Neighborhood Networks and Program Participation

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Abstract

We explore the existence of social interactions in program participation within small neighborhood networks. Our population of interest is pregnant women and their participation in Medicaid during pregnancy. Using geographically detailed data, we show that a pregnant mother is substantially more likely to participate in Medicaid, if recently pregnant mothers on her exact census block also received Medicaid benefits. To deal with endogenous sorting into neighborhoods, we only compare mothers across small neighborhoods within a broader geographic area defined as an agglomeration of nearby census blocks. The reflection problem is avoided by restricting peer groups to only mothers who have recently given birth. We also document substantial heterogeneity in the estimated network effect across various dimensions. Furthermore, increased Medicaid participation seems to translate into healthier behavior among pregnant women with earlier and more intensive participation in prenatal care.

Keywords: Program participation; Peer Effects; Networks; Medicaid

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I Introduction

Participation rates in welfare programs remain well below eligibility rates. Lack of information about program participation rules, among other frictions, impedes full participation from the pool of potential recipients (Aizer (2007). These concerns become even more signficant when participation is deemed to significantly improve outcomes of interest (e.g. Currie and Gruber (1996); Joyce et al. (2008)).

In this paper, we study the effect of informal networks on program participation at a very fine neighborhood level. We focus on the case of Medicaid participation among pregnant mothers in California. Our main research question explores whether a pregnant mother is more likely to participate in a welfare program if recent mothers in their immediate neighborhood received benefits from these programs. Previous work has established that a lack of knowledge about welfare programs can impede participation (Heckman and Smith, 2004; Stuber et al., 2000). Therefore, potential information flows within neighborhood networks among pregnant mothers may point to the existence of spillovers in program take-up.

Recently, Dahl et al. (2014) presented evidence of significant peer effects in paternity leave participation in Norway using a regression discontinuity framework. Most previous literature, on the other hand, has largely focused on peer formation based on ethno-linguistic ties and similarly finds that peer effects positively impact program participation (Figlio et al., 2015; Aizer and Currie, 2004; Bertrand et al., 2000). However, due to standard concerns regarding endogenous group and network formation, causality has been hard to establish. As Blume et al. (2010) and Durlauf (2004) discuss, adequately dealing with concerns of individuals sorting to particular neighborhood or networks is the biggest impediment in estimating peer effects in a causal framework.[1] Using data at a very fine geographic level and the identification ideas established in Bayer et al. (2008), this paper presents plausibly causal estimates of network effects in program participation.

[1]For a recent review of econometric issues related to the identification and estimation of network effects see De Paula (2016)

Our identification strategy explores how likely a mother is to take up Medicaid if a recent mother who resides on her *census block* also took up Medicaid during her pregnancy, conditional on census block group fixed effects. We refer to block groups as the reference group of a given individual in the analysis.[2] The key identification idea exploited by Bayer et al. (2008) to estimate causal network effects is the thinness of housing markets at the census block level as opposed to larger geographic entities like census tracts or city neighborhoods. In particular, this implies that even in the presence of endogenous sorting, individuals would not be able to make an exact locational choice down to a specific census block. Therefore, conditional on census block group fixed effects, defining informal networks as other pregnant mothers who reside on your specific census block likely provides as good as random sorting into peer groups. Moreover, this assumption is testable given that any existing correlations in observable individual characteristics at the census block level should disappear once we condition on the reference group fixed effect. We present evidence in section III.C that this is indeed the case.

A second key concern in the estimation of peer effects is the so called reflection problem (Manski, 1993), which reference group fixed effects would not resolve. We avoid this problem by defining the peer group of a currently pregnant mother as all mothers residing on the same census block who gave birth 6 months or 1 year prior to the conception of our currently pregnant mother. While it is possible that network effects can operate between contemporaneously pregnant mothers, it is more likely that information about program participation flows from mothers who have already given birth to mothers who are still currently pregnant. Nevertheless, if this is not the sole mechanism our estimates will then present a lower bound on the existence of peer effects in program participation.[3]

Our results indicate the presence of significant peer effects at the neighborhood level in

[2] A census block group is an aggregation of individual census blocks and represent larger neighborhoods. On average, our sample contains 11 Census blocks per block group.

[3] A related concern is that mothers could be coming in and out of Medicaid enrollment across births, which can potentially end up creating an intertemporal reflection problem. Therefore, as a robustness check we restrict our estimation sample to only those currently pregnant mothers who are having their first child. Results from this specification are remarkably similar to our baseline estimates.

Medicaid participation. Following Bayer et al. (2008) we construct our estimation sample such that each distinct observation is a matched pair of a currently pregnant mother in a given reference group and a recent mother who has successfully given birth in the same reference group. The relevant comparison of interest then becomes between mothers that reside on the same census block as opposed to those that reside in the same census block group but not on the exact same census block. Results show that a currently pregnant mother is around 3 percentage points (pp) more likely to be enrolled in Medicaid both for prenatal care and delivery if a matched mother on her census block also participated in these programs during her pregnancy. This is equivalent to approximately an 8 percent increase relative to her reference group specific average participation in Medicaid. We implement an individual level analogue of the above analysis as well to ease interpretation of the estimated social interaction effect. A currently pregnant woman is between 0.6 pp and 1.4 pp more likely to participate in Medicaid given a 10 pp increase in Medicaid participation among recently pregnant mothers on her exact census block.

We extend our analysis by studying heterogeneity in the estimated network effects based on the general observations of assortative peer group formation, i.e. whether similar individuals are more likely to interact with each other even within a census block. This is consistent with work by Figlio et al. (2015) and Aizer and Currie (2004) documenting positive effects into program participation if individuals are likely to belong to dense networks based on ethno-linguistic affiliation. However, because our estimation strategy plausibly accounts for neighborhood sorting we argue that we uncover causal estimates along these dimensions as well. Our results for this subset of our analysis align with conventional wisdom and a priori expectations. For instance, currently pregnant mothers are more likely to participate in Medicaid if their matched pair belongs to the same ethnic or racial group. We also document higher take up among foreign born Hispanic mothers if they are matched with a previously pregnant participating native Hispanic mother, implying potential information flows from the Hispanic mothers born in the US to those born outside the US.

Finally, we explore whether increased Medicaid participation induces currently pregnant mothers to partake in healthier behavior. We estimate that pregnant women are around 4 percentage points more likely to initiate prenatal care paid for by Medicaid in the first trimester if they observe previously pregnant women on their census block doing the same. Similarly, we find a positive impact for the intensity of prenatal care, with mothers being more likely to go for repeat visits if they observe other Medicaid participants in their census blocks practice similar behavior.

The literature on estimating peer and network effects is large and expansive. Researchers have explored their existence in such diverse settings as academic achievement in the classroom (e.g. Zimmerman 2003) to criminal behavior among juvenile offenders (Bayer et al., 2009). Identification strategies similar to ours have largely been applied in a labor market setting exploring hiring decisions by employers and job referrals among potential employees (see e.g. Bayer et al., 2008, Kramarz and Skans, 2014, Schmutte, 2015). As cited above, researchers have explored peer effects in program participation as well, and have largely found positive effects on take up. Bauer and Dang (2016) use an instrumental variable strategy to assess neighborhood effects at the zip-code level on German data, finding that neighbor welfare use leads to increased own welfare receipt. Using quasi-experimental variation, Åslund and Fredriksson (2009) show the existence of peer effects in welfare take up among a refugee sample in Sweden, while Markussen and Røed (2015) find that social insurance claims are contagious with effects being particularly strong among geographically and relationally closer peers.

Examining the Earned Income Tax Credit (EITC), Chetty et al. (2013) documented the existence of substantial differences in local knowledge of welfare program rules. Their results suggest a role for information dissemination in low knowledge neighborhoods, with the objective of increasing program participation among the eligible population. In section V.A, as a robustness check we repeat our analysis by within sample deciles of the Chetty measure of neighborhood knowledge of EITC. We use this as a proxy for overall neighborhood level knowledge of welfare programs, including Medicaid, our program of interest. Results show that areas with the lowest rates of welfare knowledge actually have the highest network effect rates relative to the baseline rate of participation, providing suggestive evidence that information dissemination policy interventions can lead to an increase in program participation.

The remainder of the paper is organized as follows. Section II presents background information on the Medicaid program, describes our dataset and provides descriptive statistics of our analysis sample. Section III then presents our empirical methodology and discusses in detail alternative specifications we estimate. Section IV presents our main results on the existence of peer effects in program participation at a very fine neighborhood level, a census block. Section V provides suggestive evidence of the role of information dissemination on Medicaid participation and on encouraging healthier behavior on the part of the mother during pregnancy. Section VI concludes.

II Background and Data

A Institutional Details on Medicaid

Medicaid is a social health insurance program designed to provide coverage for low income individuals, especially pregnant women and children. Over the past 30 years, the federal government has greatly expanded the Medicaid program through numerous mandates of coverage. Originally, states were required to cover individuals receiving Aid to Families with Dependent Children (AFDC). However, since the mid 1980s, federal mandates and state expansions have resulted in the decoupling of AFDC and Medicaid eligibility. Under federal mandates, all states are required to cover pregnant women and children up to age 19 in families earning less than 185 percent of the federal poverty level (FPL). Additionally, many states have expanded their individual programs to include more marginally low income individuals. For example, pregnant women in California are eligible to receive Medicaid coverage if their household income is up to 300 percent of the FPL. This 300 percent FPL threshold did not change over our study period.

Recent work suggests that expanded Medicaid coverage has substantial health and human capital benefits, especially over the long term (Cohodes et al., 2016; Wherry and Meyer, 2016; Brown et al., 2015). However, another strand of literature examines the potential crowd out effect of Medicaid on private insurance (Gruber and Simon, 2008). The health benefits of increased Medicaid participation when the alternative is private insurance is ambiguous at best, and is ultimately an empirical question (Dubay and Kenney (1997)). For this reason, we focus on Medicaid take-up in this paper and provide some suggestive evidence of the effect of increased participation on prenatal care use.

B Vital Statistics Data: California, 2002 - 2013

We use restricted access Vital Statistics Natality data containing the universe of births in California between 2002 and 2013. The data is compiled from information provided on birth certificates at the time of birth of the infant. Vital statistics data provide rich parental demographic and socioeconomic information including race and ethnicity, age, and educational attainment. They also contain detailed information about gestational age at the time of birth and prenatal care, including whether Medicaid paid for the birth and whether Medicaid paid for prenatal care visits. Two features of the restricted access version of the data are especially crucial for our analysis. First, we observe a mother's exact street address which we geocode to the census block level, our definition of a small neighborhood within which we assume network effects to operate. Second, we also observe the exact date of birth of the infant for each observation. We use this variable to calculate the date of conception of a *currently* pregnant mother using a measure of gestation. This in turn helps us define the set of all recently pregnant mothers that reside in the census block group, the broader neighborhood of our currently pregnant mother. This provides us with the crucial ingredient for causal estimation of network effects, described in detail in section III.

Part of our analysis focuses on a matched pair of a currently pregnant mother and a recently pregnant mother residing in the same census block group. Because of this, we restrict our sample to the five densest counties in California to facilitate this matching. To keep the analysis consistent, our individual level specifications also work with the same sample restrictions. We restrict to the following counties, in descending order of population density: San Francisco, Orange, Los Angeles, Alameda, and San Mateo. Overall, the five counties have 108,685 census blocks and contain 10,116 census block groups, or an average of approximately 11 blocks per block group. [4]

Table 1 presents descriptive statistics for the individual level sample. Medicaid paid for the delivery costs of approximately 59 percent of the births in California, which is substantially higher compared to the national average of 48 percent in 2010 (Markus et al., 2013; Curtin et al., 2013). This is partly explained by the generous Medicaid eligibility cutoffs in California and the increased representation of Hispanic mothers in California who historically have had higher program participation rates.[5] As the second column shows, 75 percent of all Hispanic mothers receive Medicaid benefits, compared to 56 percent among blacks and only 24 percent and 21 percent among Asians and whites, respectively. Our data also separately identifies whether the mother received prenatal care paid for by Medicaid, whereas much of the previous literature observes only Medicaid participation at the time of birth. Aizer and Currie (2004), who also use California birth data, are a notable exception. We see similar patterns of participation for prenatal care paid for by Medicaid. Interestingly, a large fraction of mothers receive prenatal care in the first trimester with the proportion being fairly consistent across racial groups.

The racial composition of California provides an interesting setting, especially in estimating heterogeneous network effects based on race. As seen in Table 1, 63 percent of all births in our sample are to Hispanic mothers, while 14 percent each are to Asians and whites, and

[4] Alameda County contains 11,484, San Francisco contains 4788, Orange County has 18,526, Los Angeles County contains 67,920, and San Mateo County comprises 5967 census blocks. Overall these counties cover around 2.5 million off the 3.9 million births (64 percent) in California during our sample period.

[5] Because we focus on the five densest counties of California, our sample is also more urban than the US as a whole.

Table 1: Descriptive Statistics - Baseline Sample

	Full	By Race/Ethnicity			
		Hispanic	White	Black	Asian
Medicaid at Birth	0.585	0.751	0.210	0.561	0.237
	(0.493)	(0.433)	(0.407)	(0.496)	(0.425)
Prenatal Medicaid	0.581	0.747	0.209	0.557	0.233
	(0.493)	(0.435)	(0.406)	(0.497)	(0.423)
Prenatal First Trimester	0.868	0.863	0.910	0.809	0.880
	(0.338)	(0.344)	(0.286)	(0.393)	(0.325)
Medicaid Prenatal First Trimester	0.493	0.641	0.168	0.430	0.189
	(0.500)	(0.480)	(0.374)	(0.495)	(0.392)
Less than High School	0.365	0.521	0.0599	0.182	0.0538
	(0.482)	(0.500)	(0.237)	(0.386)	(0.226)
High School Graduate	0.247	0.277	0.181	0.348	0.145
	(0.432)	(0.448)	(0.385)	(0.476)	(0.352)
Some College	0.183	0.146	0.251	0.327	0.214
	(0.387)	(0.353)	(0.434)	(0.469)	(0.410)
College Graduate	0.198	0.0511	0.504	0.137	0.583
	(0.398)	(0.220)	(0.500)	(0.343)	(0.493)
Age 15-24	0.318	0.394	0.163	0.408	0.0939
	(0.466)	(0.489)	(0.369)	(0.491)	(0.292)
Age 25-34	0.517	0.477	0.586	0.464	0.652
	(0.500)	(0.499)	(0.493)	(0.499)	(0.476)
Age 35-44	0.165	0.129	0.251	0.128	0.254
	(0.371)	(0.335)	(0.434)	(0.334)	(0.435)
Male Baby	0.512	0.511	0.513	0.510	0.520
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Single Birth	0.974	0.980	0.957	0.960	0.972
	(0.160)	(0.141)	(0.204)	(0.196)	(0.166)
Mother Born Outside of US	0.595	0.669	0.239	0.104	0.860
	(0.491)	(0.471)	(0.426)	(0.305)	(0.347)
Mother Born in Mexico	0.337	0.523	0.003	0.000	0.001
	(0.473)	(0.499)	(0.054)	(0.021)	(0.024)
Observations	938,760	599,811	131,823	62,824	133,986

Note: The unit of observation is all women conceiving in the five densest California counties between 2002 and 2013. The counties include Los Angeles, San Francisco, Alameda, San Mateo, and Orange. The table reports means with standard deviations in parentheses.

7 percent to blacks. Moreover, there is variation in this racial makeup across counties in California, for instance, 32 percent of all birth in Alameda are to Asian mothers, which is substantially higher than the national average. Overall, this variation allows us to conduct pairwise analysis based on characteristics of both mothers in a matched pair.

Similarly, focusing on the educational attainment of mothers, around 37 percent of births in our sample occur to high school dropouts and close to 25 percent to high school graduates. There is substantial variation in these numbers across race with Hispanic mothers in particular more likely to be high school dropouts, which may partially explain their increased participation in Medicaid. We refine our baseline analysis in section IV by using information on mother's education to account for Medicaid eligibility concerns given that all standard data sets, including our own, lack measurements of household income.

III Empirical Methodology

In this section we present our empirical methodology and the key identification ideas used to causally estimate neighborhood level peer effects in Medicaid participation among pregnant mothers. Our formulation is primarily based on the identification strategy developed in Bayer et al. (2008). We also discuss alternative specifications that we estimate to investigate the existence of heterogeneous effects across individual characteristics and a specification in which we exploit the fraction of mothers on a woman's census block who use Medicaid at birth as our key variable of interest. Finally, in section III.C, we present empirical evidence for the suitability of our approach by testing for the existence of sorting on key observables, given the preferred specification developed in this section.

A Baseline Specifications

A.1 Matched Pair Analysis

Following Bayer et al. (2008), each distinct observation in our sample is a matched pair of a currently pregnant mother i in census block group g and time period t and a recently pregnant mother j in the same census block group but who gave birth in time period $t-\tau$. We define τ as 6 months or 1 year prior to the conception of mother i.[6] However, to keep our notation sparse we do not index our variables by τ in the specifications presented below. To investigate the existence of neighborhood level effects in program participation among pregnant mothers, we first estimate the following baseline specification:

$$P^b_{ijgt} = \gamma^M R^b_{ijgt} + \lambda_g + \lambda_t + \varepsilon_{ijgt} \tag{1}$$

where P^b_{ijgt} is a binary variable equal to one if both mother i and j participated in a given program, such as Medicaid participation or initiation of prenatal care, and R^b_{ijgt} denotes a binary variable that is equal to one if i and j reside in the same *census block, b,* and zero otherwise. In this sense, each mother i is matched to a given mother j belonging to the set of recently pregnant mothers living in her census block group, $\mathbb{J}_{g\tau}$. The key identification element is the inclusion of λ_g, a block group fixed effect. Our key parameter of interest is then γ^M, which identifies the existence of any social interactions in program participation at the neighborhood level among pregnant mothers using the matched pair sample. Finally, ε_{ijgt}, is a matched pair specific idiosyncratic shock, which corrects for correlated errors within a census block group.

The key identification idea for causal estimates of neighborhood effect in such a setting

[6]For ease of presentation, we present time as year t, but all dates and reference time periods are specific to the imputed date of conception. For example, a woman who conceived on March 1, 2003 will be matched to all mothers living in the reference neighborhood who gave birth between September 1, 2003 and February 28, 2003. We run alternative specifications by including all mothers who gave birth 1 or 2 years prior to the conception of mother i. The results are extremely similar to the baseline specification presented here.

is the thinness of the housing market at the census block level (Bayer et al. 2008). At the time of making a residential location choice, even if individuals want to sort based on characteristics of their potential neighbors they might not be able to choose an exact house on a specific census block. In other words, individuals might be able to choose a broader neighborhood like a census block group or a census tract in which to locate, but are less likely to be able to choose the exact census block. Therefore, due to this structure of the housing market, once we condition on a census block group fixed effect, λ_g, the variation in our peer group at the census block level is likely to be as good as random. Hence, the spillover parameter, γ^M, is identified by mother i being influenced by a mother j on her own exact census block as opposed to a mother k who resides in her broader neighborhood but not on the exact same census block. The underlying assumption here is that the formation of peer groups occurs at the immediate neighborhood level and that its formation is not entirely within the individual's control. Moreover, this assumption is fundamentally testable by comparing correlations in individual characteristics at the block level with and without the census block group fixed effect. We present results from such an exercise in section III.C to validate our identification strategy.

A key concern that plagues most empirical investigations of peer effects is the so called reflection problem (Manski, 1993), which can lead to simultaneity related concerns. The literature on job referrals that has previously utilized this strategy is especially likely to be beset with such concerns, given that individuals might make a workplace location decision first and then receive a referral to a residential neighborhood in which their work colleague resides. Our setup does not suffer from such concerns for two reasons. First, matching each currently pregnant mother, i, with a mother, j, who gave birth in the recent past allows us to sidestep the reflection problem since the former are less likely to belong to the 'peer group' of the latter.[7] Information pertaining to the existence of welfare programs that

[7] However, one can expect a scenario where some form of a dynamic reflection problem might operate. A recently pregnant mother (j) can have a currently pregnant mother (i) as part of her peer group, if mother i had a previous pregnancy already paid by Medicaid. Under this scenario, our estimates can be biased upwards. We, therefore, estimate specifications by restricting mother i only to women who are having their

may help pregnant mothers is more likely to flow from mothers who have already given birth successfully to mothers who are still currently pregnant. However, we may still expect the existence of peer effects within contemporaneously pregnant mothers in which case our benchmark estimates would only provide a lower bound for potential peer effects in program participation. Second, in our framework, because we match mothers to women who gave birth before the matched woman conceived, it is extremely unlikely that mothers enroll in Medicaid first and then make their residential choice based on a referral from another Medicaid participant they met at a clinic or health facility. For these reasons, we have confidence that these issues do not bias our results.

Finally, our specification is less likely to suffer from biases owing to correlated effects based channels given the extremely fine geographic level of our analysis. In particular, common unobservable shocks would have to operate at the census block and not the census block group level to diminish our findings. For instance, if a health center services only a particular census block then one can expect that both currently and recently pregnant mothers are more likely to take up Medicaid in that block even in the absence of spillovers. However, health or policy interventions likely operate at a much broader level, and therefore our census block group fixed effects should account for any biases that might be introduced by differential access to local health services.

Appendix figures A.1 and A.2 plot histograms of the number of matches we observe at the census block level in our data. Because births are a relatively rare phenomenon, compared to, for instance, having a job, we observe a high fraction of blocks with zero or one matches. Our baselines sample restricts to blocks in which we were able to observe at least three matches in the previous 6 months on a currently pregnant mother's census block. Using this sample, we include approximately 45 percent of births that occur during our sample time frame. Additionally as a robustness exercise, we include census blocks with any matches at all, as well as those with four, or five matches, and results are remarkably consistent across

first child. The estimates from these regressions are similar to the baseline results presented in the main text, and are presented in appendix table A.1.

specifications.[8]

A.2 Individual Level Analysis

Because controlling for block group fixed effects provides an adequate way of dealing with endogenous sorting into neighborhoods, we also estimate a more conventional econometric specification using data at the individual mother level rather than for matched pairs. This gives us the following equation:

$$P^b_{igt} = \beta_0 + \gamma^I FP^b_{gt} + X'_{igt}\beta_1 + \lambda_g + \lambda_t + \varepsilon_{igt} \tag{2}$$

where P^b_{igt} equals 1 if mother i in a given census block is on Medicaid and FP^b_{gt} represents the fraction of previously pregnant mothers in i's *census block* that utilize Medicaid given by $\frac{\sum_{j \epsilon \mathbb{J}_{b\tau}} P^b_j}{J}$.[9] Similarly, X_{igt} represents observable demographic and pregnancy related characteristics of mother i, λ_g represents the census block group level fixed effect, and ε_{igt} now represents an individual level error term, clustered at the census block group level to allow for serial correlation. The identification argument follows analogously from above: conditional on including a block group fixed effect the distribution of mothers across individual census blocks should not be plagued with endogenous sorting concerns. Therefore, if network effects operate at the very local neighborhood level, then the variation in the fraction of mothers on Medicaid at the census block level should identify our spillover parameter of interest ,γ^I, for our individual level sample.

This specification is similar to the one used by Aizer and Currie (2004), however by conducting our analysis at such a granular level controlling for block group fixed effect, we argue that we estimate a causal effect of social interactions in program participation. One advantage of equation (2) over the matched pair specification from section III.A.1 is its ease

[8]Results are available upon request.

[9]We again restrict previously pregnant mothers to those women who have given birth in the 6 months or 1 year prior to mother i's date of conception.

of interpretation in conventional terms. Our parameter of interest, γ^I, can be interpreted as a percentage point increase for mother i participating in Medicaid while pregnant in response to a change in the fraction of mothers on her census block that participate in Medicaid during their pregnancy. In section IV, we present results from the estimation of both equation (1) and (2) where appropriate.

B Refinements to the Matched Pair Analysis

B.1 Incorporating Individual Fixed Effects

Given that in the matched pair analysis each currently pregnant mother i in census block group g is matched to multiple recently pregnant mothers j in unit g, we can implement an individual level fixed effect strategy as well. Such a strategy would be particularly helpful if, for instance, we expect unobservable characteristics of a currently pregnant mother i to be related to both her decision to participate in welfare programs and to reside on a given census block based on some characteristics that are unobservable to the econometrician. It is crucial to note that while our baseline specification is likely to adequately deal with potential residential sorting at the neighborhood level, a panel structure would allow us to control for any individual level unobservables that could potentially bias our findings. We therefore estimate the following model motivated by the above discussion:

$$P^b_{ijgt} = \alpha_i + \gamma^M R^b_{ijgt} + \lambda_t + \varepsilon_{ijgt} \tag{3}$$

where all elements are the same as equation (1) with the exception of α_i that represents individual level fixed effect for mother i. Note that λ_g, the census group level fixed effect from equation (1), will be subsumed by α_i in equation (3). Additionally, each currently pregnant mother i can have a unique 'peer group', given she is matched to previously pregnant mothers j on a rolling basis based on the conception date of mother i. As mentioned earlier, we impute the conception date based on the exact date of birth for the infant and the clinical estimate of gestation. Essentially, only mothers who reside in the same census block group and give birth on the exact same date will necessarily have the exact same set of matches of mothers j.

B.2 Heterogeneity Across Individual Characteristics

A key empirical finding in much of the empirical literature on social interactions is the tendency of assortative matching, or the decision to interact with individuals similar to oneself, in network formation. In fact this particular aspect of the problem is so well established that previous papers have used it as a defacto way of defining peer groups within neighborhoods (e.g. Figlio et al., 2015; Aizer and Currie, 2004). However, because individuals are in essence sorting based on observable characteristics, it becomes hard to isolate the causal effect of social interactions from the effect of similar individuals sorting to the same neighborhood.[10] Our framework, on the other hand, provides us with a setting to causally explore such social interactions. We augment equation (1) in the following way to study heterogeneous treatment effects across individual characteristics:

$$P^b_{ijgt} = \gamma_0^M R^b_{ijgt} + \gamma_1^{'M} R^b_{ijgt} * X_{ijgt} + \alpha^{'} X_{ijgt} + \lambda_g + \lambda_t + \varepsilon_{ijgt} \tag{4}$$

where all variables are defined as before along with introducing a vector of individual characteristics of each matched pair. We explore heterogeneity across a number of predetermined demographic dimensions. For instance for race, X_{ijgt} would contain variables that map out relationships within each matched pair.[11] The key coefficient vector of interest in

[10] Aizer and Currie (2004) are particularly cognizant of this concern and hence differentiate between a neighborhood effect and an actual network effect while presenting their results.

[11] All possible combinations include white mother matched to white mother, white mother to black mother, white mother to Hispanic mother, white mother to Asian mother, black mother to black mother, black mother to white mother, black mother to Hispanic mother, black mother to Asian mother, Hispanic mother to Hispanic mother, Hispanic mother to white mother, Hispanic mother to black mother, Hispanic mother to Asian mother, Asian mother to Asian mother, Asian mother to white mother, Asian mother to black

equation (3) is $\gamma_1'^M$, which measures the interaction effect of residing on the same census block with the relevant mapping of individual characteristics of the pair. Results from this specification are presented in section IV.B.

C Testing for Endogenous Sorting at the Census Block Level

Table 2 explores the suitability of the above outlined identification strategy by analyzing neighborhood level sorting based on observable characteristics.[12] Each row in Table 2 reports a given observable characteristic of a randomly selected individual from each census block and the likelihood of observing a similar individual on that census block based on the dimension under consideration. Column 1 reports unconditional correlations while Column 2 controls for the census block group fixed effect, our preferred specification for handling endogenous sorting.[13]

Without controlling for census block group fixed effects, we find strong unconditional correlations in individual characteristics at the block level for both comparison groups. A mother aged between 25 and 34 years of age is 23 percent more likely to be exposed to a similarly aged mother on her census block. Similarly, we see strong correlations in education variables, with high school dropouts being 38 percent more likely to be exposed to other high school dropouts on their census blocks, and college graduates being 53 percent more likely to live on the same block. We also observe strong unconditional sorting on racial variables as well. However, once we condition on the census block group fixed effect, these correlations decrease to nearly zero. Likelihood of observing another middle aged mother falls from 0.229 to 0.003, for high school dropouts the correlation decreases from 0.381 to 0.007, and for college graduates from 0.532 to 0.006. Similar decreases are seen for educational

mother, and Asian mother to Hispanic mother.

[12]These tests were motivated and outlined in Bayer et al. (2008).

[13]In column 1 and 3 of table 2, we first separately regress individual characteristics of a randomly chosen mother in a given census block, and characteristics of other mothers in the same block on year fixed effects. We then calculate the residuals from both these regressions and report the correlation between these residuals. In column 2 and 4, we further add a census block group fixed effect, which is the defining feature of our empirical strategy, and then report the correlation between the residuals.

Table 2: Testing for Endogenous Sorting at the Census Block Level

	6 Month Comparison		1-year Comparison	
Mother Characteristics	Unconditional	Census Block Group F.E	Unconditional	Census Block Group F.E
Age 15 - 24 years	0.229	0.003	0.264	−0.001
Age 25 - 34 years	0.060	−0.006	0.084	−0.004
Age 35 - 44 years	0.157	0.008	0.194	−0.007
Less than High School	0.381	0.007	0.409	0.011
Some College	0.094	0.001	0.124	0.012
College Graduate	0.532	0.006	0.561	0.010
Hispanic	0.584	0.005	0.607	0.015
White	0.521	0.005	0.550	0.013
Black	0.357	0.001	0.390	0.009
Asian	0.430	0.004	0.464	0.011
Father Characteristics				
High School Dropout	0.393	0.008	0.423	0.010
Some College	0.099	0.008	0.131	−0.006
College Graduate	0.545	0.002	0.575	0.009
Hispanic	0.584	0.005	0.611	0.015

The unit of observation is the individual. We randomly choose one individual from each census block and calculate correlations between her characteristics and the characteristics of other mothers residing on her census block. In column 1 and 3 we separately regress individual characteristics of a randomly chosen mother in a given census block, and characteristics of other mothers in the same block on year fixed effects. The reported correlations are between the residuals from these two regressions. In column 2 and 4, we further add a census block group fixed effect, and then report the same correlation between the residuals.

attainment of the father. Among racial variables the correlation for whites and Hispanics falls from over 0.5 to 0.005. Furthermore, we see similar drops for the one-year comparison group after conditioning on the census block group fixed effect. While the correlations are slightly larger in column 4, they are still close to zero. This analysis provides support that, once we condition on census block group, endogenous sorting based on observable variables at the census block level is not a concern. In other words, individuals do not seem to have perfect control of sorting to an exact census block even if they would like to, giving us plausibly random variation in peer group formation at a very fine local level.[14]

Because the above analysis explores potential endogenous sorting only on observable characteristics of neighbors, it is possible that individuals sort to actual census blocks based on characteristics unobservable to the econometrician. A number of factors mitigate this concern. First, prospective movers into a neighborhood can observe only a limited number of characteristics of their potential neighbors, which are largely limited to basic demographic information. We observe these variables in the data as well and hence can test for sorting based on them. Second, given the framework established by Altonji et al. (2005), one can reasonably extrapolate a lack of sorting based on observables presented in Table 2 as indirect evidence of minimal sorting on unobservables as well.

IV Results

We now present results from the implementation of the above outlined empirical methodology to investigate the existence of network effects in program participation at the neighborhood level. Section IV.A first presents our baseline results estimating equation (1) and (2), as well as a placebo exercise to mitigate concerns about our identification strategy. We then investigate how eligibility concerns regarding Medicaid might affect our findings. Section IV.B explores potential heterogeneity in effects by observable characteristics of the matched

[14]For this test to fail, we would need evidence that within a block group sorting continued, or essentially that individuals would be more likely to live in a particular section of a block group based on observable characteristics.

pair providing evidence of the potential mechanisms that may explain our findings.

A Baseline Results

Table 3 presents results from our baseline specifications from Section III. Panel A presents estimates from the matched pair analysis where the first column provides estimate of the network effect parameter, γ^M, from equation (1) and where the set of recently pregnant mothers, $\mathbb{J}_{g\tau}$, is restricted to births in census block group g, 6 months (τ) prior to the date of conception of mother i. In addition we also restrict to census blocks in which we identify at least 3 matches for each currently pregnant mother. Estimates show that a currently pregnant woman, i, is 3.1 percentage points more likely to be on Medicaid if a recently pregnant woman (j), on her exact census block, was also on Medicaid during her pregnancy.[15] This is equivalent to an 8 percent increase in participation of mother i, relative to the overall level of prevalence of Medicaid on her census block group g. This implies the existence of substantial network effects at the very local neighborhood level in program participation among pregnant women. The second column of Table 3 then presents results from incorporating individual fixed effects for mother i as outlined in equation (3) above. The estimated spillover parameter is almost identical to our baseline specification giving us confidence that unobservable mother level heterogeneity is not driving our results.[16]

The last two columns then expand the set of previously pregnant mothers to all birth in the year before mother i's conception date. The results are very similar to the first two columns for the matched pair analysis. However, our preferred specification restricts only to births in the 6 months before conception. Given that we do not observe the residential choice history of mothers in our sample, our main specification restricts to a smaller time

[15] Appendix table A.2 presents our analysis based on the sample with at least 5 matches for the currently pregnant mother and the results are extremely robust. We do a similar robustness check by dropping the top 10 percent of the densest census blocks in table A.3, the results are again remarkably similar to our baseline estimates.

[16] Because of concerns that eligible mothers may sign for Medicaid right at the time of delivery, appendix table A.4repeats the above analysis for prenatal care paid by Medicaid. We find exremely similar effect sizes compared to our baseline estimates in table 3. Similarly, appendix table A.1 restricts to mothers having their first births and finds similar estimates although slightly smaller in magnitude.

Table 3: Estimated Network Effect in Medicaid Participation - Baseline Results

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Panel A: Matched Pair Sample				
Network Effect (γ^M)	0.031**	0.028**	0.032**	0.026**
	(0.002)	(0.001)	(0.002)	(0.001)
Intercept	0.377**	0.414**	0.360**	0.400**
	(0.002)	(0.000)	(0.002)	(0.000)
Observations	22,301,554	22,301,554	58,422,048	58,422,048
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓
Panel B: Individual Level Sample				
Fraction Medicaid (γ^I)	0.137**	0.058**	0.202**	0.087**
	(0.005)	(0.003)	(0.004)	(0.002)
Observations	938,760	834,078	1,408,673	1,254,616
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

window to minimize concerns regarding moving in and out of the neighborhood.[17]

Panel B of Table 3 presents results for network effects estimating equation (2) based on the individual level sample. These estimates are more readily interpretable but use an analogous identification idea as for the matched pair analysis in Panel A. The estimated effect, γ^I, shows that a currently pregnant mother is 1.4 percentage point (pp) more likely to participate in Medicaid given a 10 pp increase in the fraction of recently pregnant women on her *census block*. Column (2) then controls for a number of individual characteristics of mother i that are likely to affect participation. These variables include basic demographic measures including race and education of both parents, mother's age, and additional pregnancy characteristics.[18] After including individual characteristics, the peer effect coefficient decreases to 0.6 pp given a 10 pp increase in the fraction participating in Medicaid on a given census block. When we expand our sample to all matches in the past year, our estimated effect, in column (4), increases slightly to 0.9 pp given a 10 pp change in participation at the block level.

Our identification strategy here is again based on including a fixed effect for the census block group on which the mother resides, allowing the distribution of Medicaid participation across census blocks within a census block group to be essentially random based on observables.[19] However, the existence of heterogeneous effects by characteristics of just mother i, and in the case of the matched analysis both mother i and j, can lead to estimation of different network effects conditional on various observable characteristics, contributing to the drop in the estimated effect in panel B. The rest of this section explores concerns regarding eligibility for Medicaid and section IV.B further explores these heterogeneity concerns along a number of key dimensions.

One concern that may hamper inference is that Medicaid eligibility (and thus participation) is based on household income thresholds. For California over our study period,

[17]Our data provide a mother's residence at the time of birth. We assume this reflects her residence during pregnancy and that mothers who recently gave birth are not likely to move, allowing for interactions in which these mothers disseminate information to currently pregnant women. If this assumption does not hold, our estimates would be biased towards zero.

[18]See Table 1 for a complete list of covariates.

[19]Refer to table 2 in section III.C and the relevant discussion.

pregnant women whose household income was less than 300 percent of the FPL were eligible for Medicaid. Unfortunately, we do not observe household income as part of our data. Previous literature analyzing Medicaid participation as well as treatment effects has also suffered from similar concerns. The main worry for this analysis is that a currently pregnant mother who observes a recent mother with Medicaid and decides to participate in the program might not in fact be eligible to do so. This would result in an underestimation of the effect of social interactions on program participation in our baseline specification. We explore this concern by restricting our sample of currently pregnant mothers by educational attainment, the strongest measure of socioeconomic status in our dataset.

Table 4 presents results from such an analysis. Panel A separately estimates equation 1 by education level given the well-known correlation between education and income and the fact that previous studies have restricted their samples to those without a college degree (Aizer and Currie, 2004). We put no restriction on the match mother j given that we are only concerned with whether a recently pregnant mother in mother i's census block was on Medicaid or not. Among women who are high school dropouts we see a 4.6 pp increase in Medicaid participation if another mother on the given census block was also on Medicaid during her pregnancy. The estimated coefficients is close to 12 percent relative to the Table 3 baseline participation in Medicaid in the reference group of mother i excluding her own census block. The relative effect for high school graduates is around 9 percent. As we move along education classifications across columns in Table 4, our estimated spillover parameter moves in the right direction and decreases in magnitude. We see a much smaller effect (1.8 pp) for mothers who have some college education, around 5 percent relative to the mean. The network effect falls to only 0.3 pp or 0.7 percent relative to the mean, for women who are college graduates given that they are the least likely to be eligible for Medicaid. These results are extremely robust when we incorporate individual fixed effects in the estimation giving us confidence in the suitability of our identification strategy. [20]

[20] We repeat the eligibility analysis for the sample of census blocks with at least 5 matches for a currently pregnant mother in appendix table A.5. The results are again remarkably consistent along this dimension.

Table 4: Restricting Currently Pregnant Mothers by Education

	Less Than High School		High School		Some College		College Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Matched Pair Sample								
Network Effect (γ^M)	0.046**	0.046**	0.033**	0.030**	0.018**	0.015**	0.005**	0.003**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Intercept	0.582**	0.636**	0.404**	0.456**	0.239**	0.276**	0.062**	0.061**
	(0.003)	(0.000)	(0.002)	(0.000)	(0.003)	(0.000)	(0.002)	(0.000)
Observations	8,337,611	8,337,611	5,355,103	5,355,103	3,679,611	3,679,611	4,209,028	4,209,028
Block Group and Year F.E	✓	✓	✓	✓	✓	✓	✓	✓
Individual F.E	×	✓	×	✓	×	✓	×	✓
Panel B: Individual Level Sample								
Fraction Medicaid (γ^I)	0.039**	0.028**	0.094**	0.071**	0.092**	0.057**	0.070**	0.049**
	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Observations	334,053	294,082	232,266	208,424	167,438	154,649	180,809	180,809
Block Group and Year F.E	✓	✓	✓	✓	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Panel B of Table 4 presents our findings from the individual level sample. Although we see a higher estimated effect for high school dropouts compared to the baseline in Table 3, there is less variation across educational categories. The estimated coefficients across panel A and B measure the network effect along different dimensions and hence cannot be directly compared. Moreover, the analysis based on census block level fractions can also be relatively more noisy depending on the number of matches in each block, affecting the variance in the calculated fraction.

As evidenced in Table 2, so far our identification strategy has relied on a lack of neighborhood (census block) level sorting on observables conditional on a broader neighborhood (census block group) fixed effect. Although it is hard to envisage scenarios where individuals sort over unobservables into exact census blocks, one can be worried that common environmental shocks at a geographic level larger than a census block but smaller than a census block group can partially be driving our resutls. In Table 5, we present results from a falsification test designed to explore this potential concern. We randomly assign each mother i to a block different from her actual block of residence but within her block group keeping all mother j's correctly assigned to their respective residential block. If our baseline results above are being driven by unobservable shocks at a neighborhood level broader than a census block, for instance targeted Medicaid awareness campaign or church/social organization membership, then we should estimate similar effect sizes here as well. Furthermore, if network effects operate at a geographic level larger than an individual's census block we should again see higher effect sizes for this specification. However, our estimates for both the 6 month and 1-year comparison group in table 5 are extremely close to zero although statistically significant due to the large sample size. This gives us further confidence that our identification strategy is indeed uncovering the causal impact of neighborhood networks on program participation.

Table 5: Estimated Network Effect in Medicaid Participation - Falsification Test

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Panel A: Matched Pair Sample				
Network Effect (γ^M)	-0.003^{**}	0.000	-0.006^{**}	-0.002^{**}
	(0.001)	(0.000)	(0.001)	(0.000)
Intercept	0.426^{**}	0.461^{**}	0.397^{**}	0.433^{**}
	(0.002)	(0.000)	(0.002)	(0.000)
Observations	12,205,132	12,205,132	39,487,631	39,487,631
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓
Panel B: Individual Level Sample				
Fraction Medicaid (γ^I)	-0.033^{**}	-0.010^{**}	-0.036^{**}	-0.014^{**}
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	500,623	444,296	1,408,673	829,853
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

B Heterogeneity Analysis

The above subsection provided evidence for the existence of a robust network effect in program participation at the very local neighborhood level. We now explore the existence of potential heterogeneity in effect size across matched pairs in terms of race, educational attainment, nativity status, and separately by county.

B.1 Across Individual Characteristics of the Matched Pair

Previous literature has hypothesized the tendency for matching along dimensions that are similar across agents. Various studies have documented the existence of such assortative matching in behavior as diverse as job choice, marriage decisions, immigration decisions, and welfare take up (Becker, 1973; Marsden, 1987; Lam, 1988; Hendricks, 2001). Motivated by these observations, we decompose our estimated effect from section IV.A across a number of different characteristics of our matched pair of currently pregnant mother i and a recently pregnant mother j. This analysis illuminates potential mechanisms that can explain our estimated social interaction effect. Assortative matching would entail that similar individuals are likely to interact more with each other, and hence information flow from mother j to mother i, who say are both Hispanics, is more likely to take place. At the same time, one might expect information flow to take place from groups that, on average, are more likely to be eligible, and hence are likely to have more knowledge of welfare programs, to relatively unaware groups. We would expect a positive effect for mothers who differ on an observable characteristic that captures such a dynamic. In other words, it is difficult to isolate these two channels and hence our analysis, at best, can only provide some broad inferences about the underlying mechanisms governing the estimated network effect.

Table 6 presents results from the estimation of equation (4), exploring heterogeneity across race for our matched pair ij. The first two columns focus on the full sample of matched pairs with the omitted category being when both mothers are white ($i = White$; $j = White$). The estimates reported for various combinations then can be interpreted as the additional

Table 6: Heterogeneity Across Matched Pair Characteristics - By Race

	Full Sample		High School or Less		Above High School	
	(1)	(2)	(3)	(4)	(5)	(6)
Network Effect	0.011**	0.010**	0.033**	0.033**	0.005**	0.004**
	(0.002)	(0.001)	(0.004)	(0.004)	(0.001)	(0.001)
$i = Hisp$; $j = Hisp$	0.026**	0.021**	0.003	0.000	0.023**	0.012**
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.001)
$i = Hisp$; $j = Black$	0.001	0.006	−0.019**	−0.016**	−0.001	0.003
	(0.003)	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)
$i = Hisp$; $j = Asian$	0.012**	0.023**	−0.003	0.002	0.001	0.010**
	(0.003)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)
$i = Hisp$; $j = White$	0.029**	0.042**	0.022**	0.028**	0.008**	0.018**
	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.002)
$i = Black$; $j = Black$	0.016**	0.011**	−0.002	−0.006	0.011**	0.008**
	(0.005)	(0.003)	(0.006)	(0.005)	(0.005)	(0.003)
$i = Black$; $j = Hisp$	0.002	0.002	−0.019**	−0.019**	0.007*	0.004*
	(0.003)	(0.002)	(0.005)	(0.004)	(0.003)	(0.002)
$i = Black$; $j = White$	0.017**	0.024**	0.021**	0.020**	0.007**	0.004**
	(0.004)	(0.004)	(0.008)	(0.008)	(0.003)	(0.002)
$i = Black$; $j = Asian$	0.004	0.007	−0.013	−0.014	0.005	0.008**
	(0.004)	(0.004)	(0.007)	(0.007)	(0.003)	(0.003)
$i = White$; $j = Black$	0.007**	0.000	0.000	−0.010	0.000	−0.003
	(0.003)	(0.003)	(0.007)	(0.006)	(0.003)	(0.002)
$i = White$; $j = Hisp$	−0.006**	−0.005**	−0.024**	−0.020**	−0.003*	−0.003*
	(0.002)	(0.002)	(0.004)	(0.004)	(0.001)	(0.001)
$i = White$; $j = Black$	−0.008**	−0.002	−0.012*	−0.012**	0.008*	0.002
	(0.002)	(0.002)	(0.005)	(0.004)	(0.002)	(0.001)
Observations	21,816,919	21,816,919	13,557,466	13,557,466	7,738,486	7,738,486
Block Group and Year F.E	✓	✓	✓	✓	✓	✓
Individual F.E	×	✓	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level. The omitted category is the following pair: $i = White$; $j = White$. We suppress the coefficients for currently pregnant Asian mothers.

effect of the particular matched pair residing on the same census block over and above a white-white pair. For instance, we see that if both mother i and mother j are Hispanic there is an additional 2.6 pp likelihood of participation in Medicaid for the currently pregnant mother. We see a similarly positive number, 1.6 pp, when both mothers are black. The effect size for Asian mothers is of a similar magnitude to that of the omitted reference group of white mothers.

As outlined above, it is crucial to note that this cannot be solely attributed to assortative matching based channels. To explore this further, the next two columns restrict mother i to those with a high school education or less, while the last two columns restrict to currently pregnant mothers who have had more than high school education. From our analysis on eligibility in Section IV.A we concluded that high school or less educated mothers had higher network effects in program participation as expected. At the same time, the baseline prevalence of program participation is also higher in this sub group especially among minorities, and hence an average group member is likely to be more well informed about welfare programs. We find a null effect of having another Medicaid participating Hispanic mother on a block for currently pregnant mothers that are high school graduates or less, compared to white mothers. The effect is much higher among mothers with at least some college, around 2.3 pp, implying information flows towards more educated mothers about welfare participation among Hispanics. Similar patterns are observed when both mothers in the matched pair are black although the magnitudes are much smaller.

However, we see higher network effects even among pairs where $i = Hispanic$; $j = White$ and $i = Black$; $j = White$. This certainly does not fit an assortative matching story and other factors might be at work here. For instance, because minorities are more likely to be eligible any information flows are also more likely to be realized into actual participation among minorities, which can explain the additional effect over and above the base category of $i = White$; $j = White$. The estimated coefficients for a minority-white pair are positive and significant even when estimated separately by education in the last four columns of Table

6. Overall, this provides evidence for the existence of substantial heterogeneity based on the characteristics of the matched pair, although the exact mechanism is difficult to identify.[21]

B.2 Across Nativity Status within Racial Groups

In this subsection, we further explore heterogeneity concerns focusing on sub-samples where establishing a clear mechanism might be easier. For instance, Figlio et al. (2015) show that information about welfare programs is likely to flow within ethno-lingusitic group with nativity status of the mother determining direction. Because native born mothers are more likely to be aware of the prevailing welfare net, one can expect information flows to operate from native to foreign born mothers. This would especially be true if the foreign born mothers are relatively recent immigrants and hence have not fully acclimated to the US. In Table 7, we restrict our sample to only Hispanic mothers, in columns (1) and (2), and explore network effects across nativity status of the matched pair. Our omitted category is $i = Native$, $j = Native$, or pairs where both mothers were born in the US, and hence all comparison are relative to this pair. We estimate a 1 percentage point additional effect if the currently pregnant mother is foreign born while her matched recently pregnant mother is native born but only in column (2) where we incorporate the individual fixed effect. No statistically significant effect exists if both mothers are foreign born, however, we see a negative effect relative to both mothers being native born if $i = Native\, j = Foreign$.

The last two columns show similar findings when we do the nativity analysis for the full sample without restricting to Hispanic mothers. However, given that Hispanics represent close to 66 percent of all births in our sample, and 77 percent of these births are paid by Medicaid, we explore network effects within the Hispanic population in more detail. Appendix table A.6 breaks down Hispanic matched pairs by foreign born status, separating mothers born in Mexico from mothers born any where else outside the US. We again see an additional effect of 1.4 percentage point, for the individual fixed effect specification, when

[21] Eligibility may be an important confounder in the education regressions if white women with some college are less likely to be eligible for Medicaid compared to other races or ethnic groups.

Table 7: Heterogeneity Across Matched Pair Characteristics - By Nativity

	Only Hispanic		Full Sample	
	(1)	(2)	(3)	(4)
Network Effect	0.032** (0.002)	0.023** (0.001)	0.027** (0.002)	0.022** (0.001)
$i = Foreign$; $j = Native$	0.003 (0.002)	0.011** (0.002)	0.001 (0.002)	0.010** (0.002)
$i = Native$; $j = Foreign$	−0.007** (0.002)	−0.010** (0.001)	−0.005** (0.001)	−0.009** (0.001)
$i = Foreign$; $j = Foreign$	−0.004 (0.003)	−0.001 (0.002)	0.003 (0.004)	0.006** (0.003)
Observations	11,441,057	11,441,057	22,301,554	22,301,554
Block Group and Year F.E	✓	✓	✓	✓
Individual F.E	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level. The omitted category is the following pair: $i = Native$; $j = Native$.

the currently pregnant mother was born in Mexico and was matched with a native born Hispanic mother. Furthermore, the effect seems to be equally distributed by education levels of the currently pregnant mother. We see no robust evidence for an additional effect over the baseline when both mothers are born in Mexico. There is an opposite effect if the recently pregnant mother is foreign born and the currently mother is a native, as one would expect given that the baseline category is $i = Native\, j = Native$. It is crucial to note that undocumented mothers were not eligible for Medicaid during our study sample. However, we cannot determine in our data whether a woman born outside of the US was a citizen at the time she gave birth. To the extent that undocumented mothers are likely to have lower educational attainment and be ineligible for Medicaid, our results would be biased towards zero.

B.3 Across Spatial Dimensions

Thus far, our heterogeneity analysis has focused on individual characteristics of the matched pair using the full sample of the 5 densest counties in California. However, there are substantial differences in both eligibility and participation across cities which could result either from the demographic and socioeconomic composition of the population, or city specific unobservables affecting participation. Moreover, Aizer (2007) provides strong evidence for the role of reducing administrative hassle, including providing bilingual help in filling out applications, in increasing Medicaid take up rates, especially among Hispanic and Asian children. Using the roll out of this service across zip codes she demonstrates that differential implementation of assistance programs across cities can impact program take up. For instance, if a given city has a well developed information dissemination system for public welfare programs then one can expect relatively little room for potential information flows within neighborhood networks inducing higher participation.

The overall participation in our sample varies from around 35 percent for Alameda up to 64 percent for Los Angeles. Similarly, the demographic composition varies substantially across cities as well with Asians representing close to 32 percent of the population in Alameda and Hispanics making up 63 percent of the population in Los Angeles. Although our analysis above deals with individual characteristics, it is instructive to explore our baseline specification across cities to test whether effect sizes differ across cities with different demographics.

Table 8 reports results from estimating equation (1) and (2) by county. From Table 3 using all 5 counties, a currently pregnant mother is around 3 percentage points (8 percent) more likely to participate in Medicaid if a recently pregnant mother on her exact census block also received Medicaid benefits during her pregnancy. On the other hand, we estimate a much larger relative effect for Alameda where the participation is on average much lower. The effect size is approximately 24 percent of the baseline in Alameda. This compares to just 7 percent in Los Angeles. The results for San Francisco, Orange, and San Mateo Counties are between 12 and 13 percent. Overall, we document substantial heterogeneity in city specific network effects in program participation.

In appendix figure A.3, we further investigate the heterogeneity in the estimated effect across a baseline measure of participation in our sample. We define quartiles of participation in Medicaid in 2000, two years before the start of our estimation sample. As can be seen, we estimate a much higher relative effect of around 45 percent for the lowest quartile compared to only a 5 percent effect for the highest quartile of participation.

We also split our sample across time periods to document any differential effect as a result of broad macroeconomic or societal changes. For instance, restricting our sample to the Great Recession time period gives us robust estimates for network effects in program participation compared to our baseline results. Similarly, recent literature suggests that the exponential growth in social media might affect individual decisions including fertility (Kearney and Levine, 2015; Trudeau, 2016), which is particularly relevant for our analysis. Both Facebook and Twitter user bases have grown dramatically since 2009. However, we find extremely similar results compared to our baseline results in Table 3 when we restrict our sample to before 2009.[22]

V Potential Policy Implications

A Increasing Participation in Medicaid

We document robust evidence of substantial network effects in program participation at the local neighborhood level in section IV. This finding can inform policy interventions focused on increasing program participation, especially given takeup rates well below 100 percent (Gruber and Simon 2008). For instance, Dahl et al. (2014) find a protracted multiplier effect leading to higher long-run participation rates than one would expect in the absence of social interactions. At the same time, previous literature has also established that interventions

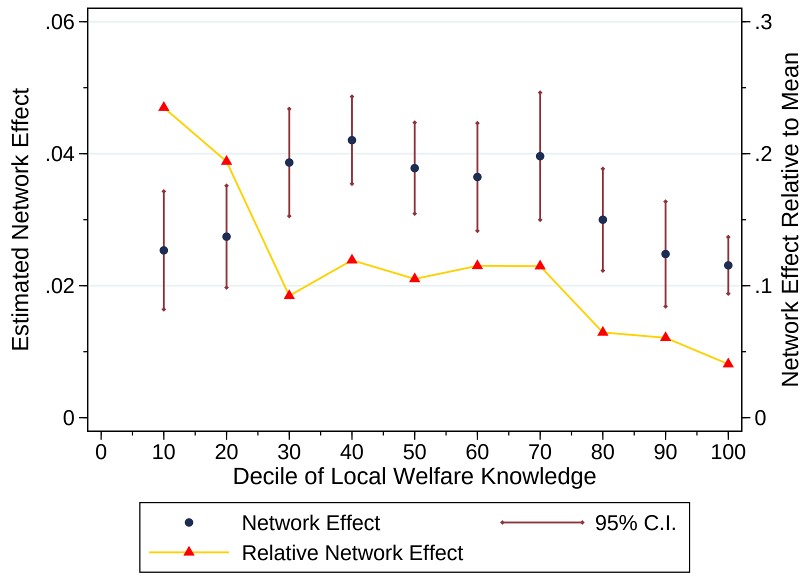
[22]Facebook was not available to the general public without a dot edu email address before 2006. Our results also are robust to limiting our sample to before 2006.

Table 8: Estimated Network Effect in Medicaid Participation - Across Counties

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Matched Pair Sample				
Baseline Sample	0.031** (0.002)	0.028** (0.001)	0.032** (0.002)	0.026** (0.001)
San Francisco	0.026** (0.005)	0.024** (0.005)	0.026** (0.004)	0.023** (0.003)
Los Angeles	0.028** (0.002)	0.025** (0.002)	0.028** (0.002)	0.023** (0.001)
Orange County	0.044** (0.005)	0.040** (0.004)	0.045** (0.005)	0.038** (0.003)
Alameda	0.028** (0.005)	0.024** (0.004)	0.027** (0.004)	0.023** (0.003)
San Mateo	0.021** (0.005)	0.021** (0.004)	0.023** (0.004)	0.022** (0.003)
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓

The unit of observation is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Figure 1: Network Effect by Local Welfare Knowledge



can be designed to help successfully disseminate information about program rules, among potential participants. For instance, Chetty and Saez (2013) implement such an intervention to teach the tax code to EITC participants. Furthermore, as discussed earlier, Chetty et al. (2013) also document substantial variation in local 'knowledge' of program rules across neighborhoods.

Motivated by these findings, we split our sample by potential prevalence of local knowledge of program rules and investigate whether the magnitude of estimated social interactions differ across neighborhoods. We proxy for neighborhood knowledge of welfare program rules using a measure developed by Chetty et al. (2013) for their study on EITC benefits defined at the ZIP-3 level.[23] Using their measure, we define block groups by decile of EITC knowledge based on our 5 sample counties. This is an instructive exercise as long as knowledge about EITC rules is correlated with general knowledge of welfare programs. Figure 1 presents results from this analysis. The solid circles represent the estimated network effect parameter for the relevant decile of local welfare knowledge. These estimates range from a little over 2 pp to 4.2 pp. To interpret these parameters, we plot the effect relative to the baseline preva-

[23]We are grateful to Raj Chetty for graciously sharing his dataset.

lence of Medicaid participation in each decile shown with a solid line and triangle markers. Estimated effects for the lowest knowledge decile are close to 24 percent, while this effect falls to around 10 percent from the third decile onward. This provides suggestive evidence that conditional on being matched to a Medicaid participant in the very low knowledge neighborhoods, the potential inducement of currently pregnant mothers into Medicaid is much higher. Hence, neighborhoods which have historically been prone to smaller participation to eligibility ratio can be substantially benefited by interventions designed to disseminate information about program rules.

B Medicaid Participation and Healthy Behavior

The above analysis establishes that there is a persistent, robustly estimated network effect at the local neighborhood level for program participation among pregnant mothers. A currently pregnant woman is more likely to be influenced to enroll in Medicaid if another recently pregnant mother on her residential census block also received Medicaid benefits during pregnancy. Moreover, these estimates are unlikely to be driven by pure neighborhood sorting concerns (see Section III.C). In this section, we estimate whether increased participation in Medicaid also affects pregnant women's health behaviors during pregnancy. Vital Statistics data only contains adequate measurements of healthy behavior for prenatal care.[24] We observe both the timing of the first prenatal care visit and the total number of visits throughout the pregnancy. These two channels may improve birth outcomes as well (Reichman and Teitler, 2005; Joyce, 1999).

A crucial feature of our data is that we separately observe prenatal care paid for by Medicaid, as well as the more common measure of just observing whether delivery expenditure was paid for by Medicaid. We first restrict all currently pregnant mothers i to those whose delivery was paid for by Medicaid. This creates a sub-sample of mothers that we know were eligible for Medicaid prenatal care benefits as well. We explore whether mother i is more

[24]California Vital Statistics data does not contain a consistent measure of cigarette smoking.

likely to start prenatal care earlier if she observes recently pregnant mothers doing the same because of enrollment in Medicaid. We estimate equation (1) to explore this behavior across two distinct dimensions of prenatal care usage in Table 9. The dependent variable for the first two columns takes the value 1 when both mother i and j receive prenatal care paid for by Medicaid and initiate care in their first trimester. We estimate that a currently pregnant mother is around 3.6 pp more likely to initiate prenatal care paid for by Medicaid during the first trimester if she observed a recently pregnant mother doing the same in her pregnancy using Medicaid benefits. We estimate a slightly muted effect for initiation of prenatal care in the first two months, which is expected given that early on a substantial proportion of mothers are not aware of their pregnancy.

Column (5) and (6) of Table 9 define the dependent variable as a measure of the intensity of prenatal care. The outcome variable takes the value 1 if both mothers in the matched pair received Medicaid prenatal care benefits and had more than the median prevalence of prenatal care visits in the population. We again see a positive and statistically significant effect with currently pregnant mothers 2 percentage points more likely to receive intensive prenatal care. Moreover, all estimated coefficients are extremely robust to the inclusion of individual fixed effect for mother i. Panel B repeats the above analysis for the individual level sample. Although we find zero or very small positive effects for early imitation into prenatal care, we still report a fairly substantial network effect in the intensity of prenatal visits.

VI Conclusion

We present robust evidence for the existence of social interaction in program participation among pregnant women in California. Currently pregnant women are induced to take-up Medicaid during pregnancy if a recently pregnant mother, on her exact census block, also received Medicaid benefits during her pregnancy. Our results confirms previous work

Table 9: Prenatal Care and Medicaid Participation

	First Trimester		First 2 Months		Above Median Visits	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Matched Pair Sample						
Network Effect (γ^M)	0.035**	0.036**	0.024**	0.025**	0.020**	0.021**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Intercept	0.471**	0.494**	0.335**	0.341**	0.258**	0.274**
	(0.002)	(0.000)	(0.002)	(0.000)	(0.003)	(0.000)
Observations	13,336,661	13,336,661	13,010,810	13,010,810	12,717,610	3,679,611
Block Group and Year Fixed Effect	✓	✓	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓	×	✓
Panel B: Individual Level Sample						
Fraction Medicaid (γ^I)	0.007**	0.000	0.012**	0.008*	0.044**	0.043**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	549,296	471,816	543,828	467,954	536,889	462,312
Block Group and Year Fixed Effect	✓	✓	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

suggesting network effects operate at the very local level (see e.g., Schmutte, 2015). We provide suggestive evidence that increased participation in Medicaid also induced mothers to engage in healthier behavior during pregnancy by improving prenatal care initiation and intensity. Finally, our findings suggest that exploiting peer and social networks can be a powerful policy lever in increasing program participation, especially in areas with lower knowledge of welfare participation rules.

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A For Online Publication

Figure A.1: 6 Months

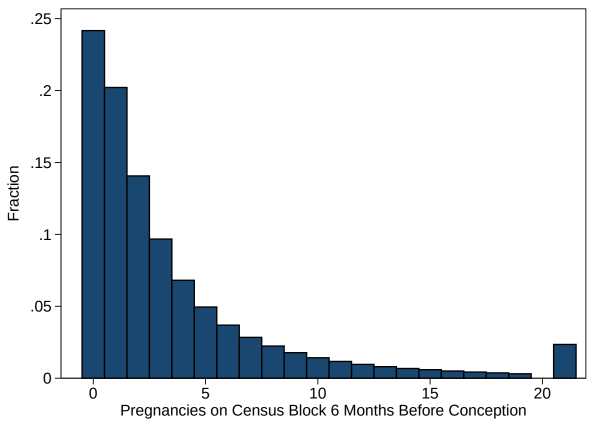

Figure A.2: 1 year

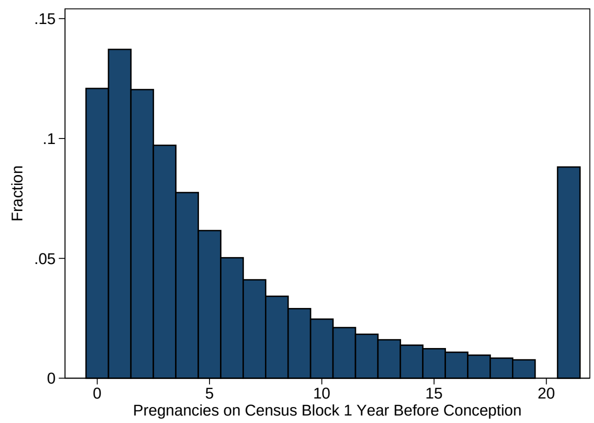

Figure A.3: Network Effect by Medicaid Participation in 2000

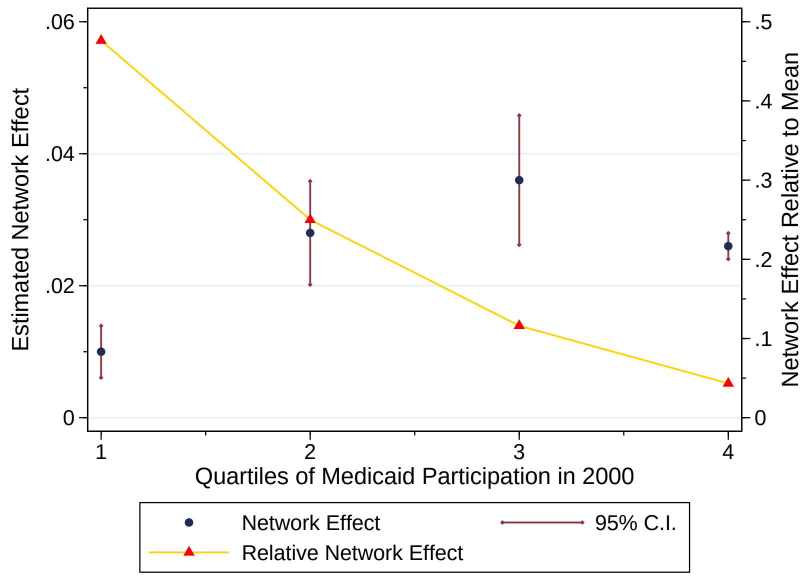

Table A.1: Estimated Network Effect in Prenatal Medicaid Participation - First Births

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Matched Pair Sample				
Network Effect (γ^M)	0.026**	0.023**	0.026**	0.021**
	(0.002)	(0.001)	(0.002)	(0.001)
Intercept	0.337**	0.359**	0.324**	0.349**
	(0.002)	(0.000)	(0.002)	(0.000)
Observations	8,474,973	8,474,973	22,257,460	22,257,460
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓

The unit of observation is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Table A.2: Estimated Network Effect in Medicaid Participation - At least 5 Matches

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Panel A: Matched Pair Sample				
Network Effect (γ^M)	0.033**	0.032**	0.032**	0.029**
	(0.002)	(0.002)	(0.002)	(0.001)
Intercept	0.384**	0.424**	0.378**	0.418**
	(0.003)	(0.001)	(0.002)	(0.000)
Observations	15,648,288	15,648,288	46,844,200	46,844,200
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓
Panel B: Individual Level Sample				
Fraction Medicaid (γ^I)	0.146**	0.064**	0.222**	0.094**
	(0.007)	(0.005)	(0.006)	(0.004)
Observations	566,415	502,366	1,012,972	899,903
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Table A.3: Estimated Network Effect in Medicaid Participation - Excluding Dense Blocks

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Panel A: Matched Pair Sample				
Network Effect (γ^M)	0.032**	0.027**	0.033**	0.025**
	(0.003)	(0.002)	(0.003)	(0.002)
Intercept	0.444**	0.486**	0.430**	0.475**
	(0.004)	(0.001)	(0.004)	(0.001)
Observations	9,600,792	9,600,792	22,766,781	22,766,781
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓
Panel B: Individual Level Sample				
Fraction Medicaid (γ^I)	0.184**	0.082**	0.271**	0.124**
	(0.012)	(0.006)	(0.013)	(0.007)
Observations	247,224	217,954	301,533	265,622
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level. We drop the top 10% blocks in terms of match density as a robustness check.

Table A.4: Estimated Network Effect in Prenatal Medicaid Participation

	6 Month Comparison		1-year Comparison	
	(1)	(2)	(3)	(4)
Panel A: Matched Pair Sample				
Network Effect (γ^M)	0.031**	0.028**	0.032**	0.026**
	(0.002)	(0.001)	(0.002)	(0.001)
Intercept	0.368**	0.407**	0.351**	0.393**
	(0.002)	(0.000)	(0.002)	(0.000)
Observations	22,344,123	22,344,123	58,534,106	58,534,106
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓
Panel B: Individual Level Sample				
Fraction Medicaid (γ^I)	0.135**	0.056**	0.201**	0.087**
	(0.005)	(0.003)	(0.004)	(0.002)
Observations	938,760	834,078	1,408,673	1,254,616
Block Group and Year Fixed Effect	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Table A.5: Restricting Currently Pregnant Mothers by Education - At least 5 Matches

	Less Than High School		High School		Some College		College Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Matched Pair Sample								
Network Effect (γ^M)	0.050**	0.051**	0.036**	0.034**	0.019**	0.017**	0.004**	0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Intercept	0.592**	0.648**	0.414**	0.469**	0.245**	0.282**	0.061**	0.061**
	(0.003)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.002)	(0.000)
Observations	6,011,593	6,011,593	3,664,177	3,664,177	2,467,179	2,467,179	2,958,575	2,958,575
Block Group and Year Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effect	×	✓	×	✓	×	✓	×	✓
Panel B: Individual Level Sample								
Fraction Medicaid (γ^I)	0.039**	0.028**	0.108**	0.083**	0.105**	0.068**	0.072**	0.050**
	(0.006)	(0.006)	(0.009)	(0.009)	(0.011)	(0.011)	(0.009)	(0.009)
Observations	216,141	190,645	139,768	125,478	96,068	88,648	99,759	97,595
Block Group and Year Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓
Individual Characteristics	×	✓	×	✓	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. The unit of observation in Panel B is an individual mother i. The main variable of interest in this panel is the fraction of Medicaid use of all individuals giving birth in the same block in the 6 months prior to the conception of mother i. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level.

Table A.6: Heterogeneity Across Matched Pair Characteristics - Within Hispanics

	Full Sample		High School or Less		Above High School	
	(1)	(2)	(3)	(4)	(5)	(6)
Network Effect	0.031**	0.023**	0.034**	0.027**	0.021**	0.014**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
$i = Foreign^{Mex}$; $j = Native$	0.004*	0.014**	−0.003	0.011**	0.003	0.008**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
$i = Foreign^{Mex}$; $j = Foreign^{Mex}$	−0.003	0.001	−0.006*	−0.003	0.011*	0.002
	(0.002)	(0.002)	(0.003)	(0.005)	(0.005)	(0.002)
$i = Foreign^{Mex}$; $j = Foreign^{Any}$	−0.007*	0.000	−0.010**	−0.003	0.005	0.001
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.004)
$i = Foreign^{Any}$; $j = Native$	−0.002	0.003	−0.004	0.001	0.005	0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)
$i = Foreign^{Any}$; $j = Foreign^{Mex}$	−0.008*	−0.008**	−0.012*	−0.011**	0.005	−0.003
	(0.003)	(0.002)	(0.003)	(0.002)	(0.006)	(0.003)
$i = Foreign^{Any}$; $j = Foreign^{Any}$	−0.004	−0.004	−0.009**	−0.008**	0.011	0.002
	(0.004)	(0.003)	(0.004)	(0.003)	(0.006)	(0.004)
$i = Native$; $j = Foreign^{Mex}$	−0.006**	−0.010**	−0.009**	−0.012**	−0.004	−0.007**
	(0.002)	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
$i = Native$; $j = Foreign^{Any}$	−0.008**	−0.009**	−0.011**	−0.011**	−0.004	−0.004
	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)
Observations	11,441,057	11,441,057	9,493,795	9,493,795	1,766,589	1,766,589
Block Group and Year F.E	✓	✓	✓	✓	✓	✓
Individual F.E	×	✓	×	✓	×	✓

The unit of observation in Panel A is a matched pair of a currently pregnant mother i and a recently pregnant mother j, residing in census block group, g. Refer to section III for complete details. Standard errors are clustered at the census block group level. Bootstrapped standard errors are similar to those reported here. ** indicates significance at the 1% level and * indicates significance at the 5% level. The omitted category is the following pair: $i = Native$; $j = Native$.