Networks or Neighborhoods? Correlations in the Use of

Publicly-Funded Maternity Care in California

Anna Aizer*
(Brown University)

Janet Currie**
(UCLA and NBER)

Abstract

This study focuses on "network effects" in the utilization of publicly funded prenatal care using Vital Statistics data from California for 1989 to 2000.   Networks are defined using 5-digit zip codes and a woman's racial or ethnic group.   Like others, we find evidence that the use of public programs is highly correlated within groups defined using race/ethnicity and neighborhoods. These correlations persist even when we control for many unobserved characteristics by including zip code-year fixed effects, and when we focus on the interaction between own group behavior and measures of the potential for contacts with other members of the group ("contact availability"). However, the richness of our data allows us to go further and to conduct several tests of one important hypothesis about networks: That the estimated effects represent information sharing within groups. The results cast doubt on the idea that the observed correlations can be interpreted as evidence of information sharing. In particular, we find estimated effects to be a large or larger among women who have previously used the program as among first-time users.

* Dept. of Economics, Brown University, Box B, 64 Waterman St., Providence R.I. 02912.

** Dept. of Economics, UCLA, 405 Hilgard Ave., Los Angeles CA 90095-1477, currie@simba.sscnet.ucla.edu.

Introduction

Social scientists have long been interested in the interpretation of what have come to be known as network effects.[1] Yet in the absence of a well-designed experiment, it is difficult to determine whether correlations in observed behaviors within groups represent a causal influence of one person on another (for example, through the sharing of information) or some unmeasured characteristic of the group or neighborhood. In this paper, we attempt to shed light on this issue using data from California's birth public use files from 1989 to 2000.

In California, eligibility for public prenatal and delivery services has been extended to women in families with incomes up to 300 percent of the federal poverty line.[2] However, large differences in the utilization of prenatal care persist between racial and ethnic groups and across geographic areas. Remediation efforts make it important to know whether they reflect neighborhood characteristics such as differential access to facilities, or differences in the knowledge or attitudes about public programs that might be shared by women in different social networks.

We define "networks" using a pregnant woman's 5-digit zip code and other new mothers in the same racial or ethnic group. Thus, our implicit assumption about networks is that pregnant women are most likely to be influenced by new mothers from the same area and ethnic group in terms of their own choices about takeup of public maternity care programs. The richness of our data allows us to control for omitted variables to a greater degree than many other non-experimental studies. In particular, we can include zip code-year effects in many of our models in order to control for characteristics of local neighborhoods. We explore various measures of network effects including the "contact availability measure" suggested by Bertrand, Luttmer, and Mullainathan (2000).

---

1. Moffitt (forthcoming) provides a survey of much of this literature. Prominent recent examples include Katz, Kling and Liebman (2001), Ludwig, Duncan, and Hirschfield (2001), Duflo and Saez (2001), Borjas (1992, 1995) , Borjas and Hilton (1996), and Calvo-Armengol and Jackson (2002).

2. See Aizer and Currie (2002) for more information on public maternity care in California.

The more important contribution of our study is that we attempt to say something about the nature of this effect. A leading hypothesis about networks is that people rely on them for information about public programs. We test this hypothesis by comparing estimated network effects for first and second births. Once a woman has had a publicly-funded delivery, she learns both about the existence of the public program (if she did not know already), and that she is eligible.

We also find that women in different ethnic groups use different hospitals regardless of where they live, and that this fact drives much of the estimated correlation in the use of public maternity care services. This strong sorting of women across hospitals may be facilitated by networks, but it is also likely to indicate differential behavior by hospitals which can be considered a neighborhood rather than a network effect.

Finally, we compare estimated network effects for foreign-born and native-born Hispanic women. We find little evidence that foreign-born women are more dependent on networks than native born women, even though foreign-born women may require more information. This suggests once again that information-sharing within networks is not the primary reason for the observed correlations in use of publicly funded programs.

To summarize, like many others, we find evidence that behavior is highly correlated within groups defined using race/ethnicity and neighborhoods. These correlations persist even when we control for many unobserved characteristics via zip code-year effects, and when we focus on the interactive effect of own group behavior and own group "contact availability". However, our investigations cast doubt on the idea that these correlations can be interpreted primarily as evidence of information sharing.

1. Background

As Manski (1993), Moffitt (forthcoming), Brock and Durlauf (2001) and others have highlighted, correlations between the behavior of the index woman and other women from her "network" could reflect an *endogenous effect* where the propensity of an individual to behave in a particular way is causally influenced by the behavior of other members of the group; an *exogenous effect* where the individual's behavior is

influenced by an exogenous characteristic that defines group membership; or a *correlated effect* where individuals from the same group tend to behave the same way because they have similar individual characteristics, or face similar constraints.

One type of endogenous effect that is often mentioned is stigma (c.f. Moffitt, 1983), but surveys of eligible non-participants in means-tested programs find that welfare stigma is seldom the only barrier to participation. On the other hand, lack of information about ones own eligibility and about the magnitude of the benefits available is a common problem. For example, Daponte, Sanders, and Taylor (1999) found that informing households about their eligibility, as well as about the size of the benefit available to them, increased participation in the Food Stamp program. Aizer (2001) shows that enrollments in the State Children's Health Insurance Program (SCHIP) are higher in states that contract out outreach efforts, suggesting that providing information about programs can increase take up. However, in addition to information about the existence of programs, women may need information about where to go to receive services under the program, and immigrants might require additional information about their eligibility to use services.[3]

To the extent that they are shared by members of a group, direct transactions costs associated with enrolling in Medicaid could be viewed as an exogenous effect. Currie and Grogger (2001) show that declines in welfare rates have significant effects on Medicaid caseloads, presumably because women who lose welfare eligibility must go through a separate certification process to retain Medicaid benefits. Aizer (2002) finds that community-based organizations that assist women in filling out applications have had a large impact on the takeup of public health insurance in California, which further underlines the importance of transactions costs.

Finally, the behavior of hospitals towards different groups of women may be viewed as an example of

---

3. Proposition 187 in 1994 and the welfare reform act of 1996 created considerable uncertainty about whether immigrants were in fact eligible.

a correlated effect. Hospitals serving large numbers of indigent women have strong incentives to help eligible women to enroll because they are required to provide emergency care to women in active labor. Thus, births to eligible women may be covered by Medicaid even when prenatal care is not, which accounts for the fact that usage of public prenatal care services has lagged behind the public funding of deliveries (Ellwood and Kenney, 1995).

On the other hand, hospitals can certainly take steps to either encourage or discourage some groups of eligible women from presenting. Duggan (2000) discusses a change in the system of reimbursing hospitals for indigent care which made it profitable for some hospitals to increase the share of Medi-Cal deliveries in their caseloads. He shows that hospitals were able to respond in this way to financial incentives, suggesting that they can manipulate their indigent caseloads even when they are required by law to provide care.

2. Data and Methods

a) Data Overview

The data for this study come from the California Birth Public Use File for 1989 to 2000. These data are abstracted from birth certificates. Variables relevant for our analysis include the mother's age, race, education, marital status, place of birth, and 5-digit zip code; the parity of the child; month prenatal care began; the principal source of payment for prenatal care; the principal source of payment for delivery, which may be different; and the hospital of delivery.

We distinguish three racial/ethnic groups: African-Americans (blacks), non-Hispanic whites (whites), and Hispanics. We have excluded Asians and the five percent of mothers who are of "other" or unknown race. Asians in California are a relatively small and very heterogeneous group. It proved impractical to use our methods to analyze subgroups of Asians due to small sample sizes. We also limit our analysis to mothers with less than 4 years of college since few college-educated women use public prenatal care services.

The outcome we focus on is whether women who went on to have a public delivery (and therefore are

known to have been eligible for public prenatal care services) used these services beginning in the first trimester of their pregnancies.

Table 1 shows the means of the variables included in model (1) for first and second births, by race and ethnicity. Hispanic women have the largest number of publicly-funded deliveries, followed by whites and then blacks. Hispanic mothers with public deliveries also have much lower education than black or white mothers, though the the probability that they are single mothers is comparable to whites.[4] Finally, 71 percent of these Hispanic mothers are foreign born. While most mothers with a public delivery report receiving some public prenatal care, only around 70 percent of them received public prenatal care and began care in the first trimester.

The second part of Table 1 reports characteristics of the neighborhoods of the women receiving public deliveries. Here, variables such as the share of the woman's own group with public prenatal care in the first trimester are calculated using all women in the group, not just those with public deliveries. Hence, for example, 43 percent of Hispanic women in these neighborhoods received publicly funded prenatal care in the first trimester, compared to 67 percent of those Hispanic women who actually went on to have a public delivery. Other neighborhood characteristics are also calculated using the Vital Statistics data, and so pertain to new mothers. For example, on average, Hispanic women with public deliveries live in zipcodes in which new mothers are less educated and which have relatively high fractions of births to teenage mothers

A comparison of means for the first and second births, shows that in general they are quite similar, particularly in terms of maternal education and neighborhood characteristics. One interesting difference is that second births are somewhat less likely to be to single women.

In Aizer and Currie (2002) we also show that there is tremendous variation over time in the fraction

---

4. Up until 1997, California "inferred" marital status using other information on the birth certificate rather than asking it directly. However, we found that this change in procedures produced little change in the fraction of married women. For the sample as a whole, the fraction married was 36% in 1996, and 40% in 1998, while for Hispanics, the corresponding fractions were 39%, and 43%.

of women receiving public first trimester prenatal care across race/ethnic groups within zipcodes. We also

show that the probability that public prenatal care is chosen increases with both "contact availability" and

with the share of one's own group using the service.

b) Estimation

We begin with a base model of the form:

(1) $Y_{ait} = a_0 + a_1 X_{ait} + a_3 YBAROWN_{ait-1} + a_4 N_{at} + a_5 YEAR_t + v_{ati}$,

where Y is one of the two outcomes, and the subscripts i, a, and t denote the individual, area, and time,

respectively. The vector X includes maternal characteristics such as education (high school dropout, high

school graduate, some college); marital status; whether the mother is foreign born; and age (teen, 20-29, 30-

39, 40-45, 45+). The vector N includes measurable time-varying characteristics of the neighborhood such as

the fraction of mothers in each race/ethnic group; the fraction of births to teen mothers; the fraction of births

to single mothers; and the fraction of births to mothers who are foreign born, YEAR is a vector of year

dummies, and the variable YBAROWN is the fraction of all women in the index woman's race/ethnic group

and zip code group who used the publicly funded service in the 11 months prior to the birth month. This

model follows several suggestions of Brock and Durlauf (2001) in that we use panel data and use lags of

YBAROWN in order to identify the effect (and avoid the "reflection" problem). Brock and Durlauf (2001)

also show that this model can be derived from, and is consistent with, a standard utility maximization

framework.

In our second model, we augment (1) by defining YBAROTHER, which is the fraction of women

of other race/ethnicity in the index woman's zipcode who used the public service (either delivery or prenatal

care) in the past 11 months. In the third model, we add zipcode fixed effects. In these specifications, the

variable YBAROTHER controls for time-varying characteristics of neighborhoods which affected all women,

while the zipcode fixed effects capture any variable that is fixed over time.

The fourth model we estimate is analogous to those of Bertrand, Luttmer, and Mullainathan (2000):

(2) $Y_{ait} = a_0 + a_1 X_{ait} + a_3 YBAROWN_{ait-1} + a_4(YBAROWN_{ait-1}*CA_{ait}) + a_5 YBAROTHER_{ait-1} + a_6 ZIPCODE*YEAR_{at} + v_{ati}$,

where all the variables are defined as before and CA is "contact availability", or the fraction of new mothers in the local area who are in the index woman's group.   The intuition behind the inclusion of the interaction term is that even if the main effect of a variable like YBAROWN is driven by omitted variables bias, network effects ought to be more important in areas where people are more likely to have contact with someone in their group. Since the interaction term varies within zip codes and years, we can also control for unobserved neighborhood-specific variables in a very complete way by including zip code-year fixed effects. The zip code-year effects subsume the vector N that was included in (1) above, and also subsume the "main effect" of contact availability. However, they do not quite subsume the YBAROWN and YBAROTHER variables because these are defined using the 11 months prior to the index woman's birth rather than the calendar year. Finally, we estimate an alternative to (2) that includes a spline in YBAROWN, to ensure that the interaction between CA and YBAROWN does not simply pick up an underlying non-linear effect of YBAROWN.

All of these models are estimated separately for each race/ethnic group, and using only first births. In order to test the "information hypothesis" about the reason for network effects, we estimate three more models. First, we estimate the final model using second births. We argue that if the primary function of networks is to furnish information about publicly funded maternity care programs, then network effects should be more important for first pregnancies than for subsequent pregnancies. In addition to estimating the model over all second births, we obtained a confidential version of the birth certificate data with the mother's name, birthdate, and the birth interval. Using this information we were able to link first and second births to the same mother, so that we could identify second births to mothers who had previously used public deliveries, or public prenatal care services. These women should certainly be informed about such services.

Second, we estimate models with hospital*year fixed effects rather than zipcode*year fixed effects. While the choice of hospital is obviously endogenous, these models can tell us how much of the estimated network effect is associated with the sorting of women who live in the same place into different hospitals. If

7

controlling for hospital of delivery significantly reduces the size of the estimated "network effects", then this suggests that the estimated effect could in fact be picking up unobserved characteristics associated with the public health infrastructure available to each group; that is, it could be at least partly a neighborhood rather than a network effect.

Third, we estimate separate models for foreign and native-born Hispanic women. We estimate two different versions of these models, one in which the YBARs are defined using all Hispanic women and one in which they are defined using only native/foreign born women. Given the maintained hypothesis that foreign-born women need more information about these programs (e.g. about how use of the program will affect their immigration status), we expect that if information sharing drives network effects, we will find larger effects among foreign-born women when we define the network to include only foreign-born women.

3. Estimates

a) "Baseline" estimates

Panels 1 through 3 of Table 2 show the estimated effect of YBAROWN in models of the form (1). Panel 1 suggests that YBAROWN has a very large effect on the use of public maternity care services. Panel 2 shows that among Hispanics, this effect is only slightly mitigated by the inclusion of YBAROTHER, which has a statistically insignificant effect. However, among whites and blacks, the inclusion of YBAROTHER reduces the size of the coefficient on YBAROWN by more than 50 percent, and YBAROTHER has a larger impact than YBAROWN. Thus, Panel 2 suggests that at least among whites and blacks, much of the estimated "network effect" may actually be a neighborhood effect. Panel 3 shows that the estimated effect of YBAROWN is also very sensitive to the inclusion of zip code fixed effects among whites and blacks, though not among Hispanics.

The other variables included in the Table 1 specifications are not shown due to space constraints but have the anticipated signs. For example, in the Panel 3 model, being a teenage mother instead of a mother 45 or older reduces the probability of having used public prenatal care among Hispanics, while the effect of

being a highschool dropout rather than having some college reduces the probability. There is also some evidence that being in a neighborhood with a higher fraction of people from ones own group is associated with a higher probability of using public prenatal care in the first trimester.

Panels 4 and 5 of Table 2 show estimates of models of the form (2), which include interactions between "contact availability" and YBAROWN as well as zip code-year fixed effects. Panel 4 includes only the main effect of YBAROWN, while Panel 5 includes a spline in YBAROWN, as described above. The effects are much larger when the spline is included, although only the effect for Hispanics is statistically significant in either specification. Given the means in Table 1, the estimated value of a_4 in column 1 of Panel 5 implies that increasing the number of Hispanic women using publicly funded prenatal care in the first trimester from .43 to .53 while keeping CA constant would increase the probability that the index woman used such care by 11 percentage points.

The coefficients on the spline on YBAROWN in Panel 5 suggest that it does indeed have a non-linear effect on the propensity to use public programs. Note that YBAROWN is not subsumed by the zipcode*year fixed effects, because it is defined over a different time period than the calendar year (the 11 months prior to the birth). For Hispanics, the main effect of increasing YBAROWN is increasingly negative, while for whites and blacks, it becomes less negative as YBAROWN increases. The most reasonable explanation for these different patterns may be that YBAROWN does indeed capture omitted characteristics of neighborhoods, which is the reason we focus on the interaction between YBAROWN and CA. Since the effects of YBAROWN do seem to be non-linear, we estimate our remaining models using a specification comparable to that shown in Panel 5 of Table 2.

b) Interpreting the Estimated Network Effects

As discussed above, there are many women who have publicly funded deliveries in California who did not begin the use of publicly funded prenatal care in the first trimester. This may be because these women lack information about the programs, or because they do not know how to apply. Networks could act by passing on this type of information. However, mothers who have already had one delivery are likely to know

a lot more about these programs than women who have not. Specifically, if they are eligible, they will find out about the program and how to get on it. Thus, if the main role of networks is to pass on this type of information, we should expect that networks will have much smaller effects for second births than they do for first births.

Panel 1 of Table 3 shows estimates similar to those in Panel 5 of Table 2 except that they are estimated using all second births rather than all first births. The coefficients on the interaction terms in the models for second births actually tend to be somewhat larger than the corresponding coefficients from models of first births, and the coefficient becomes statistically significant for blacks. Thus, there is no evidence that the estimated network effects are greater for first than for second births, as one might have expected if the estimated network effects reflected shared information about the existence of these public programs.

Panel 2 of Table 3 shows models estimated using only second births to women from the matched sample of first and second births who are known to have used publicly funded prenatal care during their first pregnancy. These women obviously know about such care, and so should have no need of obtaining information about it from their networks. Yet, the estimated effects of "networks" in this sample are almost the same as those for all second births. Similarly, Panel 3 of Table 3 focuses on second births to women known to have had a public delivery at the first birth. There is arguably less possibility for measurement error in public payor for first birth rather than public payor for prenatal care in the first trimester. However, the results are quite similar. Thus these estimates provide strong evidence that the estimated effects cannot be due to women sharing information about the program, since they exist even among women who know all about the program.

Panel 4 of Table 3 shows models similar to Panel 5 of Table 2 except that they include hospital*year effects rather than zipcode*year effects. These estimates examine the estimated network effects conditional on the choice of hospital. Recall that our baseline models with zipcode*year fixed effects control for the availability of hospitals. For example, if a woman lives near a hospital that is very accommodating to Medicaid mothers, then this is accounted for. In contrast, the models with hospital fixed effects control for

10

the hospital that was actually chosen by the mother, given her own characteristics and those of her neighborhood. Controlling for the choice of hospital reduces the size of estimated size of the network effect greatly.

This striking result suggests that network effects in the use of public maternity services could be "explained" by the choice of hospital where the woman delivers. This choice, in turn, is likely to reflect characteristics of the hospital which are changing over time, such as the quality of services offered to Medi-Cal mothers, whether translation services are provided, whether the hospital is affiliated with clinics attractive to low income women, and so on. That is, the estimated network effect may reflect exogenous constraints on groups of women rather than endogenous network effects. The choice of hospital may also be affected by information sharing among women in a network, but without differences in hospital services to begin with, there would be no relevant information to share. It is also difficult to explain this result via "stigma". If the stigma a woman feels changes with the number of people in her group who use public services, then one would expect this to be largely independent of the hospital chosen.

Panel 5 of Table 3 explores the results for Hispanics further by dividing women into those who are foreign-born and those who are native born. These models are estimated at the level of the Minor Civil Division (MCD) rather than at the zip code level, in order to conserve sample size. An MCD is a small cluster of 4 to 5 contiguous zip codes. In results that are not shown, we found that moving from the zip code to the MCD level had little impact on the estimated network effect for Hispanics.

Panel 5 shows that the estimated network effect is remarkably similar for foreign-born and native-born mothers, despite the fact that native-born mothers might be expected to have better information about U.S. social programs, and that foreign-born mothers might have larger informational requirements. It is possible that the fact that hospitals seldom ask about a woman's immigration status, so that it is likely that hospitals treat foreign-born and native-born Hispanic women very similarly.

4. Discussion and Conclusions

A possible concern with our calculation of YBAROWN is that there might be a spurious variation within small zip codes. In order to check for such measurement error, we have recalculated all of our estimates excluding mothers in the smallest 10 percent of race-zip code-year cells. This procedure produced very similar estimates of the coefficient on YBAROWN.

In Aizer and Currie (2002) we also estimated models that looked separately at the probability that a woman had a public delivery, at the probability that she used publicly funded prenatal care, and at the probability that she used public prenatal care in the first trimester. In essence, these estimates relax the assumption which is maintained in this paper, that all the women who are entitled to a public delivery actually receive one. We found however, that the qualitative results were very similar to those reported above.

Like many others, we find evidence that the takeup of public programs is highly correlated within groups defined using race/ethnicity and neighborhoods. These correlations persist even when we control for many unobserved characteristics via zip code-year effects, and when we focus on the interactive effect of own group behavior and own group "contact availability". In our view, the more important contribution of the paper is that the richness of our data allows us to go further, and to attempt to test the hypothesis that the estimated effects represent information sharing within groups.

Our results cast doubt on the idea that the estimated network effects reflect the sharing of information about the public program within the network. In particular, we show that the estimated "network" effects persist even among women who know all about the services because they have used it before. We also suggest that the estimated effect may actually reflect exogenous constraints on the women, in the form of hospital-level behavior. If this is the case, then we may be able to gain further understanding of group-level correlations in the propensity to take up public programs by examining the way that relevant institutions, such as hospitals, treat members of different groups; or alternatively, how members of different groups respond to similar treatment. We currently have little systematic data about the way that most public programs are administered "on the ground". While this study focuses on a particular program in a specific location, the results suggest that such administrative information could help us to understand group and location-level

differences in the propensity to participate in public programs more generally.

References

Aizer, Anna. "Covering Kids: Improving the Health Insurance Coverage of Poor Children", xerox, Dept. of Economics UCLA, August 2001.

Aizer, Anna. 2002. "Got Health? Advertising, Medicaid, and Child Health?", UCLA xerox, January 2002.

Aizer, Anna and Janet Currie. "Networks or Neighborhoods? Correlations in the Use of Publicly Funded Maternity Care in California", NBER Working Paper #9209, Sept. 2002.

Bertrand, Marianne, Erzo Luttmer, and Sendhil Mullainathan. "Network Effects and Welfare Cultures", Quarterly Journal of Economics, v CXV, August 2000, 1019-1056.

Borjas, George. "Ethnic Capital and Intergenerational Mobility", Quarterly Journal of Economics CVII, 1992, 123-150.

Borjas, George. "Ethnicity, Neighborhoods, and Human-Capital Externalities", American Economic Review, LXXXV, 1995, 365-390.

Borjas, George and Lynette Hilton. "Immigrant Participation in Means-Tested Entitlement Programs", Quarterly Journal of Economics, v111 #2, May 1996, 575-604.

Brock, William and Steven Durlauf. "Interactions Based Models" in James Heckman and Edward Leamer (eds.) Handbook of Econometrics, v5 (Amsterdam: North Holland) 2001.

Calvo-Armengol, Antoni and Matthew Jackson. "Social Networks and Resulting Patterns and Dynamics of Employment and Wages", California Institute of Technology xerox, March 2002.

Currie, Janet and Jeffrey Grogger. "Medicaid Expansions and Welfare Contractions: Offsetting Effects on Prenatal Care and Infant Health", Journal of Health Economics, v21 #2, March, 2002, 313-335.

Currie, Janet and Jonathan Gruber , "Health insurance eligibility and Child Health: Lessons from Recent Expansions of the Medicaid program", Quarterly Journal of Economics, May, 431-466, 1996a.

Currie, Janet, and Jonathan Gruber. "Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women," Journal of Political Economy, December 1996b.

Daponte, Beth, Seth Sanders, and Lowell Taylor. "Why Do Low Income Households Not Use Food Stamps: Evidence From an Experiment", Journal of Human Resources, 34 #3, Summer 1999, 612-628.

Duflo, Esther and Emmanuel Saez. "The Role of Information and Social Interaction in Retirement Plan Decisions: Evidence from a Randomized Experiment", xerox, Dept. of Economics, Harvard University, 2001.

Duggan, Mark. "Hospital Ownership and Public Medical Spending", Quarterly Journal of Economics, v 115 #4, November 2000, 1343-1373.

Ellwood, Marilyn and Genevieve Kenney. "Medicaid and Pregnant Women: Who is Being Enrolled and When?", Health Care Financing Review, 17 #2, 1995, 7-28.

Katz, Lawrence, Jeffrey Kling, and Jeffrey Leibman. "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment", Quarterly Journal of Economics, CXVI, #2, May 2001, 607-645.

Kogan, Michael D. et al. "Relation of the Content of Prenatal Care to the Risk of Low Birth weight", Journal of the American Medical Association, 221 #17, May 4, 1994, 1340-1345.

Ludwig, Jens, Greg J. Duncan, and Paul Hirschfield. "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing Mobility Experiment", Quarterly Journal of Economics, CXVI #2, May 2001 655-680. Manski, Charles. "Identification of Endogenous Social Effects: The Reflection Problem", The Review of Economic Studies, 60 #3, July 1993, 531-542.

Manski, Charles. "Economic Analysis of Social Interactions", Journal of Economic Perspectives, v14 #3, Summer 2000, 115-136.

Moffitt, Robert. "An Economic Model of Welfare Stigma", American Economic Review, 73 #5, December 1983, 1023-1035.

Moffitt, Robert. "Policy Interventions, Low-Level Equilibria and Social Interactions" in Steven Durlauf and Peyton Young (eds.) Social Dynamics (Cambridge MA: MIT Press) forthcoming.

Table 1: Variable Means for Mothers with Public Deliveries, and all Mothers in Neighborhood

	First Births			Second Births		
Maternal Characteristics	Hispanic	Black	white	Hispanic	Black	White
Public Prenatal	0.962	0.983	0.987	0.958	0.986	0.988
	(0.0003)	(0.0005)	(0.0003)	(0.0003)	(0.0005)	(0.0003)
Public Prenatal in First Trimester	0.672	0.719	0.731	0.666	0.732	0.729
	(0.0006)	(0.0017)	(0.0010)	(0.0007)	(0.0019)	(0.0012)
Prenatal in First Trimester (any payor)	0.697	0.731	0.741	0.692	0.743	0.738
	(0.0006)	(0.0017)	(0.0010)	(0.0007)	(0.0019)	(0.0012)
Maternal Age	21.299	20.686	22.124	24.365	23.996	25.052
	(0.0059)	(0.0172)	(0.0108)	(0.0072)	(0.0214)	(0.0133)
Maternal education	9.734	11.848	12.142	9.375	12.171	12.177
	(0.0039)	(0.0068)	(0.0043)	(0.0048)	(0.0070)	(0.0050)
single	0.590	0.810	0.548	0.494	0.749	0.441
	(0.0006)	(0.0015)	(0.0011)	(0.0008)	(0.0019)	(0.0013)
foreign born	0.707	0.050	0.086	0.766	0.050	0.099
	(0.0006)	(0.0008)	(0.0006)	(0.0006)	(0.0009)	(0.0008)
Neighborhood Characteristics						
Share of own group with public prenatal in 1st	0.430	0.451	0.298	0.439	0.453	0.309
	(0.0002)	(0.0004)	(0.0004)	(0.0002)	(0.0005)	(0.0004)
Share of other group with public prenatal in 1st	0.327	0.404	0.385	0.334	0.402	0.393
	(0.0002)	(0.0005)	(0.0003)	(0.0002)	(0.0006)	(0.0004)
Share Asian (all births)	0.045	0.059	0.049	0.045	0.061	0.048
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0003)	(0.0002)
Share Black (all births)	0.073	0.253	0.062	0.075	0.249	0.064
	(0.0001)	(0.0007)	(0.0002)	(0.0002)	(0.0008)	(0.0002)
Share Hispanic (all births)	0.652	0.463	0.365	0.659	0.456	0.366
	(0.0003)	(0.0008)	(0.0005)	(0.0003)	(0.0009)	(0.0005)
Share White (all births)	0.198	0.182	0.480	0.191	0.191	0.478
	(0.0002)	(0.0007)	(0.0005)	(0.0003)	(0.0008)	(0.0006)
Share of Teen Mothers	0.154	0.166	0.138	0.154	0.166	0.140
	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
Share of Single Mothers	0.423	0.479	0.333	0.427	0.477	0.337
	(0.0001)	(0.0005)	(0.0002)	(0.0002)	(0.0005)	(0.0003)
Share of Foreign-born Mothers	0.556	0.443	0.325	0.560	0.436	0.324
	(0.0002)	(0.0007)	(0.0004)	(0.0003)	(0.0007)	(0.0005)
Average maternal education	10.242	10.669	11.334	10.215	10.673	11.309
	(0.0014)	(0.0037)	(0.0021)	(0.0016)	(0.0042)	(0.0025)
Average maternal age	25.870	25.659	26.089	25.858	25.638	26.010
	(0.0011)	(0.0028)	(0.0026)	(0.0012)	(0.0032)	(0.0030)
Observarions	576178	70447	200920	397334	48772	120038

Table 2: Estimates of "Network Effects" on the Publicly Funded Prenatal Care in 1st Trimester

	Hispanic	Black	White
1. No zipcode fixed effects			
share own group with outcome	0.607	0.299	0.283
	[0.020]	[0.019]	[0.027]
	0.07	0.05	0.04
2. No zipcode fixed effects, controls for "others"			
share own group with outcome	0.617	0.127	0.152
	[0.023]	[0.023]	[0.025]
share other group with outcome	-0.022	0.294	0.268
	[0.021]	[0.027]	[0.026]
R-squared	0.07	0.05	0.05
3. Zipcode fixed effects, controls for "others"			
share own group with outcome	0.58	0.095	0.04
	[0.011]	[0.021]	[0.029]
share other group with outcome	0.099	0.285	0.334
	[0.014]	[0.018]	[0.030]
R-squared within	0.0648	0.0292	0.0381
4. Zipcode-year fixed effects, controls for "others"			
share own*percent own	1.189	0.142	-0.4
	[0.098]	[0.379]	[0.240]
share own group with outcome	-0.894	-0.861	-0.478
	[0.059]	[0.050]	[0.073]
share other group with outcome	0.1	0.182	0.02
	[0.025]	[0.037]	[0.068]
R-squared within	0.01	0.02	0.02
5. Zipcode-year fixed effects, controls for "Others", spline in interaction			
share own group w/outcome*share own group	1.726	0.447	0.291
	[0.107]	[0.286]	[0.164]
share other group with outcome	0.122	0.012	0.177
	[0.025]	[0.068]	[0.037]
share own group w/outcome	-0.998	-0.756	-1.673
	[0.069]	[0.108]	[0.121]
share own group w/outcome - middle third	-0.483	0.095	0.792
	[0.069]	[0.162]	[0.134]
share own group w/outcome - top third	-0.162	0.09	0.169
	[0.083]	[0.156]	[0.109]
R-squared within	0.01	0.02	0.02
Observations	576178	70447	200920

Robust standard errors in brackets. Regressions 1-3 include year fixed effects, the share of teen mothers, share of foreign-born mothers, mean maternal education, mean maternal age and the share of single mothers. All regressions inlcude the following maternal characteristics: whether foreign-born, single, teenage, age20-29, age 30-39, age 40-45, a high school drop out, or a high school graduate

Table 3: Tests of the Hypothesis that Networks Transmit Information

1. Model estimated using 2nd births	**Hispanic**	**Black**	**White**
share own*percent own	1.937	0.815	0.259
	[0.144]	[0.373]	[0.233]
Observations	397334	48772	120038
R-squared within	0.01	0.02	0.02

2. Model using 2nd births to women who used public prenatal care in 1st pregnancy			
share own*percent own	1.268	0.786	0.136
	[0.248]	[0.591]	[0.348]
Observations	161031	20640	49022
R-squared within	0.01	0.02	0.02

3. Model using 2nd births to women who had a public delivery at 1st pregnancy			
share own*percent own	1.549	0.788	0.12
	[0.231]	[0.581]	[0.345]
Observations	174755	21164	49792
R-squared within	0.01	0.02	0.02

4. Model estimated using 1st births, including hospital*year fe			
share own*percent own	0.126	0.231	0.025
	[0.028]	[0.102]	[0.043]
Observations	575730	70275	200520
R-squared within	0.01	0.01	0.01

5. Model estimated separately for native and foreign-born Hispanic women		
	Native	**Foreign-born**
share own*percent own	1.466	1.553
	[0.297]	[0.195]
Observations	176154	423019
R-squared within	0.01	0.01