The Effect of Income on Fertility: Evidence from a Randomized Study of Cash Transfers

Sumit Agarwal and Jeffrey Liebman[1]

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ABSTRACT

We provide new evidence on the causal effect of income on fertility by studying a randomized experiment of a monthly cash transfer. In the midst of the Covid-19 pandemic, a low-income city just outside of Boston, Massachusetts, allocated monthly cash benefits for nine months, worth up to $3,600 in total, via lottery. The cash transfer resulted in a large increase in pregnancies and live births. Detailed health information from linked electronic medical records allowed us to examine the mechanisms underlying income's effect on fertility, including whether there are changes to fecundity or family planning. There was no significant effect on miscarriages or problems with ovulation as markers of fecundity. Examining reproductive decision making, there was no significant effect on contraception or medical and procedural abortions. There was, however, an increase in prescriptions for prenatal vitamins in preparation for pregnancy and evaluations for infertility. The effects were largest for multiparous women between the ages of 30-35, implying that the cash transfer enabled women who were already mothers and facing declining fecundity to have another child. Despite the well-documented negative relationship between income and fertility in the cross-section, our findings suggest that, at least for the population we studied at the lower end of the income distribution, the causal relationship between income and fertility is positive, consistent with the idea of children as "normal" goods.

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1. Introduction

Starting with his 1960 paper and expounded upon in subsequent work by himself and others, Becker put forth a compelling explanation for the inverse relationship between income and fertility that is consistent with the more favored idea of children as "normal" goods (Becker 1960, 1965; Becker and Lewis 1973; Willis 1973; Becker and Tomes 1976; Becker and Barro 1988; Hotz, Klerman, and Willis 1997). A key insight was that higher wages can have two opposing effects, a positive income effect and negative substitution effect given that children can be costly in terms of time. An alternative but related explanation, one preferred by Becker himself (Becker 1991), is that the effective price of children rises with higher wages due to an interaction between the quantity and quality of children. In this formulation, children, more broadly construed in terms of both quality and quantity, are normal goods; fertility as a narrower reflection of child quantity can thereupon decrease with income if demand for child quality is positive and sufficiently more elastic with respect to income. In this paper, we provide new evidence on the causal effect of income on child quantity.

Although there have been several advances in the economics of fertility (Doepke et al. 2023), challenges remain in empirically testing the theory's assumptions or predictions. Inherent difficulties include separating the income and substitution effects and identifying plausibly exogenous variation in prices or income. Consider the causal effect of income, the focus of this paper.[2] One group of studies leverages exogenous changes to men's income, though these tend to rely on an assumption that men's time is less important to the household production of children to isolate the income effect (e.g., Black et al. 2013; Kearney and Wilson 2018). Another group of studies examines the impact of cash transfers on fertility, including the Alaska Permanent Fund Dividend, negative income tax, child tax credit, and earned income tax credit, though the latter three in particular may have effects on other important margins that could also affect fertility given their conditional nature on employment or number of children (e.g., Yonzan, Timilsina, and Kelly 2024; Keeley 1980; Crump, Goda, and Mumford 2011; Bastian 2017). Finally, several additional studies have leveraged shocks to housing wealth or lottery winnings (e.g., Lovenheim and Mumford 2013; Cesarini et al. 2023). No studies, however, have examined the fertility

[2] By "causal effect of income," we refer to the pure income effect, as opposed to the effect from a change in *wages*, which have both substitution and income effects.

response utilizing a randomized study design of an unconditional cash transfer as we do in this paper, which can more definitively isolate the effect of income on fertility.

Furthermore, the underpinnings of a theoretically positive income effect remain underexamined, particularly from a reproductive perspective. Our use of detailed electronic health record data allows us to examine the potential mechanisms underlying income's effect on fertility and is thus a strength over vital records data on which prior studies of fertility have relied. We observe not only births but also pregnancies as well as decisions related to reproductive health care. If the income effect were positive as typically assumed, does additional income result in prospective parents intentionally choosing to have children? Are they discontinuing contraception, increasing their frequency of sex, seeking specialized fertility care, or after conception choosing against medical and procedural abortion? Or does income reduce financial stress or improve food insecurity such that fecundity increases resulting in, for example, fewer miscarriages?

Before examining mechanisms, however, we first establish whether the income effect is indeed positive. If there are "obstacles to the production or prevention of children" (Becker 1991), as may be true in certain segments of the population (Cowan and Wyndham-Douds 2022), the direction of income's effect on fertility is theoretically ambiguous. Low-income or uninsured individuals, for example, may not have ready access to assisted reproductive technology or effective contraception, and if additional income relieves one or both constraints, the effect could go in either direction. Unpacking the determinants of fertility decisions could have implications for proposals under consideration to expand income support and for understanding the current fertility rate in the U.S., which has dropped to historic lows.[3]

Leveraging a randomized controlled study of a monthly cash transfer and detailed electronic health care records, we provide strong empirical evidence that the causal effect of income on fertility is indeed positive and then examine the underlying behaviors that drive the increase in pregnancy and birth rates. During 2020-2021, the City of Chelsea, a low-income city just outside of Boston, Massachusetts, allocated monthly cash benefits for nine months, worth up to $3,600 in total, via lottery. Most applicants were women of reproductive age. We obtained and matched participants to their electronic health record data from multiple health systems,

[3] The total fertility rate in the U.S. has fallen from 2.12 in 2007 to 1.64 per woman in 2020 (Congressional Budget Office 2025).

restricted the study population to women aged 14 to 44, and compared outcomes for individuals randomized into the cash transfer group with individuals who were randomized into the control group. The lottery provided a unique opportunity to evaluate the casual effects of income on fertility using a randomized controlled study design. The results, first and foremost, reject the hypothesis that additional income has no effect on fertility. Live births were significantly greater in the cash transfer group, and the magnitude was large. The live birth rate increased by 3.2 percentage points, a 60% increase relative to the control mean of 5.3%.

The electronic health record data provide several ways to further examine this result beyond vital records since it captures reproductive and obstetric care. Over the nine-month study period, study participants randomized to receive the cash transfer had significantly more pregnancies than the control group by 3.6 percentage points, or a 60% increase relative to the control mean of 6%. This finding is concordant with the increase in the live birth rate. Over 85% of these pregnancies in both the cash transfer and control groups resulted in live birth. Of note, excluding pregnancies that began prior to the study period increases the precision of the estimate for the pregnancy rate without substantively changing the effect size, suggesting that the effect is driven by new, incident pregnancies rather than continuations of existing pregnancies.

One set of potential mechanisms is increased fecundity ceteris paribus. Fecundity is difficult to measure, much less observe, but two pieces of evidence suggest that fecundity was unchanged. There was no significant effect of the cash transfer on miscarriages (spontaneous abortions) or problems with ovulation, as determined by health care visits for amenorrhea (absence of menstrual periods) or oligomenorrhea (infrequent menstrual periods). Another set of potential mechanisms, therefore, is changes in reproductive decision making. After conception, fewer medical or procedural abortions (sometimes referred to as elective abortions) could lead to more live births in the cash transfer group. The effect of the cash transfers on medical and procedural abortions, though in the negative direction, was small in magnitude and not statistically significant. Medical and procedural abortions were notably rare in our study population compared to the national rate. We next examined whether the increase in fertility is driven by changes in family planning prior to conception, for example discontinuing (or not seeking) contraception, changing the frequency or timing of sexual intercourse, or seeking specialized fertility care. Our electronic health record data allows to measure prescriptions for birth control sent to the pharmacy and removals of long-acting reversible contraception. We find

no significant difference in prescriptions for birth control or discontinuation of long-acting reversible contraception. In fact, the point estimate is in the positive direction and significantly so in the same subgroup of women in whom the fertility effect is concentrated. It could instead be that women not seeking contraception may be changing their behavior as a result of the cash transfer and actively trying to conceive. We find some support for this. A higher proportion of women in the cash transfer group were prescribed a prenatal vitamin in preparation for pregnancy compared to women in the control group. Furthermore, a higher proportion of women in the cash transfer group sought care for infertility compared to the control group. Subgroup analyses reveal that the effect on pregnancies and births is concentrated most on multiparous women in their early 30s, implying that the cash transfer enabled women who were already mothers and facing declining fecundity to have another child.

Our paper relates most closely to existing research documenting the causal effect of income on fertility in the U.S. We are, to our knowledge, one of the first to examine this effect using a randomized controlled study design, and the findings are consistent with several quasi-experimental studies that have mostly found that fertility increases with income. Additional income can help overcome access barriers to health care, so our paper also relates to the literature on access to birth control and infertility treatment. Finally, we also contribute to the growing literature on the effects of guaranteed income and other similar forms of cash transfers to low-income groups. These studies have tended to focus on labor market and health outcomes; we bring new evidence of the effect of a guaranteed income program on another important outcome, fertility. We conclude that, at least for low-income women who may face constraints to reproductive autonomy, additional income can substantially increase fertility rates.

2. Background

2.1. Why might income affect fertility

Fertility, defined as the ability to reproduce, reflects a complex, physiologic process of conception, pregnancy, and birth. There are four basic steps in conception: sperm transport, egg transport, fertilization, and implantation (Price and Valea 2022). The pregnancy hormone human chorionic gonadotropin (hCG) can be detected in the blood within 48 hours of implantation and subsequently in the urine. An estimated 10 to 30% of clinically recognized pregnancies results in pregnancy loss, and the large majority (80%) of pregnancy loss occurs during the first trimester.

Maternal age is a strong predictor of spontaneous abortion (miscarriage), and the risk of pregnancy loss declines with gestational age. Some pregnancies are terminated via medical or procedural abortion. In the U.S. during 2020-2021, there were approximately 11-12 terminations of pregnancy per 1,000 women of reproductive age, or about 100 per 1,000 live births (Kortsmit et al. 2022, 2023). Only live births are counted towards the fertility rate.

Additional income, holding wages and prices constant, could affect multiple steps in this pathway from conception to live birth, particularly among low-income women. Undesired pregnancies are more common among low-income women who face greater barriers to effective contraception (Dehlendorf et al. 2010; Bailey et al. 2023). Contraception, such as condoms, oral contraceptive pills (OCPs) and intrauterine devices (IUDs), prevent pregnancy in the steps prior to implantation. Additional income could either improve individuals' access to contraception or lead individuals to discontinue contraception in the hopes of becoming pregnant. If income increases the desirability of pregnancy, individuals may also change the frequency and timing of sexual intercourse to improve their chances of conceiving, which occurs maximally around the time of ovulation (Wilcox, Weinberg, and Baird 1995). After conception, additional income could decrease the likelihood of terminating a pregnancy, given that many women cite economic hardship as a contributing factor in the decision to terminate (Finer et al. 2005; Miller, Wherry, and Foster 2023). Independent of pregnancy desirability and consequent behaviors to increase the chances of pregnancy and birth, income could improve fecundity. Psychological distress is associated with infertility, and to the extent that this relationship is causal and if income reduces such distress, additional income could improve fecundity within the conception process or by lowering the rate of miscarriage (Wu et al. 2022). There is similarly some evidence that malnutrition increases the risk of spontaneous abortion (Abu-Saad and Fraser 2010). For women who are unable to conceive despite attempts, income could improve access to health care for the evaluation and management of infertility via assisted reproductive technology, for which access differs across socioeconomic status (Bitler and Schmidt 2012).

2.2. Existing evidence of the income-fertility relationship

The negative correlation between income and fertility is well-documented (Jones and Tertilt 2008; Kearney, Levine, and Pardue 2022).[4] Here we focus on quasi-experimental studies of how income causally affects fertility. One group of studies leverages exogenous changes to men's income. Black et al. (2013) find that increases in world energy prices in the 1970s, which raised incomes for men in the Appalachian coal-mining region of West Virginia, Kentucky, Ohio, and Pennsylvania, increased fertility. Kearney and Wilson (2018) find that higher male wages from localized fracking booms across multiple U.S. states in the late 2000s increases fertility but not family formation. Schaller (2016) uses variation in male and female demand for labor from 1980-2009 and finds that improvements in men's labor market conditions increases fertility whereas the effects are smaller and in the negative direction for improvements in women's labor market conditions. There are similar findings by Amialchuk (2013) using relative wage changes and Autor, Dorn, and Hanson (2019) using gender-specific large-scale demand shocks. Common in these studies is an assumption that men's time is less important to the household production of children to isolate the income effect. Related to this group of studies are those that leverage involuntary job displacement and generally find smaller, negative effects on fertility (Lindo 2010; Amialchuk 2013b; Ananat, Gassman-Pines, and Gibson-Davis 2013; Bono, Weber, and Winter-Ebmer 2015; Huttunen and Kellokumpu 2016). As some of these studies have noted, however, job loss may affect fertility through other mechanisms besides loss of income.

Another group of studies leverages exogenous changes in wealth from lottery winnings. As discussed in a recent review cash transfers, however, unexpected one-time windfalls as from winning the lottery may produce different effects compared to smaller but regular and expected transfers (Page 2024). Cesarini et al. (2023) find that the Swedish lottery increases male but not female fertility, potentially due to the increased marriage formation among men. Tsai et al. (2023) similarly find that Taiwanese lottery winners have higher fertility and marriage rates. Bulman, Goodman, and Isen (2022) use U.S. tax data and state lottery wins between 2000 and 2019; they find that total fertility is unchanged, but births happen sooner, potentially due to effects on homeownership and marriage formation. A study by Lovenheim and Mumford (2013)

[4] The correlation exists over time and birth cohorts, across countries, and within countries, at least since the demographic transition in the early 19th century. Also see Joseph Hotz, Klerman, and Willis (1997) and Doepke et al. (2023) for additional overviews. The correlation across countries may be weakening in recent years among high-income countries (Doepke et al. 2023).

use data from the Panel Study of Income Dynamics and finds that positive wealth changes driven by housing market variation increases fertility.

A final group of studies examines the impact of cash transfers on fertility. Two studies of the Alaska Permanent Fund Dividend find that the yearly cash transfer increases fertility, with the relative effects largest for socioeconomically disadvantaged populations and women ages 25-34 (Cowan and Wyndham-Douds 2022; Yonzan, Timilsina, and Kelly 2024). A randomized experiment of a negative income tax in the 1960s showed that the program decreased fertility for Whites and increased fertility for Chicanas (Keeley 1980). The amount of the negative income tax, however, varied by the number of children and may therefore not isolate the income effect. The empirical evidence for the effect of the earned income tax credit on fertility is mixed. These studies similarly are unlikely to isolate the income effect as there are many margins on which the earned income tax credit can have effects that also affect fertility since it could affect marriage formation, is conditional on work, and the amount varies by household size (Bastian 2017). Child tax benefits may also affect fertility. This has been examined by a number of studies, including one by Whittington, Alm, and Peters (1990) who find an increase in fertility and reexamined by Crump, Goda, and Mumford (2011) who find effect sizes of smaller magnitudes and suggest it only effects the timing of fertility but not overall levels. The recent 2021 expansion of the child tax credit into a monthly cash benefit may have affected fertility but is difficult to disaggregate from other changes during the pandemic (Bailey, Currie, and Schwandt 2023). Child tax benefits, however, have both income and price effects.

3. The Chelsea Eats Study

The City of Chelsea was disproportionately affected by the health and economic consequences of the COVID-19 pandemic. By the end of 2020, the city had the highest COVID-19 cumulative incidence rate in Massachusetts (Figueroa et al. 2020). Furthermore, the population of Chelsea was heavily concentrated in sectors of the economy that shut down, and its large undocumented population was not eligible for unemployment insurance or federal Economic Impact Payments (Rangadass and Dominguez-Santos 2021). After operating a large food distribution program from April through August 2020, the City decided to redirect its efforts toward providing direct financial support to its residents. The city accepted applications between July 27, 2020 and August 17, 2020 for “Chelsea Eats” debit cards (Figure A1). Applicants with

one person, two people, or at least three people in the family could receive $200, $300, or $400 per month, respectively.

To be eligible, individuals had to live in Chelsea and have a family income below 30% of the median income for the Boston metropolitan statistical area (i.e., $26,850 for one-person households, $30,700 for two-person households, $34,550 for three-person households, $38,350 for four-person households, $41,450 for five-person households, $44,500 for six-person households, $47,600 for seven-person households, and $50,650 for eight-person households). Only one individual from each family was permitted to enter the lottery. City workers with tablet computers walked along the lines at food distribution sites, signing residents up for the lottery. Community groups also assisted residents in applying, and residents could apply directly on the City's website or by submitting a paper application.

The City of Chelsea announced that, "Families with children, disabled residents, veterans, seniors over 65, households that have been economically impacted by COVID-19, and households that are ineligible for other forms of federal assistance, such as SNAP and WIC, will have preference" in the lottery. In practice, these preferences were implemented by giving applicants a different number of lottery "tickets" depending on how many criteria they met. The eight criteria were: (1) no one in the family was currently working; (2) no one in the family was receiving income support through unemployment insurance; (3) the applicant was not receiving food assistance (e.g., from the Supplemental Nutrition Assistance Program); (4) the applicant was disabled; (5) the applicant was a veteran; (6) there was a family member who was under 6 years of age living at the same address; (7) there was a family member between the ages of 6 and 17 living at the same address; and (8) there was a family member over 65 years of age living at the same address. The probability of winning the lottery was therefore higher among participants with more tickets (Table A1, Table A2), and we accounted for the differential probability of winning the lottery across applicants in our statistical analysis (details below).

Randomization via the lottery occurred at the individual level and was held on September 17, 2020, and was broadcast live on the City's Facebook page. Individuals randomly selected in the lottery could pick up their debit cards at Chelsea City Hall. The debit cards, first filled on November 24, 2020, and refilled monthly for 8 additional months, could be spent anywhere Visa was accepted.

At the time of the lottery, the City created a randomized waitlist that was not initially publicized but could be used to allocate unused cards in case any lottery winners were determined to be ineligible or did not claim their cards. Ultimately, a small percentage of additional applicants from the waitlist (<5%) were offered debit cards and are included in the treatment group. While these individuals did not receive their cards until December, their cards were credited with both the November and December payments, and their outcomes were assessed over the same intervention period as the other applicants in the treatment and control groups. In the event that individuals reported lost debit cards, their cards were replaced so that they could continue to receive funds going forward but funds for prior months were not restored. The payments were initially intended to run for six months. Given the ongoing economic and public health difficulties in Chelsea, the city and its philanthropic partners extended the program for an additional three months. The total cost of the program was approximately $9 million.

After excluding households who had multiple applications in the lottery, there were 1,746 individuals allocated to the cash transfer group and 1,134 individuals allocated to the control group (Figure A2). The study population was 77% female. The mean family size was 3.2, and the mean annual family income was $16,721. Of treatment group individuals, 1,606 (92%) picked up their debit card. Card recipients spent a mean of $335 per month, which represented, on average, a 23% increase in baseline monthly income. For this analysis, we further restricted our study population to women of reproductive age, which includes women ages 15 through 44 as defined by the Centers for Disease Control and Prevention, resulting in 741 women in the cash transfer group and 505 women in the control group. As shown in Table 1, characteristics were balanced between the cash transfer and control groups.

4. Data and Outcomes

We obtained electronic health record data for 2019 through 2022 from three major health systems encompassing 15 hospitals, 30 health centers, and many additional outpatient clinics throughout the Boston area. These three health systems account for approximately 78% of the emergency, inpatient, and outpatient care among Chelsea residents. Lottery participants were probabilistically linked to their medical records at each health system on the basis of application data, including name, address, phone number, date of birth, and gender; 95% of the study

population linked to at least one health system (Table A3). The electronic health record data included detailed information on date and type of visit, department and specialty, and diagnoses.

To assess the coverage of acute care from our electronic health record data, we used the Massachusetts Case Mix Database, which comprises detailed information on inpatient discharges, emergency department visits, and observation stays from all Massachusetts hospitals. Using data from 2017 through 2019, we calculated that 77% of all acute care visits among residents of Chelsea are captured by the 15 hospitals in our data. Specifically, 41% of acute care visits are to Cambridge Health Alliance's Everett Hospital and 27% of acute care visits are to Mass General Brigham's Massachusetts General Hospital. Geographically, these are the two closest hospitals to the City of Chelsea (Figure A3). Another 9% of acute care visits are to other hospitals in the Cambridge Health Alliance and Mass General Brigham health systems that are represented in our data.

To assess the coverage of outpatient care from our electronic health record data, we used the Massachusetts All-Payer Claims Database. Using data from 2016, we calculated that 78% of all outpatient visits among residents of Chelsea are captured by the health systems in our data. Specifically, 6% of outpatient visits are to Cambridge Health Alliance, 43% are to Mass General Brigham, and 29% are to East Boston Neighborhood Health Center. Our data included Massachusetts General Hospital's Chelsea HealthCare Center and the East Boston Neighborhood Health Center, the two largest providers of outpatient health care in Chelsea.

Our study period extends from November 24, 2020, the first date the debit cards were filled, through August 31, 2021, four weeks after the date the debit cards were refilled for the last time, representing an approximately nine-month observation period. The pre-randomization period included January 1, 2019, the date our data begin, through September 16, 2020, the day before the lottery.

5. Estimation

We fit a linear regression model in an intention-to-treat analysis to compare outcomes for individuals randomized into the treatment group with individuals who were randomized into the control group. Specifically, we estimated the following linear regression model: $y_i = \beta_0 + \beta_1 \mathrm{CASH}_i + \beta_3 \mathbf{X}_i + \epsilon_i$, where y_i is the utilization or clinical outcome for individual i. "$CASH_i$" is an indicator for whether individual i won the lottery and was thus randomized into the treatment

group. $\boldsymbol{X_i}$ is a vector of covariates, specifically patient characteristics (i.e., age in categories), that were included to increase precision. We used dummy variable adjustment for missing patient characteristics, and we weight observations as described below. The weights ensure that the treated and untreated groups are balanced and exchangeable. The coefficient on CASH_i, β_1, is our main coefficient of interest; it provides the adjusted difference in means between the treatment groups and the control group. Standard errors were adjusted for heteroskedasticity. The protocol, which included our prespecified analysis plan and outcomes, was published before analysis of the data (Agarwal and Liebman 2022).

There are two identifying assumptions necessary for the difference between treatment and control group averages to equal the average causal effect. First, Y_1 and Y_0 must be independent of treatment assignment (D). In a simple experiment with a single probability of assignment to the treatment group, randomization directly produces this independence. In our setting, in which the probability of winning the lottery varies with the number of lottery tickets an individual receives, the key condition is that Y_1 and Y_0 are independent of treatment assignment conditional on the number of lottery tickets (L), in other words $(Y_1, Y_0) \perp\!\!\!\perp \mathrm{D|L}$. The overall average causal effect (ACE) is then estimated by taking a weighted average of the estimates for groups of individuals with the same number of lottery tickets: $\widehat{ACE} = \sum_{L=1}^{L=8} W_L \left(\bar{Y}_L^{Winner} - \bar{Y}_L^{Loser}\right)$, where $\bar{Y}_L^{Winner}$ and $\bar{Y}_L^{Loser}$ are average outcomes among lottery winners and lottery losers among individuals with L tickets, where the number of tickets ranged from $L = 1$ to $L = 8$ (Table A1), and W_L is the weight for lottery group L. Because we are interested in the average causal effect for the population that was the target of the city's intervention, those who received the cash transfer, we weight the lottery groups by the proportion of individuals in the treatment group with that number of lottery tickets: $W_L = \frac{\#\ of\ treatment\ group\ individuals\ with\ L\ tickets}{total\ number\ of\ treatment\ group\ individuals}$. This "average treatment effect on the treated" can be estimated in a regression context by pooling all observations and assigning a weight of 1 to each individual in the treatment group and a weight of (p_L/1-p_L), where p_L is the fraction of individuals with L lottery tickets who won the lottery (Table A2), to each individual who received L lottery tickets in the control group.

Second, we assume no interference (sometimes called the "stable unit treatment value assumption"), meaning that the outcomes of a given sample member are not affected by the treatment status of other sample members. Given that the sample members in this study all come

from the same community, it is easy to imagine scenarios in which some interference might occur (e.g. if one sample member uses the emergency room less, the wait time for another sample member could go down). In practice, what we are assuming is that the effect of any interference that occurs is very small relative to the direct effects of receiving cash transfer.

We plan on conducting several sensitivity analyses to ensure that our results are robust to alternative specifications. In the first sensitivity analysis, we will trim the sample to exclude ticket groups in which there were fewer than ten participants in either the cash transfer group or control group to ensure adequate sample for comparisons and avoid some participants having excessive weight. In the second sensitivity analysis, we will saturate the model with the same covariates from the main specification as well as all the baseline characteristics from the application as listed in Table 1. In the third sensitivity analysis, we will use an alternative set of weights, the inverse probability treatment weight, in which treated participants are weighted by 1 divided by the probability of winning the lottery (i.e., 1/p) and control participants are weighted by 1 divided by the probability of not winning the lottery (i.e., 1/[1-p]). In the fourth sensitivity analysis, we will include indicators for number of lottery tickets in the model as an alternative to weighting to account for differential probability of winning the lottery across ticket groups. In the fifth sensitivity analysis, we will use a standard instrumental-variable approach, in which lottery selection was the instrument for receiving the cash card, to account for less than complete take up and estimate the local average treatment effect of receiving a cash card.

6. Results

The effect of the unconditional cash transfer on pregnancy and live birth are presented in Table 2. The first two columns display mean outcomes for the control and cash transfer groups, respectively; the effect estimates and associated standard errors and p-values follow in the final two columns. A major advantage of the electronic health record data is that we observe not only births as in vital records but also clinically recognized pregnancies and the subsequent result of those pregnancies. The first row shows results for our primary outcome. In the control group, 6% of women were pregnant at some point during the nine-month intervention period. The cash transfer increased the proportion of women who were pregnant by 3.6 percentage points, or a 60% increase relative to the control mean, a statistically significant difference ($p < 0.020$).

The next row of Table 2 shows the effect of the cash transfer on becoming pregnant. In other words, we excluded from the outcome measure any pregnancies that began (i.e., were conceived) prior to the study period based on the date of the last menstrual period. The estimate is similar to that of the primary outcome but more precise, suggesting that the effect on the pregnancy rate is driven by new, incident pregnancies rather than continuations of existing pregnancies.

The following three rows show the result of clinically recognized pregnancies from the first row of Table 2, whether they resulted in spontaneous abortion (also known as miscarriage), medical and procedural abortion (also known as elective abortion), or live birth. There were no intrauterine fetal demises or perinatal deaths in our sample, which is a relatively rare outcome for a pregnancy at 5.54 per 1,000 live births in 2021 (Valenzuela, Gregory, and Martin 2023). There was no significant difference between the control and cash transfer groups on miscarriages. As described earlier, 10% to 30% of clinically recognized pregnancies results in pregnancy loss, so the miscarriage rate in our study population falls within the range of the expected miscarriage rate. In fact, the estimate of the effect of cash transfers is in the positive direction, reflecting the higher rate of pregnancies in the cash transfer group and that not all pregnancies result in live birth.[5] There was also no significant difference between the control and cash transfer groups on medical and procedural abortions; the estimate was in the negative direction and small in magnitude. Medical and procedural abortions were notably rare in our study population compared to the national rate of 10 to 11 per 1000 women of reproductive age.

In Table 3, we turn to other potential explanations for the increase in live births, since the effect cannot be explained by either fewer miscarriages or fewer medical and procedural abortions, which is consistent with the earlier analysis of becoming pregnant showing that the effect is driven by new pregnancies rather than after conception in continuation of existing pregnancies. One such explanation is discontinuation of contraception. With the electronic health record data, we can observe when physicians prescribe birth control including oral, injectable, or long-acting reversible contraception. We can also observe removals of long-acting reversible

[5] The positive point estimate for spontaneous abortion could also reflect a higher rate of pregnancies that are detected or clinically recognized since our measure of pregnancy relies on contact with the health care system. However, we do not consider this to be a significant explanation for the positive but insignificant point estimate. There was no difference between the control and cash transfer groups in the gestational age at which contact with the health care system occurs upon becoming pregnant.

contraception. We cannot, however, observe whether patients go to the pharmacy and fill these prescriptions,[6] a more proximal event to and arguably better proxy for actual use compared to prescribing, especially given that prescriptions can have multiple refills that may or may not go filled. Nonetheless, as shown in the first set of results in Table 3, we find no significant difference between the control and cash transfer groups in prescriptions for contraception or removals of long-acting reversible contraception. Interestingly, the point estimate is in the positive direction, and in subgroup analyses, there is a significant increase in contraception prescriptions among women with two children at baseline, the same group in whom there is a significant increase in pregnancies and live births, suggesting that there are two groups of women, one in whom the cash transfer improves access to contraception and another in whom the cash transfer increases fertility.

Miscarriage occurs after an embryo implants in the uterus but is unviable or loses viability. The lack of a decrease in the miscarriage rate suggests against the cash transfer increasing fecundity, but fecundity is generally difficult to measure.[7] It reflects the natural or physiologic capacity to become pregnant and achieve a live birth; therefore, it is a function of not only viability upon implantation as miscarriage measures but also steps after or before implantation such as egg quality and ovulation. Another potential marker of fecundity is the regularity of menstrual periods. With the electronic health record data, we can measure visits to the physician for evaluation of irregular periods (oligomenorrhea) or altogether absent periods (amenorrhea), which can be caused by severe malnutrition and are known contributors to infertility. We find no change in visits or diagnoses for oligomenorrhea or amenorrhea. We interpret the lack of an effect on miscarriages or visits for irregularity of menstrual periods as suggestive that the cash transfer is not substantially affecting fecundity as the underlying cause for the increased birth rates.

The final two sets of results in Table 3 offer a potential explanation for the increased fertility. Compared to the control group, there were significantly more initiations of prenatal vitamins prior to any prenatal visit. Because prenatal vitamins are available over the counter at the pharmacy, physicians may not necessarily prescribe or document their use in the electronic medical record. The public insurer Medicaid, however, covers prenatal vitamins, increasing the

[6] Prescription claims data would have this information, and we are in the process of analyzing this data.
[7] Fecundity is often proxied with but is distinct from fertility.

likelihood of physicians prescribing the medication such that would be observable in the medical records. Overall, the cash transfer should not differentially change documentation patterns between the two groups, so the means of prenatal vitamin use in the control and cash transfer groups may be conservative and similarly biased downwards. Compared to the control group, there were significantly more visits and diagnoses for evaluation of infertility. These two sets of results – prescriptions for prenatal vitamins and evaluations of infertility – suggest that the increase in birth rates may have been intentional related to increased desire and attempts to become pregnant.

Finally, Table 4 and Table 5 show the results by maternal age and baseline number of children, respectively. These results show that the increase in pregnancy and live births is concentrated among women younger than 35 years of age, and more specifically between the ages of 30 and 35, and women with two children at baseline. Table 6 shows effect heterogeneity using endogenous stratification in which we use the relationship between baseline covariates, specifically maternal age and number of prior children as well as their interactions, and the outcomes in the control group to predict the potential outcomes in the treatment group and then estimate effects across predicted terciles (Abadie, Chingos, and West 2018). The effect on pregnancy and live birth is largest among women in the lowest predicted tercile.

7. Discussion

Using a randomized controlled study design and linked electronic health record data, we found that a monthly cash transfer increased pregnancies and live births. Despite the well-documented negative relationship between income and fertility in the cross-section, our findings suggest that, at least for the population we studied at the lower end of the income distribution, the causal relationship between income and fertility is positive, consistent with the idea of children as "normal" goods. Underlying this increase in fertility could be an increase in fecundity to the extent that financial and food insecurity contribute to lower fecundity or an increase in behaviors that promote conception. We find more evidence for the latter than the former.

One possible interpretation of our results is that cash transfer may simply change the timing of fertility (intemporal substitution) rather than overall fertility. This is unlikely to be the case given that the increase in fertility was concentrated among women ages 30-35 and delays

among this age group of even one year appreciably reduce fecundity. We plan to do further research on the long-term impacts on fertility in this sample in the future.

Our results are consistent with several quasi-experimental studies that have mostly found that fertility increases with income, though the magnitudes of our estimates are larger than those of prior studies, perhaps reflecting differences in study design that isolate the income effect from opposing price effects or the size of the cash transfer, particularly relative to baseline income. Our effects are also larger than what might be predicted from child subsidies of the same size, despite child subsidies theoretically having income and price effects in the same direction. Theories that consider state-dependent preferences or hyperbolic discounting could potentially reconcile these differences.

It is also worth considering the setting and time period in which the cash transfers experiment occurred. The City of Chelsea from which our study sample comes, in addition to being low-income, is a heavily Latino and foreign-born community in which there may be cultural differences around family planning (Sangi-Haghpeykar et al. 2006). For instance, we found that the elective abortion rate in our study population was lower compared to the national rate, which could reflect access barriers related to socioeconomic status or cultural differences. Furthermore, the study was conducted during the early years of the pandemic. One study of women in California found that "foreign-born women drove the 2020 baby bust and that US-born women drove the 2021 baby bump" (Bailey, Currie, and Schwandt 2023). It is unclear why the distinction between foreign-born versus US-born drove this pattern, but it could be related to the relative socioeconomic status of these two groups. Additional income for those at the lower end of the income distribution in our study could have mitigated against a counterfactual "baby bust" during the early years of the pandemic.

The cash transfer also increased prescriptions for contraception, suggesting that the cash transfer increased access to birth control, which all else constant would decrease fertility. While there are inequities in access to contraception, Massachusetts Medicaid covers contraception at no cost to consumers, and the Affordable Care Act requires that most private insurers do the same. Therefore, the cash transfer would have had to improve access specifically among the uninsured or by making it easier to see a prescriber among the insured. This is consistent with our prior work showing that the cash transfer increased access to outpatient care (Agarwal, Cook, and Liebman 2024). Whether women previously faced barriers to contraception or elected to

have fewer children than they desire due to financial constraints, we conclude that cash transfers improved reproductive autonomy for the low-income women in this study.

Table 1: Baseline Characteristics of Study Population

	Control (1)	Cash Transfer (2)	P value (3)
Observations, N	505	741	
Demographics			
Female	1.00	1.00	1.000
Age	34.23	33.86	0.331
Disability	0.07	0.10	0.216
Veteran	0.00	0.00	0.905
Spanish as preferred language	0.78	0.78	0.827
Diagnosed with depression or anxiety	0.28	0.26	0.495
Health care utilization			
Any emergency department use	0.26	0.25	0.871
Any outpatient use	0.86	0.88	0.419
Any primary care visit	0.76	0.76	0.996
Family characteristics			
Family annual income, $	17,749.76	18,565.07	0.296
Family size	3.75	3.77	0.799
Number of children	2.08	2.09	0.954
At least one child ages 0-5	0.62	0.61	0.679
At least one child ages 6-17	0.75	0.75	0.851
At least one older adult age 65+	0.08	0.11	0.121
At least one family member working	0.50	0.52	0.557
Receiving unemployment assistance	0.19	0.17	0.269
Receiving food assistance (e.g., SNAP)	0.56	0.53	0.272

Notes: Columns (1) and (2) show the means for the control and cash transfer groups, respectively. Column (3) reports the p-value of the difference between the two study arms.

Table 2: Effect of Income on Pregnancy, Spontaneous Abortion, Medical and Procedural Abortion, and Live Birth

	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)
Pregnant	.060	.099	0.036 (0.016)	[0.020]
Became pregnant	.026	.062	0.034 (0.011)	[0.003]
Spontaneous abortion	.005	.011	0.005 (0.005)	[0.361]
Medical and procedural abortion	.003	.003	-0.001 (0.003)	[0.844]
Live birth	.053	.085	0.030 (0.015)	[0.042]
Observations	505	741		

Notes: Columns (1) and (2) show the means for the control and cash transfer groups, respectively. Column (3) shows the estimated effect of the cash transfer for each of the outcomes listed in the rows with the standard error in parentheses. Column (4) reports the p-value of the difference between the two study arms. Statistical inference is based on heteroskedasticity-robust standard errors. All outcomes are binary measures. The first two outcomes were measured over the intervention period from November 24, 2020, through August 31, 2021. The subsequent three outcomes are measured over the intervention period and the following nine months.

Table 3: Effect of Income on Contraception, Ovulation Problems, Prenatal Vitamins, and Fertility Care

	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)
Contraception				
Contraception prescribed	0.203	0.225	0.018 (0.041)	[0.661]
Removal of long-acting reversible contraception	0.015	0.015	-0.000 (0.007)	[0.950]
Ovulation problems				
Number of visits for oligomenorrhea or amenorrhea	0.026	0.023	-0.003 (0.012)	[0.779]
Oligomenorrhea or amenorrhea diagnosed	0.019	0.018	-0.001 (0.008)	[0.891]
Prenatal vitamins				
Prenatal vitamin before initial prenatal visit	0.027	0.050	0.022 (0.011)	[0.045]
Fertility care				
Number of visits for infertility	0.003	0.024	0.021 (0.010)	[0.028]
Infertility diagnosed	0.003	0.012	0.009	[0.051]
Observations	505	741		

Notes: Outcomes related to ovulation problems or fertility care are post hoc outcomes. Columns (1) and (2) show the means for the control and cash transfer groups, respectively. Column (3) shows the estimated effect of the cash transfer for each of the outcomes listed in the rows with the standard error in parentheses. Column (4) reports the p-value of the difference between the two study arms. Statistical inference is based on heteroskedasticity-robust standard errors. Except for two count measures representing number of visits, all other outcomes are binary measures. All outcomes were measured over the intervention period from November 24, 2020, through August 31, 2021.

Table 4: Effect of Income on Outcomes by Maternal Age

	Maternal age 15-34				Maternal age 35-44			
	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)	Control (5)	Cash Transfer (6)	Effect (7)	P value (8)
Pregnant	0.086	0.137	0.051 (0.025)	[0.043]	0.035	0.056	0.021 (0.018)	[0.247]
Became pregnant	0.046	0.096	0.050 (0.020)	[0.015]	0.008	0.025	0.018 (0.010)	[0.079]
Spontaneous abortion	0.011	0.013	0.002 (0.010)	[0.868]	0.000	0.008	0.008 (0.005)	[0.083]
Medical and procedural abortion	0.007	0.003	-0.004 (0.005)	[0.467]	0.000	0.003	0.003 (0.003)	[0.318]
Live birth	0.072	0.121	0.050 (0.023)	[0.035]	0.035	0.045	0.010 (0.017)	[0.575]

(Table continued on next page.)

Table 4, continued: Effect of Income on Outcomes by Maternal Age

	Maternal age 15-34				Maternal age 35-44			
	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)	Control (5)	Cash Transfer (6)	Effect (7)	P value (8)
Contraception prescribed	0.275	0.279	0.004 (0.071)	[0.954]	0.135	0.167	0.032 (0.041)	[0.433]
Removal of long-acting reversible contraception	0.024	0.018	-0.006 (0.012)	[0.631]	0.006	0.011	0.005 (0.007)	[0.480]
Number of visits for oligomenorrhea or amenorrhea	0.011	0.031	0.020 (0.013)	[0.110]	0.041	0.014	-0.027 (0.019)	[0.165]
Oligomenorrhea or amenorrhea diagnosed	0.011	0.023	0.012 (0.010)	[0.213]	0.026	0.011	-0.015 (0.012)	[0.227]
Prenatal vitamin before initial prenatal visit	0.037	0.059	0.022 (0.017)	[0.201]	0.018	0.040	0.021 (0.013)	[0.102]
Number of visits for infertility	0.000	0.028	0.028 (0.013)	[0.034]	0.006	0.020	0.014 (0.014)	[0.322]
Infertility diagnosed	0.000	0.016	0.016 (0.006)	[0.014]	0.006	0.008	0.002 (0.007)	[0.732]
Observations	255	387			250	354		

Notes: This table reports the same outcomes as Tables 2 and 3 but by maternal age. See notes for Tables 2 and 3 for additional details.

Table 5: Effect of Income on Outcomes by Baseline Number of Children

	Zero to One Children at Baseline				Two Children at Baseline				Three or More Children at Baseline			
	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)	Control (5)	Cash Transfer (6)	Effect (7)	P value (8)	Control (9)	Cash Transfer (10)	Effect (11)	P value (12)
Pregnant	0.072	0.112	0.032 (0.028)	[0.251]	0.035	0.103	0.066 (0.023)	[0.004]	0.079	0.079	0.001 (0.032)	[0.974]
Became pregnant	0.035	0.074	0.033 (0.022)	[0.131]	0.021	0.070	0.047 (0.019)	[0.012]	0.023	0.039	0.017 (0.018)	[0.346]
Spontaneous abortion	0.000	0.012	0.011 (0.007)	[0.083]	0.000	0.011	0.011 (0.006)	[0.083]	0.018	0.009	-0.009 (0.014)	[0.511]
Medical and procedural abortion	0.004	0.004	0.001 (0.006)	[0.904]	0.005	0.004	-0.002 (0.007)	[0.760]	0.000	0.000	-	-
Live birth	0.068	0.095	0.020 (0.027)	[0.455]	0.035	0.089	0.052 (0.022)	[0.018]	0.060	0.070	0.010 (0.029)	[0.722]

(Table continued on next page.)

Table 5, continued: Effect of Income on Outcomes by Baseline Number of Children

	Zero to One Children at Baseline				Two Children at Baseline				Three or More Children at Baseline			
	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)	Control (5)	Cash Transfer (6)	Effect (7)	P value (8)	Control (9)	Cash Transfer (10)	Effect (11)	P value (12)
Contraception prescribed	0.189	0.182	-0.011 (0.057)	[0.840]	0.149	0.292	0.138 (0.060)	[0.022]	0.287	0.193	-0.094 (0.095)	[0.327]
Removal of long-acting reversible contraception	0.018	0.017	-0.002 (0.014)	[0.876]	0.021	0.015	-0.007 (0.013)	[0.618]	0.004	0.013	0.010 (0.009)	[0.270]
Number of visits for oligomenorrhea or amenorrhea	0.032	0.037	0.006 (0.021)	[0.779]	0.023	0.022	-0.000 (0.019)	[0.988]	0.025	0.009	-0.016 (0.021)	[0.425]
Oligomenorrhea or amenorrhea diagnosed	0.025	0.029	0.005 (0.016)	[0.776]	0.016	0.015	-0.001 (0.012)	[0.950]	0.016	0.009	-0.007 (0.013)	[0.579]
Prenatal vitamin before initial prenatal visit	0.041	0.058	0.017 (0.021)	[0.420]	0.032	0.052	0.018 (0.019)	[0.337]	0.007	0.039	0.033 (0.015)	[0.026]
Number of visits for infertility	0.006	0.017	0.010 (0.011)	[0.364]	0.003	0.011	0.008 (0.009)	[0.351]	0.000	0.048	0.049 (0.027)	[0.070]
Infertility diagnosed	0.006	0.012	0.006 (0.009)	[0.481]	0.003	0.007	0.005 (0.006)	[0.452]	0.000	0.018	0.018 (0.009)	[0.045]
Observations	187	242			190	271			128	228		

Notes: This table reports the same outcomes as Tables 2 and 3 but by baseline number of children. See notes for Tables 2 and 3 for additional details.

Table 6: Effect of Income on Outcomes by Predicted Live Birth in Terciles

	Lowest Tercile (Mean Maternal Age: 38.6) (Mean No. of Kids at Baseline: 2.0)				Middle Tercile (Mean Maternal Age: 32.4) (Mean No. of Kids at Baseline: 2.6)				Highest Tercile (Mean Maternal Age: 28.8) (Mean No. of Kids at Baseline: 1.6)			
	Control (1)	Cash Transfer (2)	Effect (3)	P value (4)	Control (5)	Cash Transfer (6)	Effect (7)	P value (8)	Control (9)	Cash Transfer (10)	Effect (11)	P value (12)
Pregnant	0.011	0.076	0.065 (0.017)	[0.000]	0.083	0.095	0.012 (0.030)	[0.704]	0.116	0.135	0.018 (0.038)	[0.632]
Became pregnant	0.006	0.055	0.049 (0.015)	[0.001]	0.020	0.045	0.025 (0.017)	[0.139]	0.070	0.091	0.021 (0.031)	[0.501]
Spontaneous abortion	0.000	0.010	0.010 (0.006)	[0.083]	0.008	0.004	-0.005 (0.009)	[0.624]	0.011	0.019	0.008 (0.015)	[0.590]
Medical and procedural abortion	0.000	0.000	-	-	0.006	0.008	0.002 (0.009)	[0.843]	0.005	0.000	-0.005 (0.005)	[0.319]
Live birth	0.011	0.066	0.054 (0.016)	[0.001]	0.074	0.082	0.008 (0.029)	[0.780]	0.100	0.115	0.015 (0.035)	[0.673]

Notes: This table reports the same outcomes as Table 2 but by predicted live birth in terciles. See notes for Tables 2 and 3 for additional details.

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Figure A1: Application Form

CITY OF CHELSEA
DEPARTMENT OF HOUSING AND COMMUNITY DEVELOPMENT
CHELSEA FOOD DEBIT CARD PROGRAM APPLICATION

Instructions: *Please complete the following forms. Incomplete applications will not be processed.*
Completed applications should emailed to: **chelseafooddebitcard@gmail.com**

ALL APPLICATIONS MUST BE SUBMITTED BY END OF BUSINESS DAY ON AUG. 17 th, 2020.

1. Name: _____
2. Address in Chelsea: _____
3. Do you receive mail at this address?

   ☐ YES

   ☐ NO

4. Contact Phone Number: _____
5. Number of Family Members living with you at that address: _____

   How many age 5 or under: _____

   How many age 6 to 17: _____

   How many age 65 or older: _____

6. Are you currently disabled? ☐ YES ☐ NO

7. Are you a Veteran? ☐ YES ☐ NO

8. Annual Income from all family members combined. _____

9. Is anyone in your family working? ☐ YES ☐ NO

10. Is anyone in your family collecting unemployment? ☐ YES ☐ NO

11. Do you receive other food assistance?

☐ SNAP

☐ WIC

☐ P-EBT

☐ I do not receive food assistance.

12. What language do you wish to be communicated in?

☐ English

☐ Spanish

☐ Other: _____

Figure A2: Assessment of Eligibility and Randomization

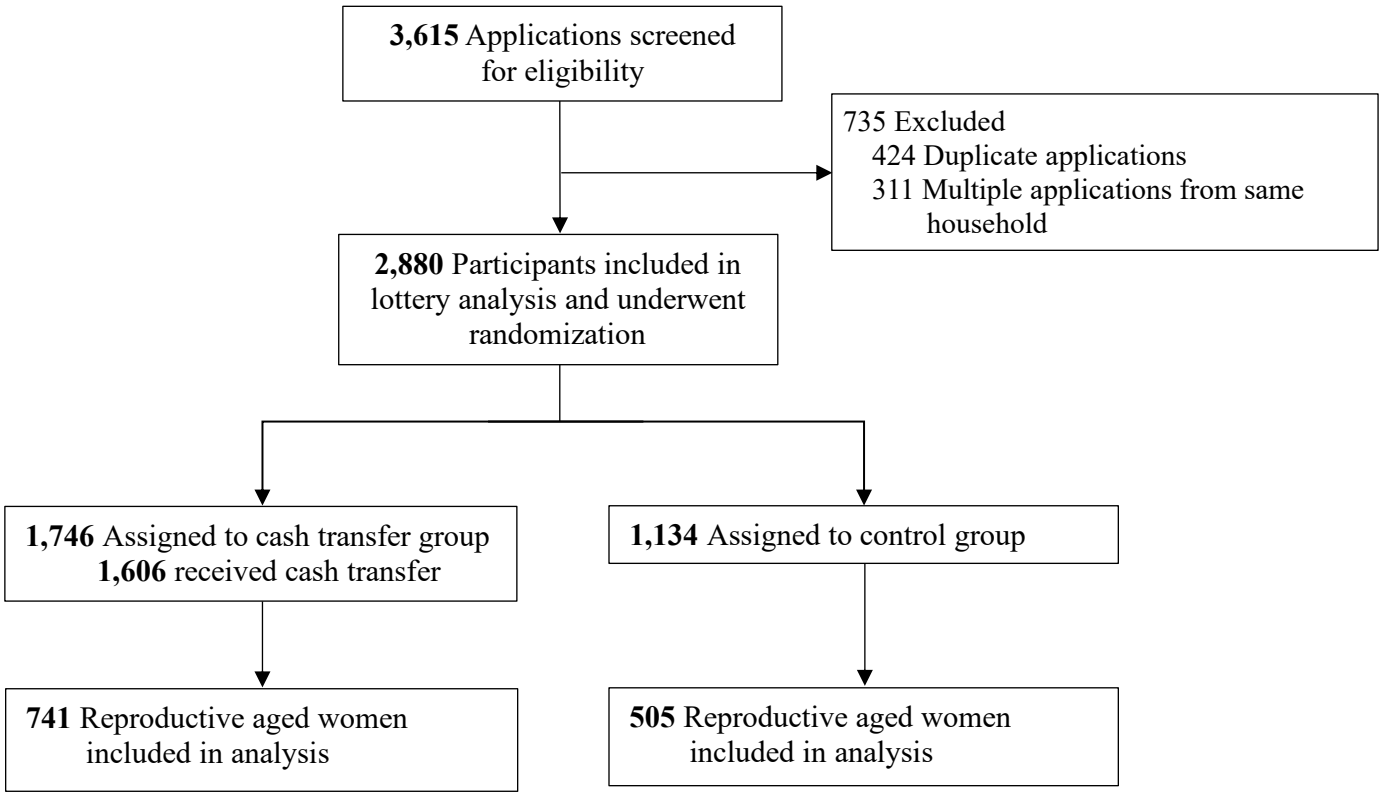
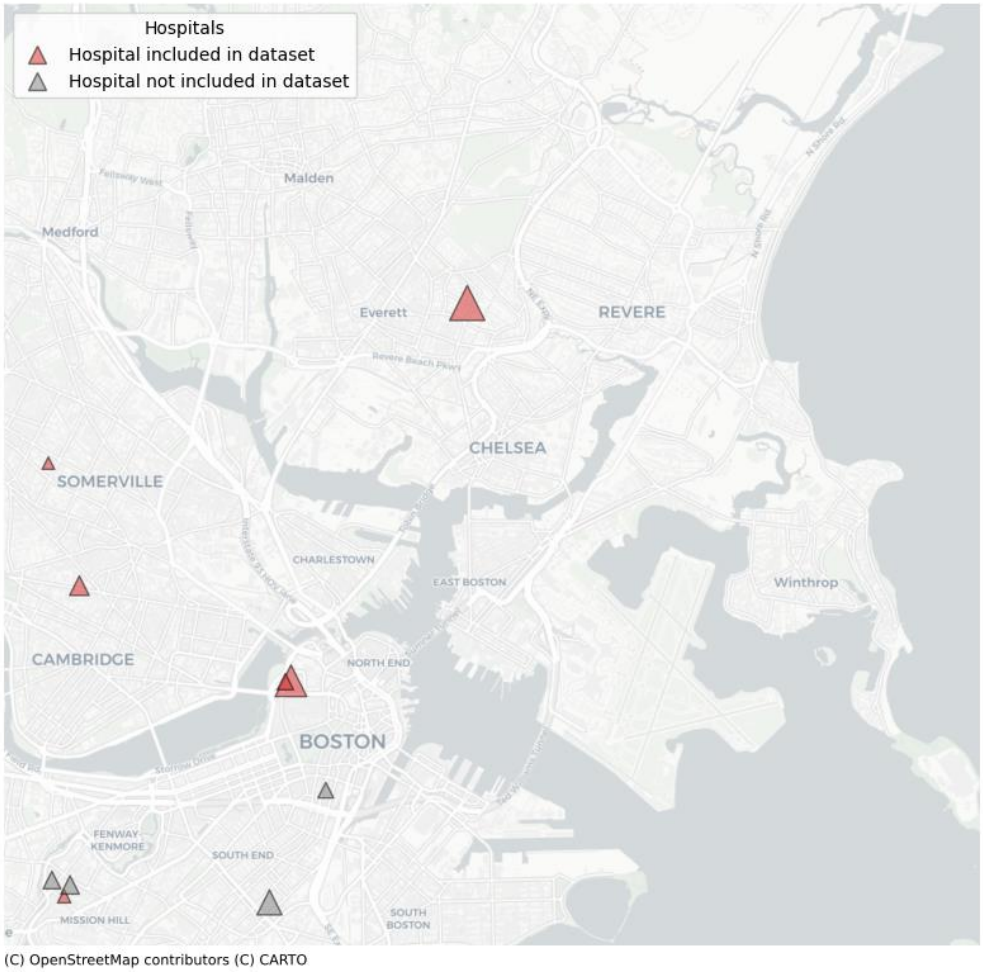


Figure A3: Locations of Emergency Departments around City of Chelsea



Notes: The triangles designate hospitals and are proportional to the share of acute care visits they provide to residents of Chelsea, Massachusetts. See text for details.

Table A1: Distribution of Lottery Tickets

	Number of Participants (% of Group)	
Number of Tickets	Cash Transfer Group (N = 1,746)	Control Group (N = 1,134)
1 ticket	1 (0.1)	5 (0.4)
2 tickets	36 (2.1)	52 (4.6)
3 tickets	259 (14.8)	308 (27.2)
4 tickets	676 (38.7)	447 (39.4)
5 tickets	509 (29.2)	252 (22.2)
6 tickets	206 (11.8)	64 (5.6)
7 tickets	57 (3.3)	5 (0.4)
8 tickets	2 (0.1)	1 (0.1)

Table A2: Proportion of Applicants Winning the Lottery

Number of Tickets	Proportion of Applicants Winning the Lottery (%)
1 ticket	1/6 (16.7)
2 tickets	36/88 (40.9)
3 tickets	259/567 (45.7)
4 tickets	676/1,123 (60.2)
5 tickets	509/761 (66.9)
6 tickets	206/270 (76.3)
7 tickets	57/62 (91.9)
8 tickets	2/3 (66.7)

Table A3: Linkage to Electronic Health Records

Characteristics	Cash Transfer Group (N = 1,746)	Control Group (N = 1,134)	P-value for Difference
Linked to at least one health system	95.0	95.1	0.84
Linked to Cambridge Health Alliance	54.4	56.9	0.22
Linked to Mass General Brigham	83.2	81.5	0.29
Linked to East Boston Neighborhood Health Center	75.1	76.5	0.43
Linked to at least two health systems	80.6	79.8	0.60
Linked to all three health systems	37.1	40.0	0.16