)	(0.1053)	(0.0892)	(0.2536)	(0.0979)
Time fixed effects (week)	X	X	X	X	X	X	X
Demand level fixed effects	X	X	X	X	X	X	X
Observations	21,112	21,112	21,112	21,112	21,112	21,112	21,112
R-squared	0.2443	0.0070	0.0162	0.0741	0.0705	0.2762	0.0596

Standard errors in parentheses, clustered at the neighborhood level, population weighted least squares.
*** p<0.01, ** p<0.05, * p<0.10

Notes. These are a robustness check of my benchmark results collapsed by week instead of by date. Each column represents a specification using the crime rate type listed below as the dependent variable. I find no statistically significant effects of installation for any crime type. The controls for these regressions include time fixed effects by week and preregistration level fixed effects and the demographic variables listed above. Please see section 6 for more discussion.

Table 3.1—Summary description of experimental conditions

Condition Title	Condition Description
Random Assignment	Randomly receive prize from lottery.
Solo Competition	Participate in competition solo, awarded prizes to top 50 percent.
Group competition	Participate in competition in a group, awarded prizes to top 50 percent.

Notes. This table outlines the types of conditions used in this paper. The first competition condition is the solo competition, where participants compete without being part of a group. The second is the group competition, where participants are grouped with a partner, competing as a group against other groups. The random assignment condition represents a baseline to judge whether there are any reasonable endowment effects observed in competition itself.

Table 3.2—Valuation and market results

	Random Assignment		Solo Competition		Group Competition	
	Sellers	*Buyers*	*Sellers*	*Buyers*	*Sellers*	*Buyers*
Market Price ($) (mean)	$5.75		$4.65		$5.85	
Valuation ($)						
Mean	$5.65	$5.36	$6.75	$3.39	$7.17	$4.04
Median	$7	$5.75	$6.75	$3.00	$7.25	$3.25
Standard Deviation	$2.20	$2.76	$2.12	$2.34	$1.48	$2.87
Market price / Mug Price (mean)	0.82		0.62		0.78	
Valuation / Mug Price						
Mean	0.81	0.77	0.89	0.45	0.95	0.54
Median	1.00	0.82	0.87	0.43	0.93	0.44
Standard Deviation	0.32	0.39	0.28	0.30	0.18	0.41
N (participants)	13	12	26	23	24	24
N (total participants)	25		49		48	
Trades	6		7		8	
Traders	12		14		14	
Percent Trading	48%		29%		33%	

Notes. This above table shows a summary of the valuation results. The mean valuation, measured in the dollars or in the valuation ratio (valuation divided by the mug price to control for a $1 higher priced mug in 2015) is lower for buyers than sellers, across random, solo, or group competitions. The statistical significance of this difference in tested in the benchmark regression in Table 4. For the random assignment, I find 48 percent of participants traded with another participant. This is in stark contrast to other endowment effect experiments which find a much lower percentage of participants trading. Please see the results section for an in depth discussion.

Table 3.3—Effort descriptive statistics

Envelopes Filled	Solo Competition		Group Competition	
	Sellers	*Buyers*	*Sellers*	*Buyers*
Mean	14.4	10.4	13.3	10.8
Median	14	10	14	11
Standard Deviation	1.8	2.4	2.1	1.7
Total	374	240	319	259
Overall total	614		577	
Mean (all)	12.5		12.0	
Median	13		12	
Max	18		17	
Min	3		6	
N (participants)	26	23	24	24
N (total participants)	49		48	

Notes. This table displays descriptive statistics concerning effort, the number of envelopes filled per type of competition. Across competitions, the mean envelopes filled for the solo participants was only 0.5 envelopes higher than the group competition. The median envelopes was only one higher, 13 versus 12. Table 5 presents OLS regression results testing the statistical significance of this difference, and finds none. This implies that regardless of competition structure, we can expect the same amount of output. The mean and median envelopes filled for sellers are higher than buyers, as is expected given that they won the competition.

Table 3.4—Benchmark OLS regression results

	[1]
	Solo vs. Group
	Dependent variable
VARIABLES	Valuation ratio
Seller	0.44***
	(0.09)
Group competition	0.09
	(0.09)
Seller * Group competition	-0.04
	(0.12)
Constant	0.45***
	(0.06)
Observations	97
R-squared	0.34

*Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10*

Notes. This table shows the benchmark ordinary least squares specification as shown in section 3.1. In this specification I test for potential valuation effects of the group competition versus the solo competition. The dependent variable is the ratio between the participants valuation over the store price of the mug. I find a positive and statistically significant effect at the one percent level of the *Seller* variable. The interpretation of this effect is that becoming a seller (winning the competition) results in a 44 percent increase in valuation regardless of mug price. Please see the results section for an in depth discussion.

Table 3.5—Effort OLS regression results

VARIABLES	[2] Solo vs. Group Envelopes filled
Group competition	-0.51
	(0.53)
Constant	12.53***
	(0.37)
Observations	97
R-squared	0.01

*Standard errors in parentheses, *** p<0.01, **p<0.05, * p<0.10*

Notes. This table shows the results from testing for potential group competition effects on envelopes filled, a measure of effort. This test is designed to show if there are any effort effects of belonging to a group versus solo in a competition. In the group competition, the groups in the top 50 percent of the trial received a mug. Therefore, it is possible for a team member to shirk and still receive a prize. The results shown above find no effect of group-based competitions versus solo competitions on effort produced. The sign of the coefficient is negative, which might be expected given the shirking story mentioned earlier, however it is not statistically significant at conventional levels. Ordinary least squares regression is used, standard errors are in parentheses. Please see the results section for an in depth discussion.

Table 3.6—Competition versus random assignment OLS regression results

	[3]
	Competition vs. Random
	Dependent variable
VARIABLES	Valuation ratio
Seller	0.04
	(0.13)
Competition	-0.27***
	(0.10)
Seller * Competition	0.38***
	(0.14)
Constant	0.77***
	(0.09)
Observations	122
R-squared	0.27

*Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10*

Notes. In specification [2] above I test for valuation effects between any type of competition and random assignment, and their interaction. The dependent variable is the ratio between the participants valuation over the store price of the mug. The coefficient on the *Seller* variable disappears in this specification, compared to the benchmark, as it now represents the effect of winning the prize during the random assignment condition alone. I find negative and statistically significant valuation effects of losing any competition versus random assignment. I also find a positive and statistically significant coefficient on the interaction between *Seller* and *Competition* with the solo and group competitions, showing that the only evidence of the endowment effect is shown in a competition, and not in the random assignment condition. Please see the results section for an in depth discussion.

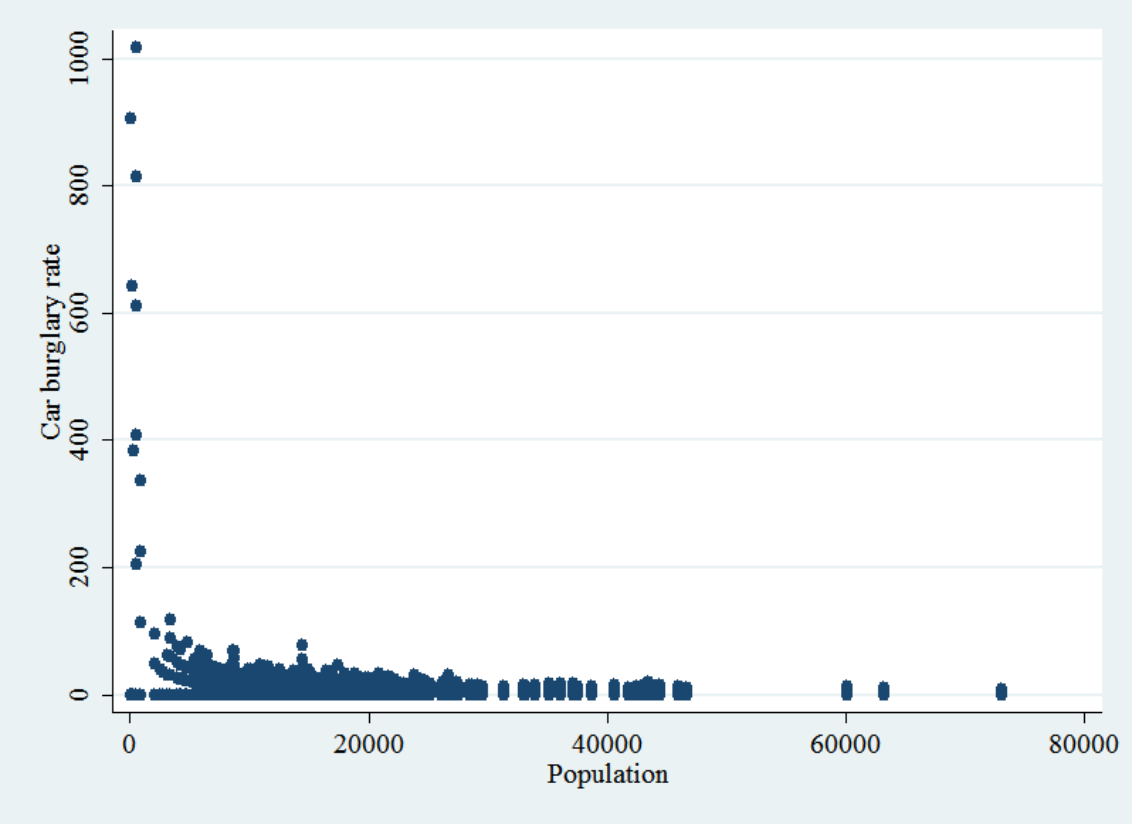
Table 3.7— Robustness specifications: valuation in dollars

	[R1]	[R2]
	Solo vs. Group	Solo vs. Group
	Dependent variable	
VARIABLES	Valuation ($)	Valuation ($)
Seller	3.36***	3.36***
	(0.65)	(0.65)
Group competition	0.65	0.64
	(0.66)	(0.66)
Seller * group competition	-0.23	-0.23
	(0.92)	(0.92)
2015 year control		0.10
		(0.46)
Constant	3.39***	3.34***
	(0.47)	(0.53)
Observations	97	97
R-squared	0.35	0.30

Notes. This table shows the results from a robustness check which uses the elicited valuations in dollars rather than the valuation ratio used in the benchmark regressions (models shown in sections 3.1 and 3.3). This test is designed to see if the main results of the paper are replicated after controlling for a later year effect (2015 year control) to potentially capture any omitted differential mug effect that could be affecting my results. I find very similar results across the robustness specifications compared to the benchmark specifications. Please see the robustness section for additional discussion.

Appendix A

Figure A1—Car burglary rate vs. population



Notes. This is a plot of car burglary rates of each library-radii-zone vs their population. This figure shows how the variance of car burglary rates, as is any other crime rate, is inversely proportional with population size. This shows the prevalence of heteroscedasticity in crime rate data, and the necessity to utilize weighted ordinary least squares, weighted by population size. This ensures that the lowest population areas, which have the highest variance, will have the least weight.

Table A1—Above 5th percentile crime rate interaction, population-weighted least squares

Beginning Date	(1) All crime	(2) Violent Non Gang	(3) Assault Non Gang	(4) Homicide Non Gang	(5) Robbery Non Gang	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
LibHours	-0.0123	0.0141	0.0133	-0.0000	0.0008	-0.0343	-0.0016	-0.0011	-0.0006	-0.0049	-0.026**
	(0.0361)	(0.0153)	(0.0115)	(0.0009)	(0.0061)	(0.0318)	(0.0143)	(0.0036)	(0.0103)	(0.0123)	(0.0121)
LibHours*Crime5th	0.0334	-0.0005	-0.0044	-0.0005*	0.0044	0.0368*	0.0118	0.0016*	0.0053	0.0163*	0.0017
	(0.0262)	(0.0088)	(0.0057)	(0.0003)	(0.0039)	(0.0213)	(0.0089)	(0.0009)	(0.0035)	(0.0095)	(0.0075)
Elem.S. days	-0.0330	0.0017	-0.0030	0.0010***	0.0037	-0.0296*	0.0000	0.0026	-0.011**	0.0004	-0.02***
	(0.0198)	(0.0069)	(0.0052)	(0.0003)	(0.0040)	(0.0150)	(0.0070)	(0.0022)	(0.0052)	(0.0083)	(0.0044)
Middle.S. days	0.0193	-0.0058	-0.0008	-0.0003	-0.0048	0.0281	0.0131	0.0012	-0.0139*	0.0144	0.0133
	(0.0339)	(0.0101)	(0.0069)	(0.0006)	(0.0065)	(0.0254)	(0.0128)	(0.0023)	(0.0071)	(0.0155)	(0.0134)
High.S.days	-0.0007	-0.0060	-0.0035	0.0003	-0.0028	0.0022	0.0044	0.0004	-0.0043	-0.0136	0.0153
	(0.0418)	(0.0164)	(0.0100)	(0.0007)	(0.0090)	(0.0293)	(0.0132)	(0.0050)	(0.0084)	(0.0139)	(0.0130)
Other S. days	0.0534	-0.0058	-0.0070	-0.0019**	0.0031	0.0604	0.0137	-0.0024	0.0126	0.0181	0.0185
	(0.0506)	(0.0172)	(0.0119)	(0.0008)	(0.0106)	(0.0370)	(0.0148)	(0.0039)	(0.0119)	(0.0213)	(0.0177)
Constant	3.968***	0.099	-0.023	-0.001	0.122***	3.9066***	1.48***	0.020	0.276***	0.524***	1.61***
	(0.2267)	(0.0953)	(0.0719)	(0.0056)	(0.0384)	(0.1992)	(0.0914)	(0.0214)	(0.0639)	(0.0754)	(0.0757)
Time (day) and Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270	72,270
R-squared	0.3218	0.2416	0.1189	0.0187	0.1798	0.2554	0.2627	0.0696	0.0952	0.0979	0.1121

*Standard in parentheses, clustered at the library level, population weighted least squares, *** p<0.01, ** p<0.05, * p<0.10, Date range equal to one day or zero, Beginning date, Overall Library Zones, summed library zones*

Notes. This is the last of three interaction specifications, the specifications in this table interact library hours with an indicator variable describing whether the library-zone (not library radii-zones) are in 5thth percentile of crime rates or greater. This includes 95 percent of library-zones, and is meant to distinguish between areas with very little crime and everywhere else. Of special note are the negative and statistically significant estimates of non-gang homicide and burglary from vehicle. Additional controls include daily fixed effects and library-zone fixed effects. Weights are by population by library-zone.

Table A2—Library-zone by date fixed effects, population-weighted least squares

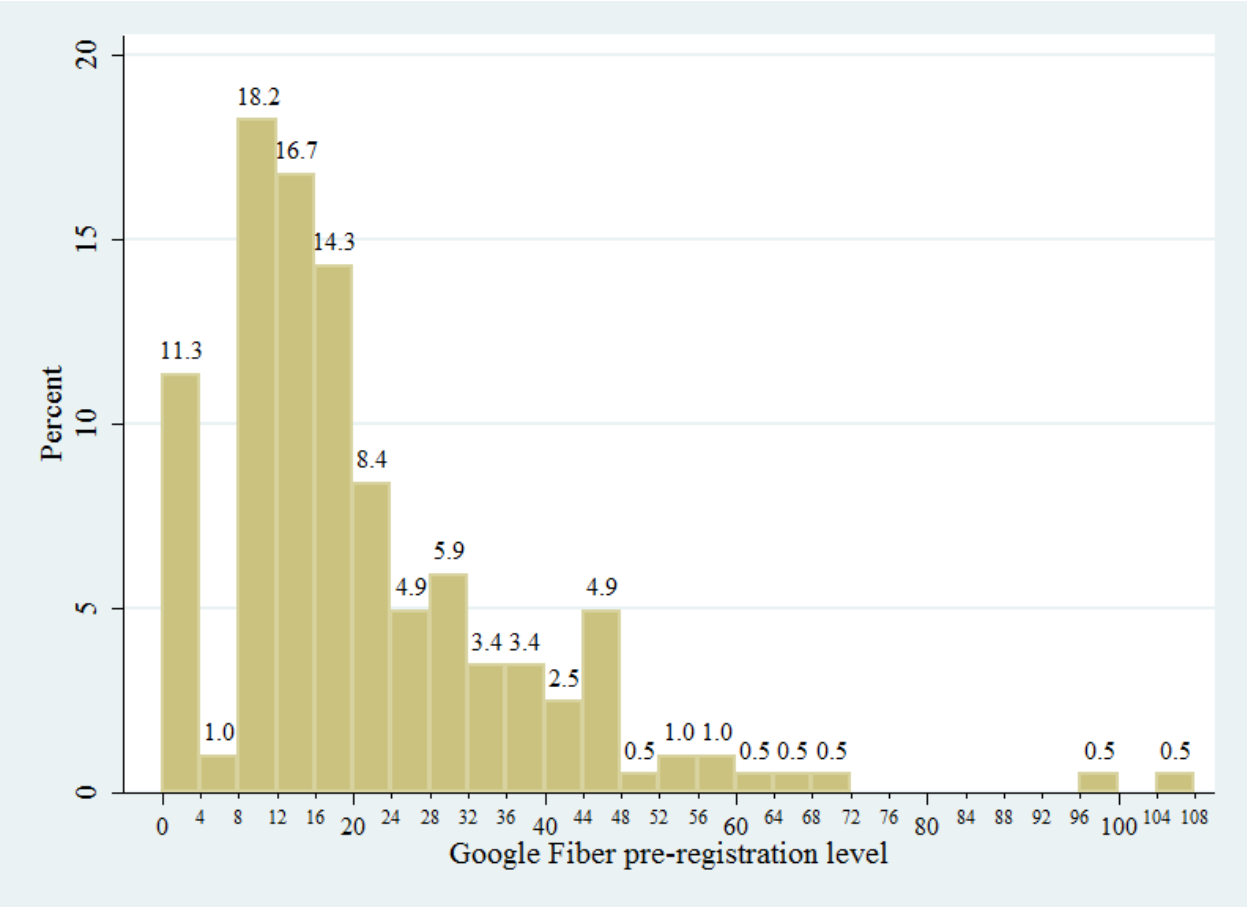
Beginning Date	(1) All crime	(2) Violent Non gang	(3) Assault Non gang	(4) Homicide Non gang	(5) Robbery Non gang	(6) Property	(7) Theft	(8) Grand Theft Person	(9) Grand Theft Auto	(10) Burglary	(11) Burglary from Vehicle
(r=0.6) * LibHours	-0.0237	-0.0074	-0.0063	0.0004	-0.0015	-0.0175	0.0075	0.0020	-0.0021	-0.0047	-0.0201**
	(0.0245)	(0.0075)	(0.0049)	(0.0004)	(0.0044)	(0.0214)	(0.0109)	(0.0073)	(0.0022)	(0.0070)	(0.0098)
(r=1.0) * LibHours	-0.052***	-0.0152**	-0.0078**	0.0004	-0.0077*	-0.0350**	-0.0104	-0.0016	-0.0020	-0.0023	-0.019***
	(0.0196)	(0.0060)	(0.0037)	(0.0005)	(0.0043)	(0.0174)	(0.0101)	(0.0067)	(0.0017)	(0.0070)	(0.0060)
Elem.S. days	-0.156	-0.0174	-0.0058	0.0006	-0.0121	-0.1377	-0.0860*	-0.0259**	0.0005	0.0117	-0.0380
	(0.0974)	(0.0182)	(0.0055)	(0.0005)	(0.0135)	(0.0863)	(0.0479)	(0.0127)	(0.0051)	(0.0148)	(0.0268)
Middle.S. days	0.058	-0.013	0.000	-0.001	-0.012	0.0753	0.1112	-0.0130	0.0020	-0.0244	-0.0005
	(0.1892)	(0.0259)	(0.0082)	(0.0009)	(0.0204)	(0.1734)	(0.1139)	(0.0279)	(0.0053)	(0.0269)	(0.0478)
High.S.days	-0.0889	-0.0312	-0.0093	-0.0002	-0.0216	-0.0616	-0.0340	-0.0068	-0.0023	0.0111	-0.0296
	(0.1517)	(0.0330)	(0.0113)	(0.0011)	(0.0235)	(0.1226)	(0.0696)	(0.0240)	(0.0053)	(0.0234)	(0.0359)
Other S. days	0.2775	0.0158	-0.0009	-0.0014	0.0180	0.2531	0.0217	0.0397	0.0054	0.0723	0.1141
	(0.4484)	(0.0891)	(0.0276)	(0.0022)	(0.0638)	(0.3537)	(0.1513)	(0.0750)	(0.0149)	(0.0626)	(0.1166)
Constant	7.95***	1.170***	0.489***	0.010***	0.672***	6.474***	1.952***	1.245***	0.091***	1.134***	2.053***
	(0.2974)	(0.0579)	(0.0243)	(0.0016)	(0.0396)	(0.2700)	(0.1636)	(0.0499)	(0.0123)	(0.0399)	(0.0836)
Time (day) * Library-radii fixed effects	X	X	X	X	X	X	X	X	X	X	X
Observations	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830	214,830
R-squared	0.3665	0.3507	0.3192	0.3469	0.3379	0.3487	0.3625	0.3071	0.3145	0.3480	0.3126

*Standard in parentheses, clustered at the library level, *** p<0.01, ** p<0.05, * p<0.10, date range equal to one day or zero*

Notes. This is a difference in differences specification with library-zone by date and radii controls, instead of the separate library-radii-zone and date fixed effects present in the benchmark model. By controlling for library-radii-zone by date, I am able to control for between library-zone by date variation. However, to identify the effects of library hours, I must drop the base LibHours variable. The interpretation of the two coefficients of interest remain the same, as they represent the marginal change in the crime rate given a one hour increase of library hours, differenced from areas far away. Additional controls include daily fixed effects and library-radii-zone fixed effects. Weights are by population by library-radii-zone.

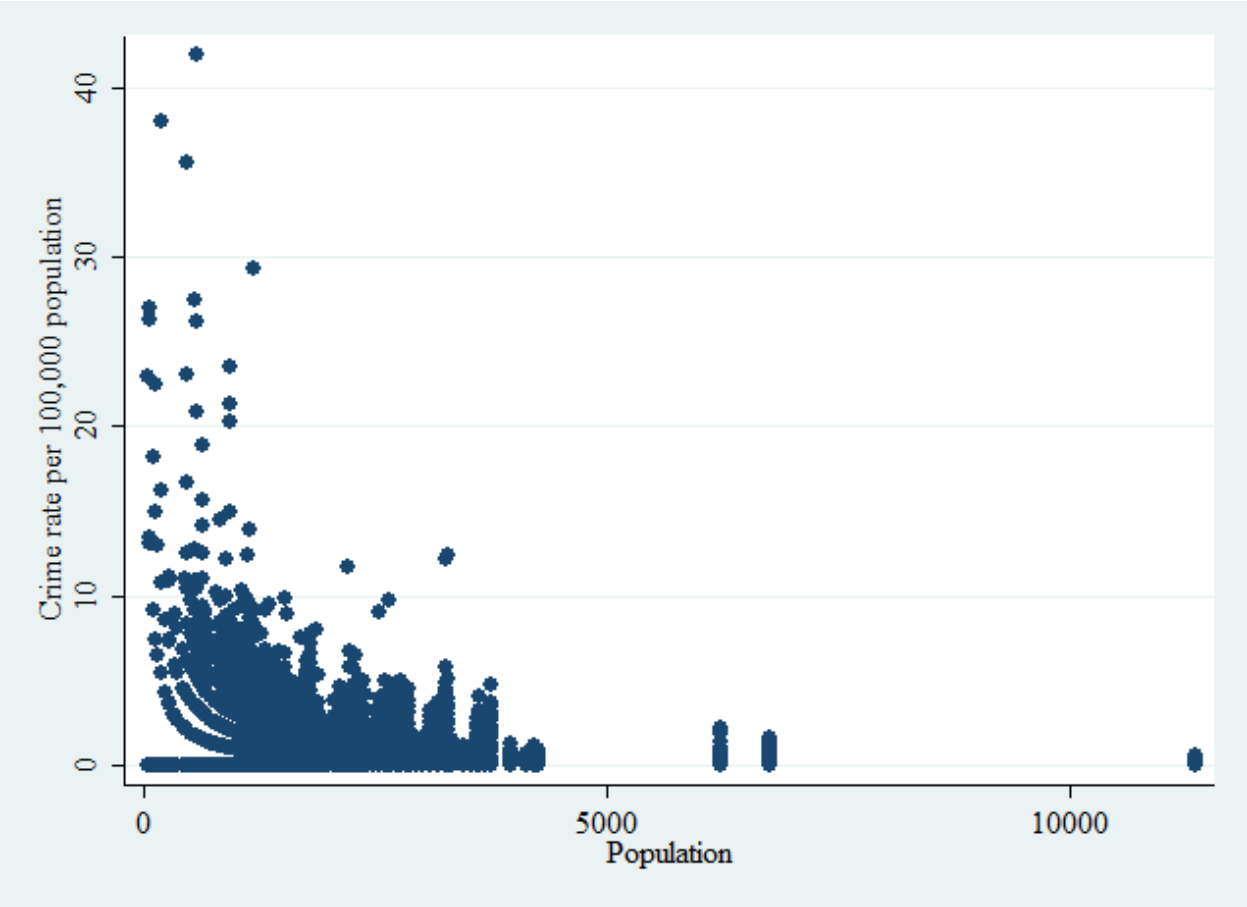
Appendix B

Figure B1—Google Fiber preregistration level histogram



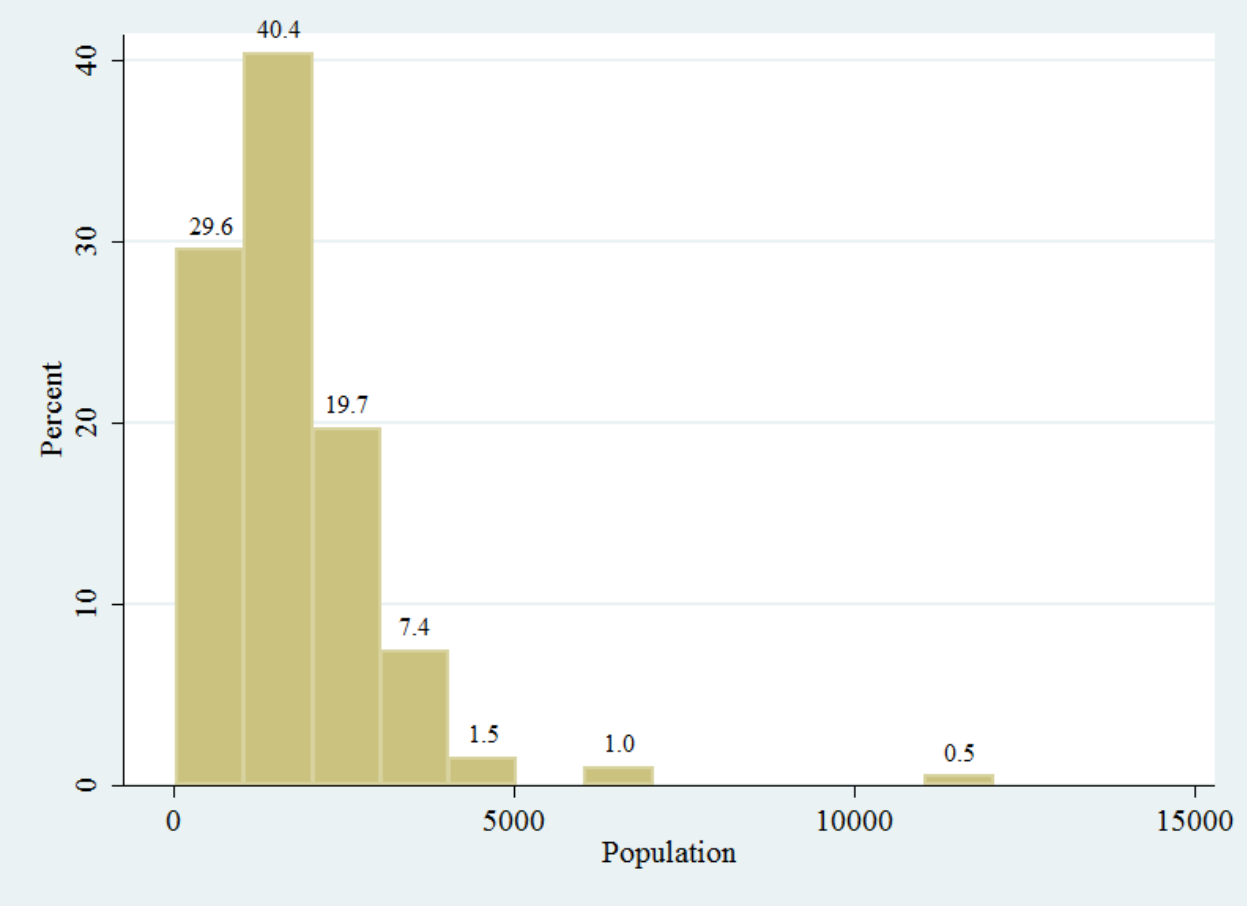
Notes. This histogram shows the counts of each Google Fiber preregistration level as a percentage of the whole in bins with width of four percentage points preregistration. There are no neighborhoods with preregistration levels less or equal to four percent than are not recorded as zero. Zero demand represents neighborhoods that did not reach their registration quota and as of 2013 did not receive Google Fiber. From the Figure, we can see that this group represents 11.3 percent of neighborhoods. Values over 100 percent are possible due to an underestimation of the number of households in a neighborhood.

Figure B2—Overall crime rate vs population, Kansas City, Kansas & Missouri, 2012-2013



Notes. This graph plots the crime rate for Kansas City, Kansas and Kansas City, Missouri for 2012-2013 against the neighborhood population count. As we can see from the graph, as the population count grows larger, the variance between estimates shrinks. This shows the importance of using weighted least squares, so that the lower populated neighborhoods do not, which have a higher variance, do not have undue influence over my estimates.

Figure B3—Overall histogram by neighborhood population



Notes. This histogram shows the neighborhood population counts. The bin width is set to population counts of 1000. The population counts were retrieved from the 2012 Census.

Table B1—Preregistration level descriptive statistics: “0” through 30 percent

Pre-reg. level	N	Pop	Median HH Income ($1,000s)	White%	Black%	Unemp. Rate	Kansas%
		Mean					
“0”	23	1,684	$31.20	31%	53%	16%	22%
5	1	134	$22.31	9%	81%	16%	100%
7	1	3,699	$21.20	47%	14%	16%	100%
8	8	2,416	$33.06	27%	49%	20%	75%
9	2	2,429	$49.42	44%	18%	11%	0%
10	3	2,624	$30.10	40%	33%	15%	67%
11	24	1,727	$31.69	19%	69%	17%	21%
12	12	1,340	$27.26	22%	69%	15%	17%
13	6	1,807	$31.01	37%	41%	17%	50%
14	11	2,003	$31.46	32%	47%	15%	64%
15	5	1,537	$33.17	32%	50%	16%	80%
16	10	1,759	$46.49	49%	32%	11%	60%
17	8	2,599	$43.88	60%	26%	10%	63%
18	6	2,820	$41.92	48%	36%	13%	17%
19	5	1,667	$43.41	53%	37%	12%	60%
20	5	2,169	$42.15	46%	46%	10%	20%
21	4	3,950	$40.33	56%	25%	12%	75%
22	1	2,216	$56.39	86%	5%	7%	0%
23	7	1,280	$48.54	62%	24%	10%	57%
24	1	1,710	$35.56	60%	32%	5%	0%
25	4	1,544	$34.66	65%	22%	11%	25%
26	3	1,206	$77.85	80%	8%	7%	0%
27	2	1,423	$41.30	43%	46%	16%	50%
28	3	859	$37.37	43%	42%	14%	67%
29	3	853	$45.04	67%	20%	8%	33%
30	4	1,958	$41.15	67%	18%	11%	25%
Mean	6.2	1,900	$39.15	47%	36%	13%	44%
Overall Mean	4.06	1,630	$52.75	62%	25%	10%	33%

Notes. This table displays descriptive statistics for all posisble levels of Google Fiber preregistration levels for the lowest through 30 percent, while the Table on the following page displays 31 through 105. The lowest demand is set to “0” in the sample, which represents a neighborhood that did not meet their neighborhood’s preregistration cutoff goal, thus Google Fiber was never installed.

Table B2—Preregistration level descriptive statistics: 31 through 105 percent

Pre-reg. level	N	Pop	Median HH Income ($1,000s)	White%	Black%	Unemp. Rate	Kansas%
		Mean					
31	2	456	$52.72	77%	14%	13%	50%
32	1	547	$69.16	81%	12%	7%	100%
33	4	1,828	$40.99	65%	23%	10%	25%
35	2	1,282	$63.02	50%	40%	10%	100%
36	1	1,861	$78.00	92%	3%	5%	0%
37	4	1,004	$72.70	85%	10%	5%	25%
38	2	1,528	$47.79	79%	11%	6%	0%
40	1	1,773	$43.03	54%	35%	8%	0%
41	1	2,675	$29.24	88%	6%	10%	0%
42	1	1,242	$102.42	78%	16%	8%	100%
43	2	814	$111.24	85%	8%	5%	0%
44	2	1,137	$133.18	87%	3%	5%	0%
45	3	1,142	$100.54	93%	3%	5%	0%
46	3	1,284	$73.05	90%	6%	7%	33%
47	2	1,342	$46.77	66%	21%	9%	0%
48	1	2,550	$40.33	79%	15%	4%	0%
52	1	1,109	$65.59	76%	18%	8%	0%
55	1	800	$51.01	94%	2%	11%	0%
56	2	729	$70.33	79%	7%	5%	50%
62	1	1,796	$56.94	84%	4%	9%	0%
65	1	669	$116.94	80%	4%	7%	0%
69	1	1,505	$81.51	92%	2%	8%	0%
99	1	1,873	$26.06	53%	34%	15%	0%
105	1	1,123	$47.24	74%	15%	14%	0%
Mean	1.7	1,336	$67.49	78%	13%	8%	20%
Overall Mean	4.1	1,630	$52.75	62%	25%	10%	33%

Notes. This table displayed descriptive statistics for all posisble levels of Google Fiber preregistration levels for 31 through 105 percent. The maximum was listed over 100 percent possibly due to errors in Google's household per neighborhood calculations.

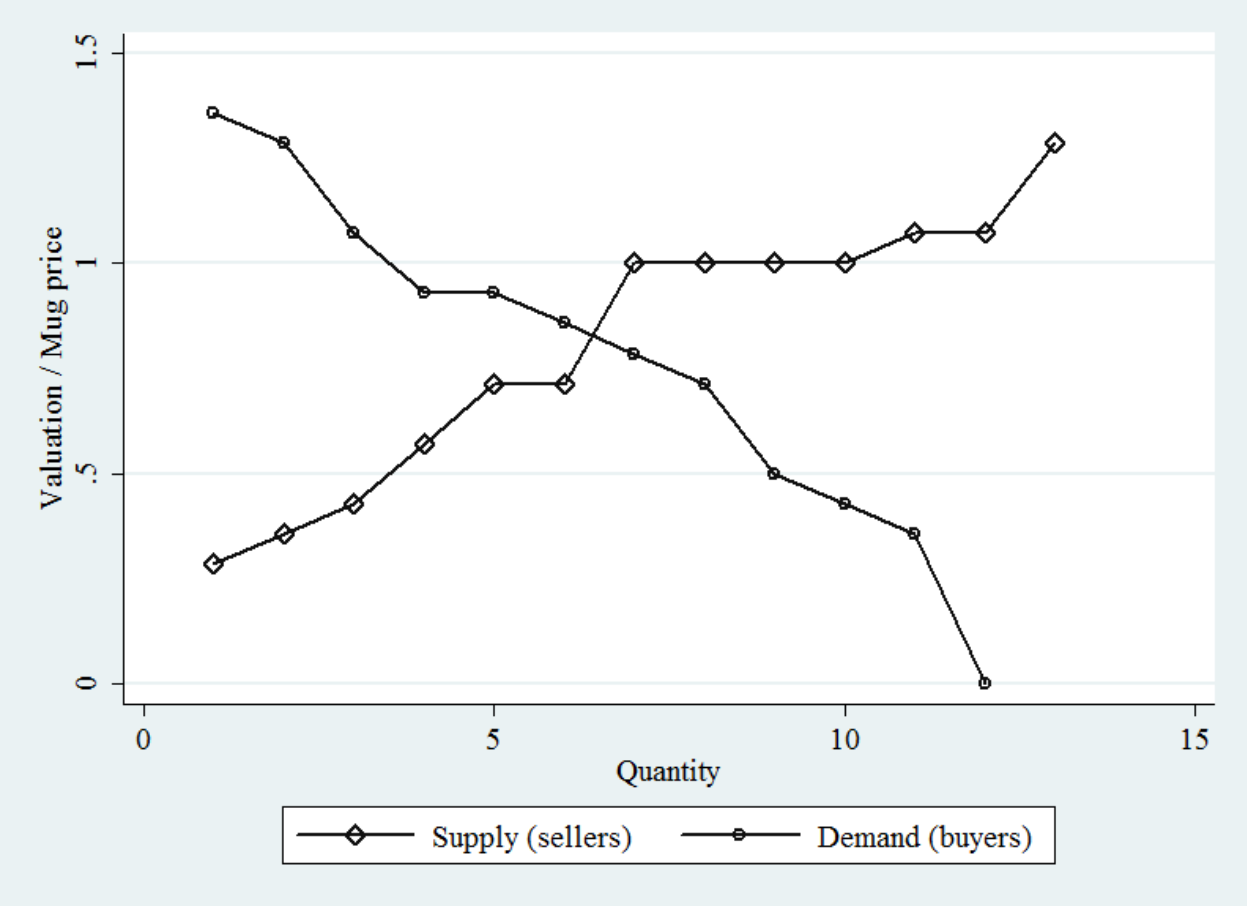
Table B3—Kansas City, Missouri and Oklahoma City, Oklahoma comparison

Demographics	Kansas City, MO	Oklahoma City, OK	KC / OKC
Population estimates, July 1, 2013	467,007	610,613	0.76
Persons under 5 years, percent	7.5	7.9	0.95
Persons under 18 years, percent	24.2	25.4	0.95
Persons 65 years and over, percent	11	11.3	0.97
Female persons, percent	51.5	50.8	1.01
White alone, percent	59.2	62.7	0.94
White alone, not Hispanic or Latino, percent	54.9	56.7	0.97
Black or African American alone, percent	29.9	15.1	1.98
American Indian and Alaska Native alone, percent	0.5	3.5	0.14
Asian alone, percent	2.5	4	0.63
Native Hawaiian and Other Pacific Islander alone, percent	0.2	0.1	2.00
Hispanic or Latino, percent	10	17.2	0.58
Households, 2009-2013	192,648	227,700	0.85
Persons per household, 2009-2013	2.36	2.55	0.93
High school graduate or higher*	87.5	84.8	1.03
Bachelor's degree or higher*	31.3	28.1	1.11
With a disability**	9.3	9.7	0.96
Foreign born persons, percent, 2009-2013	7.6	12	0.63
In civilian labor force, total***	68.3	66.4	1.03
In civilian labor force, female***	64.7	60.3	1.07
Mean travel time to work (minutes)****	21.3	20.3	1.05
Median household income (in 2013 dollars), 2009-2013	$45,275	$45,824	0.99
Per capita income in past 12 months (in 2013 dollars), 2009-2013	26889	25640	1.05
Owner-occupied housing unit rate, 2009-2013	56.3	59.4	0.95
Persons in poverty, percent	19.1	18.2	1.05

Estimates from April 1, 2010 unless otherwise stated
**percent of persons age 25 years, 2009-2013, **under age 65 years, percent, 2009-2013*
****percent of population age 16 years, 2009-2013, ****workers age 16 years, 2009-2013*
Source: U.S. Census Bureau

Appendix C

Figure C1— Rando assignment: supply and demand



Notes. This figure displays the valuation ratio (elicited valuation divided buy the mug retail purchase price versus the quantity available in the random assignment trial. The circles represent demand, and the diamonds represent supply. The valuation ratio is used instead of the raw price data to control for a $1 increase in mug price between the first set of trials and the second. The random assignment trial was only done in the first set of trials, however the valuation ratio is used to make easy comparisons between figures. The valuation ratio is very close to 1 at the equilibrium value, which represents a close approximation of the retail price of the mug. Of the participants in the market, 48 percent were traders, which is what we would expect in an efficient market. This shows no evidence of the endowment effect, which would predict much less than 50 percent trading.