NETWORK EFFECTS AND WELFARE CULTURES*

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We empirically examine the role of social networks in welfare participation using data on language spoken at home to better infer networks *within* an area. Our empirical strategy asks whether being surrounded by others who speak the same language increases welfare use more for those from high welfare-using language groups. This methodology allows us to include local area and language group fixed effects and to control for the direct effect of being surrounded by one's language group; these controls eliminate many of the problems in previous studies. The results strongly confirm the importance of networks in welfare participation.

I. INTRODUCTION

Extreme segregation of the poor in the United States has sparked a wave of theories about the disadvantaged. Many social scientists now argue that a culture has developed in which poverty reinforces itself through social networks.[1] When the disadvantaged interact mainly with other disadvantaged, networks can inhibit upward mobility. Contacts may supply more information about welfare eligibility than job availability. They may provide negative peer pressure rather than positive role models. This paper empirically investigates the importance of social networks in welfare use.

While the effect of social networks on individual behavior has long been emphasized by sociologists, economists have only recently become interested in the effects of social pressure and information spillovers.[2] Game theorists have studied the importance of learning from neighbors and information spillovers in the

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1. See, for example, the pioneering work of Wilson [1987].
2. Granovetter [1985] is an example of a sociologist who discusses the importance of embedding individual behavior into social structure.

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emergence of equilibrium. Macroeconomists have stressed the importance of human capital spillovers as determinants of growth and inequality. Labor and public economists have used stigma and informational spillovers to explain a range of outcomes including program participation, fertility, crime, and education.[3]

Empirical work, however, has found it difficult to demonstrate network effects. The existing empirical work reveals that many individual outcomes are indeed positively correlated with friends', neighbors', and ethnic group's outcomes. Such correlations have been demonstrated for many variables, including crime, drug use, single motherhood, and educational attainment. While suggestive of network effects, these correlations may result from unobserved factors about individuals, neighborhoods, and ethnic groups. For example, some areas may have better schools, making both individuals and their neighbors less likely to use welfare.

In this paper we use language spoken at home to proxy for the social links between individuals *within* a neighborhood. Ample evidence suggests that people in the United States who speak a non-English language at home interact mainly with others who speak that language.[4] Therefore, individuals living in an area with more people speaking their language will have a larger pool of available contacts. We use the number of people in one's local area who speak one's language to measure the "quantity" of networks, or *contact availability.* Contacts drawn from high welfare-using groups will likely exert a stronger influence on welfare recipiency. Therefore, welfare use of the language group provides a measure of network "quality."[5] We focus on the

3. See Banerjee [1992], Bikhchandani, Hirshleifer, and Welch [1992], Bulow and Klemperer [1994], Ellison and Fudenberg [1993, 1995], Glaeser [1997], and Goolsbee and Klenow [1998] for examples of work on information cascades and social learning. Bénabou [1996], Durlauf [1996], Lucas [1988], and Romer [1986] are examples from the literature on growth and inequality effects of human capital spillovers. Besley and Coate [1992], Borjas [1992, 1994, 1995], Case and Katz [1991], Glaeser, Sacerdote, and Scheinkman [1996], Moffitt [1983], Montgomery [1991], and Nechyba [1996] are examples from the labor and public economics literature.

4. Alba [1990] reports that the use of mother tongue is an important determinant of ethnic identity. Individuals who are more connected to their ethnic community are much more likely to speak that language. Bakalian [1993] asks foreign-born American-Armenians to list their three best friends. She finds that 71 percent of them list at least one Armenian, and 35.6 percent list all Armenians. Asked about their other friends, more than 78 percent of them said that more than half were Armenian. As expected, these numbers are lower for second and later generation immigrants.

5. By language group we mean all individuals in the United States who speak that language at home.

differential effect of increased contact availability across language groups: does being surrounded by one's language group increase welfare recipiency more for individuals from high welfare-using language groups?[6]

A simple example illustrates our approach. Imagine that an American migrates to Belgium. In order to take advantage of the generous Belgian welfare system, she would need help in understanding the rules and procedures. As the number of English speakers in her area increases, so too does the number of people who could potentially help her. Moreover, the familiarity that English speakers have with welfare affects the kind of help they could provide. At one extreme, if the English speakers all shunned welfare and were quite unfamiliar with it, they may even discourage her from participating. At the other extreme, if they all knew a great deal about it, this may actively encourage her to participate. Therefore, the "return," in terms of welfare participation, to being surrounded by English speakers rises with the familiarity English speakers have of welfare. This is the heart of our test. We focus on the interaction term between the number of people in one's area speaking one's language and the mean welfare use of one's language group in the whole country.

We implement our test using data from the 1990 United States Census 5 percent Public Use Micro Sample, which provides information on language spoken at home, welfare recipiency, as well as detailed geographic and individual information. Using a variety of specifications and samples, we consistently find strong evidence for network effects. Because several aspects of networks are not included (for example, neighborhood effects are eliminated by the fixed effects), our estimates may underestimate the true network effects. Nevertheless, the network effects we find are economically significant in size.

The main contribution of this paper is that it circumvents many of the omitted variable biases that typically plague estimates of network effects. Using language and geography to proxy for social networks generates variation within local areas and language groups, allowing us to include both local area and language group fixed effects. By measuring networks as the interaction of the quality of contacts and the quantity of contacts, we can control for the direct effects of quality and quantity. These

6. As we discuss later, focusing on this interaction term controls for any fixed differences between individuals with high and low contact availability.

controls eliminate many of the standard omitted variable biases, such as differences in leniency of welfare offices between areas, quality of local schools, differences in prejudice faced by different language groups, and omitted characteristics of people who choose to self-segregate, i.e., live in high contact availability areas. Finally, we investigate any remaining omitted variable biases by instrumenting and exploring the effects of dropping covariates. We believe that these innovations represent significant progress on the difficult problem of distinguishing network effects from unobserved differences between individuals, areas, and groups.

II. Methodology

II.1. Conceptual Framework

Economists have become increasingly interested in how social networks impact a broad set of behavior: job search [Montgomery 1991], education [Coleman et al. 1966], consumption [Abel 1990], and unemployment [Akerlof 1980] to list only a few examples. Social networks affect individual behavior through two important channels: information and norms. The information channel emphasizes how a person's knowledge depends on the behavior of others. The social norm channel emphasizes how a person's preferences themselves may depend on the behavior of others, either directly by affecting taste or indirectly via social pressure.[7] Both mechanisms highlight how nonmarket interactions can influence aggregate outcomes. They generate feedback effects that can amplify shocks or lead to multiple equilibria.

Against this broader backdrop, we will focus on the effect of social networks on welfare decisions. In this context the informational channel operates in many ways. Contacts knowledgeable about the welfare system can reduce the cost of applying for welfare (e.g., telling one which documents to bring to the welfare office) or increase benefits (e.g., sharing information about a little-known welfare provision). Besides increasing information about the welfare system, these contacts may also decrease information about alternative options to welfare, such as job opportunities, or information about activities that affect eligibility for welfare (e.g., birth control information). For example, a friend

7. For theoretical analyses, see Banerjee [1992] and Bikhchandani, Hirshleifer, and Welch [1992] for the informational channel and Akerlof [1980] for the norms channel.

who is on welfare may make one more likely to use welfare because they are less able to tell you about job opportunities.

The norms channel operates through peer pressure, stigma, or social approval. Anderson [1990] vividly describes the wide collection of norms that affect young people in an inner-city ghetto.[8] While some of these norms directly affect welfare participation (e.g., welfare stigma), many others affect other behaviors that indirectly affect welfare participation. Such norms may include peer pressure to become sexually active at an early age to avoid appearing "square," norms against abortion, or norms regarding how much responsibility unwed fathers should take for their children. We investigate to what extent information and norms operate on intervening variables such as marriage and childbirth in subsection III.5.

The different mechanisms may in theory have different policy implications. In practice, however, we are unable to distinguish between them. We think of the language group's welfare participation rate as a measure of their "welfare culture," again with the understanding that culture may operate through information or norms. We will refer to the language group's influence on own behavior as the "network effect."

II.2. Empirical Framework

To explain our methodology for estimating these, we begin by supposing that the true model governing welfare participation is given by

$$\Pr\,(Welf_{ijk}) = Netw_{ijk}\alpha^* + X_i^*\beta^* + Y_j^*\gamma^* + Z_k^*\delta^* + \epsilon_{ijk},$$

where i indexes individuals, j indexes areas, k indexes language groups, $Welf_{ijk}$ is a dummy indicating welfare recipiency, $Netw_{ijk}$ measures the information and social pressure from contacts, X_i^* are observed and unobserved personal characteristics, Y_j^* are observed and unobserved local area characteristics, Z_k^* are observed and unobserved language group characteristics, and ϵ_{ijk} is an error term.

Measuring $Netw_{ijk}$ raises difficulties. Few data sets contain information on actual contacts. Moreover, individuals choose their contacts, exacerbating omitted variable biases. For example, an individual with many friends on welfare may be different from one

8. Lindbeck, Nyberg, and Weibull [1999] provide a theoretical model of the effects of norms on welfare use.

who has few friends on welfare. Thus, estimation of this model poses two potentially interacting problems: measurement and omitted variable biases.

Much of the previous literature used mean neighborhood characteristics to proxy for networks.[9] This implicitly assumes that contacts are randomly distributed within the neighborhood. In this framework one would estimate

$$\Pr(Welf_{ij}) = \overline{Welf}_j\alpha + X_i\beta + \epsilon_{ij}, \tag{1}$$

where $\overline{Welf}_j$ represents mean neighborhood welfare recipiency and X_i are observed individual characteristics. This regression suffers from what Manski [1993] calls the "reflection problem." Does individual behavior depend on the behavior or characteristics of the group (social effects), or do individuals in a group behave similarly because they are subject to the same shocks (correlated effects)? One can view the reflection problem as a manifestation of two related omitted variable biases.[10] (1) Omitted personal characteristics may be correlated with $\overline{Welf}_j$. For example, individuals living in bad areas may be less ambitious. (2) Omitted neighborhood characteristics may be correlated with $\overline{Welf}_j$. For example, neighborhoods with a lenient welfare office may increase an individual's probability of welfare use as well as the mean welfare use in the area. More generally, this raises a simultaneity problem since any shocks affecting the whole neighborhood's welfare use will result in a positive $\hat{\alpha}$.[11] Both these biases are likely positive, resulting in an overestimate of α^*. Thus, finding a positive $\hat{\alpha}$ cannot be interpreted as evidence of networks.

Some researchers have examined randomized (or pseudo-randomized) experiments to attack these questions. In the

9. Jencks and Mayer [1990] present a thorough survey of this literature. Papers have estimated neighborhood effects for a variety of socioeconomic variables, including crime, drug use, sexual behavior, and educational attainment. Most papers tend to find correlations between individual and mean neighborhood outcomes, but as Jencks and Mayer point out, these correlations are sensitive to the inclusion of additional family background characteristics and should not be taken as strong evidence of neighborhood effects.

10. In addition, Manski [1993] investigates the possibility of separately identifying two types of social effects: endogenous effects, wherein the individual behavior depends on the behavior of the group (e.g., their welfare participation), and exogenous effects, wherein individual behavior depends on exogenous characteristics of the group (e.g., their work ethic or attitudes toward welfare). In our application, these two social effects are not separately identified. Hence, our estimates of network effects reflect both.

11. Case and Katz [1991] circumvent this simultaneity problem by instrumenting for mean peer behavior with parental background variables of the teenagers in the neighborhood.

Gautreaux experiment, individuals were assigned to neighborhoods in Chicago in what Rosenbaum [1995] contends was an essentially random manner. His analysis finds that women allocated to better neighborhoods experience better outcomes. Motivated partly by this work, Moving to Opportunity was established, a true random assignment demonstration. The initial results of MTO research are suggestive of strong neighborhood effects on child problem behaviors, child and adult health outcomes, and juvenile crime [Katz, Kling, and Liebman 1999; Ludwig, Duncan, and Hirshfield 1999; Hanratty, McLanahan, and Petit 1998]. Since these analyses use the random assignment of *neighborhood,* they are best at quantifying the effects of neighborhoods rather than networks. The resulting estimates capture not only network effects but also the effect of having jobs closer, more lenient welfare offices, or better schooling to cite a few examples. Of course, when combined with ethnographic work on the nature of social interactions that program participants experience, they have great potential for improving our qualitative understanding of network effects.

Borjas [1992, 1995] has investigated network effects using a different approach. First, rather than being determined by geographical proximity, he assumes networks are based on ethnic similarity. In essence, he uses mean outcomes in the ethnic group to measure $Netw_{ijk}$. Second, he is primarily interested in the effect of the *previous* generation's outcomes on the current generation's. He refers to the average quality of the ethnic group in the previous generation as ethnic capital [Borjas 1992]. To investigate the effect of ethnic capital in the context of welfare use, one can imagine estimating the following regression:[12]

$$\Pr\,(Welf_{ijk}) = \overline{Welf}_{(-1)k}\alpha + X_i\beta + Y_j\gamma + Z_k\delta + \epsilon_{ijk}, \tag{2}$$

where $\overline{Welf}_{(-1)k}$ is the mean welfare recipiency of the ethnic group in the previous generation and Z_k are observed language group characteristics. This regression also suffers from two omitted variable biases. (1) Omitted personal characteristics may be

12. Borjas and Hilton [1996] estimate a more complex version of this equation. They study whether "the *type of benefits* received by earlier immigrant waves influence the types of benefits received by newly arrived immigrants" (italics added). They find that participation in a specific program is correlated with mean participation in that program for the earlier wave, even after controlling for global mean welfare use of the previous wave. This hints at ethnic networks transmitting information about welfare programs.

correlated with $\overline{Welf}_{(-1)k}$.[13] (2) Omitted ethnic group characteristics may be correlated with $\overline{Welf}_{(-1)k}$. For example, ethnic groups facing higher levels of discrimination may need to rely more on welfare. Again, these biases are positive, making it hard to draw firm inferences about networks from $\hat{\alpha}$.

Our approach expands both the work on neighborhood effects and ethnicity: we use geographic and ethnic variation. One advantage of combining the two approaches can easily be seen in the two previous equations. In equation (1) one can include ethnic fixed effects, while in equation (2) one can include neighborhood fixed effects. A regression that exploits both the ethnic and geographic dimensions of networks, therefore, allows the inclusion of both neighborhood and ethnic fixed effects. This deals with two biases mentioned above: omitted neighborhood and ethnic group characteristics.

Moreover, unlike Borjas, we use language rather than ancestry as our measure of "ethnicity." Since ancestry can often include individuals more loosely connected to their ethnic group, we feel language provides a more precise measure of social links.[14] We measure $Netw_{ijk}$ using the number of people the individual interacts with in combination with the attitudes and knowledge of those people toward welfare. Thus, our network measure includes "quantity" and "quality" of contacts. If interactions occur mainly within language groups, we can write

$$Netw_{jk} \approx \begin{pmatrix} \text{density of} \\ \text{language} \\ \text{group } k \\ \text{in area } j \end{pmatrix}_{jk} \times \begin{pmatrix} \text{welfare knowledge} \\ \text{and attitudes of} \\ \text{others from} \\ \text{language group} \\ k \text{ who live in area } j \end{pmatrix}_{jk}.$$

The density of language group k living in area j measures *contact availability,* denoted by CA_{jk}, or sometimes CA for simplicity.[15]

13. In Borjas's papers this problem is less severe since many of the omitted characteristics are actually part of his story. For example, groups with higher ethnic capital may transmit more (potentially unobserved) skills to successive generations, and this is one mechanism by which ethnic capital operates.

14. Lazear [1995] provides an interesting analysis of the determinants of language use by immigrant groups.

15. In the empirical section we will measure CA_{jk} slightly differently. Instead of simply taking the proportion of neighbors who speak the language, we will take the proportion and then divide by the proportion in the entire country that speaks the language. Our results are insensitive to this choice, but the alternative measure has several nice properties. Most importantly, it does not underweight groups that are small in the overall country, as proportions would.

This is our "quantity" measure. The above formula suggests that we proxy the knowledge and attitudes of others from language group k in area j with the mean welfare use of language group k in area j (excluding individual i), which we refer to as $\overline{Welf}_{(-i)k}$. Because $\overline{Welf}_{(-i)jk}$ may reflect unobserved characteristics that an individual has in common with people from the same language group living in the same area, it can introduce an omitted variable bias. To avoid this, we replace $\overline{Welf}_{(-i)jk}$ by $\overline{Welf}_k$, the mean welfare use of the whole language group in the United States.[16] We, therefore, estimate[17]

$$(3)\quad Welf_{ijk} = (CA_{jk} * \overline{Welf}_k)\alpha + X_i\beta + \gamma_j + \delta_k + CA_{jk}\theta + \epsilon_{ijk},$$

where γ_j and δ_k are fixed effects for local areas and language groups. As noted above, CA_{jk} is a measure of the "quantity" of contacts available, and $\overline{Welf}_k$ is a measure of the "quality" of contacts: it proxies for the knowledge and attitudes of individuals from one's language group in the area. The interaction of these two will be our measure of networks. We, of course, include the direct effect of CA_{jk} as a control. We do not include the direct effect $\overline{Welf}_k$ because the language group fixed effects δ_k incorporate it. A positive estimate of α provides evidence of network effects.

This methodology allows us to control for many common omitted variable biases. First, including local area fixed effects deals with any unobserved differences between areas, such as variation in job availability. Second, the language group fixed effects absorb omitted characteristics of language groups, such as different levels of discrimination. Third, directly including CA_{jk} as a regressor deals with any omitted personal characteristics that are correlated with CA_{jk}. For example, an unobserved characteristic, such as ambition, may reduce both the likelihood of receiving welfare and the probability of living among one's own language group. This would show up as a positive estimate of θ, but it would not affect the estimate of α.

16. We do not use $\overline{Welf}_{(-i)k}$, the mean welfare use of the whole language group minus individual i. In unreported regressions we have deleted the individual from the calculation and the results are completely unchanged as might be expected from the sample sizes involved. We have also removed all individuals in the MSA (in the same language group) from the mean, and the results remain significant. It is also worth noting that the "welfare use of the whole language group" refers to the mean within in our subsample of women.

17. Although $Welf_{ijk}$ is a binary variable, we estimate a linear probability model instead of a probit or logit because probits and logits become computationally infeasible in the presence of about 1200 area fixed effects. As a specification check, we do estimate probit and logit models without the fixed effects and find similar results. See Bertrand, Luttmer, and Mullainathan [1998] for details.

One potential omitted variable bias remains. Omitted personal characteristics that are correlated with $CA_{jk} * \overline{Welf}_k$ may bias the regression. Such a correlation would arise if individuals differentially self-select away from their language group. Including *CA* in the regression controls for *fixed* differences between people who choose to live among their own language group and those who do not. But these differences may vary by language group. For example, living away from your language group may signal success if you are from a high welfare language group, whereas it may signal welfare proneness if you are from a low welfare group. Such *differential selection* might lead us to find networks where none exist. We investigate the plausibility of this hypothesis by instrumenting CA_{jk} with the number of people from language group k in the entire metropolitan area. As we discuss more thoroughly in subsection III.2, the comparison of the IV and OLS estimates leads us to believe that our results cannot completely be explained by differential selection.

II.3. Data

We use the 5 percent 1990 Census Public Use Micro Sample (PUMS). The two most precise geographic indicators in these data are the Public Use Microdata Area (PUMA) and the Metropolitan Statistical Area (MSA). PUMAs typically contain 100,000 inhabitants while MSAs refer to the extended city and, therefore, vary in size. Because we want to be able to use the same sample in regressions with PUMA and MSA level measures, we exclude people who live in mixed MSAs or in non-MSAs. We also exclude the institutional population from the sample.

Language variables are extracted from the question, "Does this person speak a language other than English at home? What is this language?" This does not identify everybody conversant in a language, making the contact availability measure an undercount.[18] For the aggregate counts by MSA or PUMA of the number of people in a language group, we sum this variable across the entire 5 percent sample. All counts are of people, not households.

We measure the size of social networks by contact availability (*CA*). CA_{jk} is the proportion of people in area j that belong to language group k divided by the proportion of people in the United

18. It would also be nice to have information about other speakers of a language, e.g., second-generation immigrants who speak only English at home but are still conversant in the home tongue. Of course, since these people are less likely to have strong ties to their ethnic group, this omission is likely not too serious.

States from that language group.[19] In most specifications we use the log of this ratio.[20] Hence, the contact availability measure is defined as

$$\ln\left(\frac{C_{jk}/A_j}{L_k/T}\right),$$

where C_{jk} is the number of people in area j who belong to language group k, A_j is the number of people who live in area j, L_k is the total number of people in the country who belong to language group k, and T is the total number of people in the country.

We have not simply used the proportion because it prevents us from underweighting small language groups. In the proportional regressions, small groups would appear to have very small contact availability regressions because even at full concentration they would never be a large fraction of any area. Given that individuals tend to self-segregate (and not match randomly), this could be quite misleading. In practice, the results are insensitive to this division.[21]

The sample used in the regressions is a subset of the sample used to construct the contact availability measures. First, we restrict the sample to non-English speakers.[22] Too many people speak only English for that language to be a good proxy of the size of an English speaker's social network. Second, we restrict the sample to language groups that have more than 2000 people sampled in the 5 percent PUMS, which represents 400,000 people in the United States.[23] The rationale for this is to drop language groups that are so small that the sampling error for the concentration measure at the PUMA-level would be high. Third, we restrict the sample to women between the ages of 15 and 55. We do not include older women since some of their measured welfare participation would actually be recipiency of Supplemental Security Income (SSI).

The variable "welfare use" is a dummy variable that equals one if the individual received any income from public assistance

19. Areas are either PUMAs or MSAs.

20. We have extensively checked the robustness of our results to the choice of this measure. Using simply fraction in the area, not taking logs or many other variants all produce similar results. See Bertrand, Luttmer, and Mullainathan [1998] for details.

21. Mechanically, the ratio is absorbed by the language group fixed effects in the log formulation since it enters additively.

22. Individuals in our sample may also speak English, but they need to speak a language other than English at home.

23. The results are insensitive to this choice. This is not surprising, since these small language groups contribute relatively few sample points.

(other than Social Security income). The Census does not ask more precise questions about the type of public assistance received. The variable "welfare use" includes more than just Aid to Families with Dependent Children (AFDC) because it also includes public assistance such as General Assistance and Heating Assistance. However, in-kind benefits such as provided by the Food Stamps program and the Women, Infants, and Children (WIC) program might not have been reported as income from public assistance. Whenever we refer to "welfare," we mean all forms of public assistance as measured by the variable "welfare use." Our measure of mean welfare use by language group is based on the women in the sample at this point.

In the end, we obtain 42 language groups, 271 MSAs, 1,196 PUMAs, 22,543 PUMA-language cells, and 6,197 MSA-language cells. The final sample consists of 397,200 women between the ages of 15 and 55 who do not speak English at home, whose language group consists of at least 2000 individuals in the 1990 5 percent PUMS, and who live in a single MSA.

II.4. Summary Statistics

Table I summarizes the main variables. The women in our sample resemble the average women of the same age except in three respects. First, 5.8 percent of the women in our sample receive welfare, whereas this figure is only 4.7 percent for the country average. Second, the women in our sample have had less education on average. Especially striking is that the percentage of women without a high school degree is about twice as high (40 percent versus 22 percent).[24] Finally, our sample has a higher fraction of people who are neither White nor Black. Cross tabulations (not presented) indicate that a substantial number of women who are *not* single mothers still receive welfare. This confirms that our welfare measure is not just measuring AFDC participation but also other forms of welfare.

In Table II we give selected summary statistics for each of the 42 language groups. The most striking fact is that more than 50 percent of our sample speak Spanish.[25] The remaining languages

24. This raises concerns that our four education dummies do not capture enough of the variation in education level. Hence, we also replaced the four education dummies by a finer partition of seven education groups. More specifically, we split the high school dropouts in four different groups: less than first grade, first to fourth grade, fifth to eighth grade, and ninth to twelfth grade (without diploma). The results were unchanged.

25. In Table V we investigate the effects of excluding Spanish speakers from our regressions.

TABLE I
SUMMARY STATISTICS

Variable:	Mean	Standard deviation	United States mean
Welfare	.0582	.2341	.0466
Age	33.03	11.02	33.88
HS drop out	.4020	.4903	.2200
HS degree	.2164	.4118	.2790
Some college	.2247	.4174	.2995
College and more	.1569	.3637	.2015
Single mother	.0960	.2945	.0979
Married, spouse present	.5177	.4997	.5296
Married, spouse absent	.0393	.1943	.0181
Widowed	.0185	.1348	.0166
Divorced	.0735	.2610	.1042
Separated	.0392	.1940	.0318
Never married	.3118	.4632	.2996
Child present	.4671	.4989	.4315
Number of kids (if number >0)	2.58	1.59	2.37
White	.5075	.4999	.7746
Black	.0488	.2155	.1255
Foreign-born	.6323	.4822	.1388
Years since entry (if foreign born)	13.29	9.60	14.57
English fluency	.7673	.4225	.9601

Variable:	Mean	Standard deviation	Minimum	Maximum
PUMA CA	7.85	20.79	.0123	325.60
MSA CA	4.33	9.76	.0070	161.14
Log PUMA CA	1.12	1.32	−4.40	5.79
Log MSA CA	0.86	1.08	−4.96	5.08

a. Data for columns 1 and 2 in the top panel are composed of all females between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).

b. Data for the United States mean are composed of all females between 15 and 55 years old in the 1990 5% Census extract excluding women living in mixed and non-MSAs (*sample size: 2,344,139*).

c. The four contact availability (*CA*) measures are defined in detail in the text. They are calculated using all observations in the 1990 5% Census extract (*12.2 million observations*). The lower panel reports the mean and standard deviation of these measures for all women in our sample (*sample size: 397,200*).

d. "Welfare" is a dummy variable that equals one if the woman receives public assistance. "Child present" is a dummy that equals one if the woman has some own children at home. "Number of kids" is the number of children ever born. "English fluency" equals zero for individuals who speak English "not well" or "not at all" and one for those who speak it "well" or "very well."

come from many areas. European (Eastern and Western), South Asian, Far Eastern, and Middle Eastern languages are all represented. There is also one African (Kru) and one Native American (Navajo) language group in our sample.

The language groups exhibit large variation in mean welfare participation. The lowest is Gujarathi speakers with only .5

TABLE II
SUMMARY STATISTICS BY LANGUAGE GROUP

Mean of:	Group size in sample	Group size in 5% census	% Welfare	% of High school drop outs	Age	% Married (spouse present)
Language:						
Spanish	237582	817239	7.5	49.69	31.94	47.11
Chinese	19434	57453	2.5	28.57	33.98	58.14
French	17448	79415	2.9	23.54	33.29	47.42
Tagalog	15552	15554	1.1	14.22	35.84	59.05
German	14574	75963	2.2	18.04	36.56	59.59
Italian	11565	60636	2.1	29.96	36.17	60.07
Korean	10417	28117	1.1	23.75	34.68	65.79
Vietnamese	7567	23612	11.0	45.43	31.57	48.96
Polish	5635	34173	2.0	20.83	36.98	59.66
Portuguese	5552	20948	2.8	47.15	33.62	60.55
Japanese	5438	19653	1.1	12.45	36.11	65.91
Hindi	4576	4579	0.9	19.25	33.66	72.92
Greek	4555	17517	1.6	27.95	35.51	60.62
Arabic	4073	15482	3.6	29.27	32.77	65.01
Thai	3234	9841	9.2	48.27	32.95	57.39
Persian	2905	9304	3.2	15.08	33.28	60.24
Russian	2776	10515	6.2	15.24	35.81	64.66
Creole	2608	7736	4.0	48.77	32.64	34.78
Hebrew	1983	6339	1.9	15.23	33.71	67.12
Armenian	1945	7030	11.5	29.15	35.16	62.37
Mon-Khmer	1919	6040	28.9	69.93	31.09	46.12
Gujarathi	1649	4879	0.5	22.50	34.00	72.47
Dutch	1438	7007	2.0	15.92	36.71	64.81
Hungarian	1248	6918	1.5	18.91	37.85	62.18
Yiddish	1209	8949	3.2	27.38	34.38	63.85
Rumanian	877	3041	4.2	30.79	34.70	61.46
Serbo-Croatian	868	3251	2.1	35.02	34.84	63.13
Ukrainian	836	4619	2.5	14.35	36.46	56.70
Miao	785	3666	33.1	76.05	29.91	61.66
Formosan	754	2173	0.7	17.90	35.77	63.26
Punjabi	751	2387	1.2	31.42	33.11	66.31
Swedish	743	3754	1.9	11.31	36.51	58.95
Kru	742	2378	2.6	11.59	31.70	57.41
Norwegian	562	4241	1.6	13.35	36.43	61.21
Penn. Dutch	551	5229	1.1	68.97	32.98	63.70
Ilocano	528	2064	0.9	30.68	35.01	56.63
Czech	515	4838	1.6	14.76	38.63	63.11
Croatian	445	2117	1.8	30.34	35.58	62.47
Slovak	398	4046	1.3	12.56	39.06	65.58
Lithuanian	350	2644	0.9	7.71	37.95	62.00
Navajo	329	7044	7.9	29.18	30.76	44.38
Finnish	284	3058	2.5	10.91	37.89	62.67

a. Data are composed of all females between 15 and 55 years old in 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).

b. Group Size in 5% Census is the number of individuals who speak the language at home in the entire 5% 1990 Census.

c. The language group Chinese includes the Chinese dialects of Cantonese, Yueh, and Min, but excludes Mandarin and other Chinese dialects. Tagalog is spoken in Manila and its adjacent Provinces, and Ilocano is spoken in northern Luzon in the Philippines. Mon-Khmer is spoken in Cambodia. Miao (also called Hmong) is a language spoken in the mountainous regions of Southern China and adjacent areas of Vietnam, Laos, and Thailand. Gujarathi and Punjabi are spoken on the Indian subcontinent. Formosan (also called Minnan) is the dialect of Chinese spoken on most of Taiwan. Kru is spoken in Nigeria. Navajo (also called Navaho) is the language spoken by a Native American people mainly living in Arizona, New Mexico, and southeast Utah.

percent of the Gujarathi women in our sample receiving welfare. Consistent with the low welfare use, they also have one of the highest marriage rates in our sample. Miao and Mon-Khmer speakers, on the other hand, have the highest levels of welfare recipiency. Around 30 percent of these women use welfare. They are also characterized by extremely high numbers of high school dropouts and tend to be younger.[26] The next highest welfare use is by the Armenian and Vietnamese speakers. Members of these four language groups are more likely to be refugees, which partly explains their high level of welfare recipiency.

III. EMPIRICAL RESULTS

III.1. Differences-in-Differences

Before discussing the basic results, it is useful to present a simple differences-in-differences calculation. Suppose that we split people into two groups: those from language groups with above and those from language groups with below median welfare use. We can also split people on the basis of contact availability: those with above and those with below median contact availability. The interaction of these two splits yields four groups. An individual may be from a high or low welfare-using group and live in a high or low contact availability area. Our empirical strategy in this case translates into a differences-in-differences estimation. In this simplification, taking the difference between low and high contact availability is the analogue of using language fixed effects. Similarly, the control for contact availability becomes the difference between low and high welfare groups. Finally, the interaction term becomes the difference of these differences.

Table III displays the diffs-in-diffs calculation for our data. Each panel contains nine numbers. Consider first Panels A and B. The first two columns and rows represent the mean of the dependent variable. For example, Panel A tells us that the mean welfare use of the low contact availability and low welfare group is 2.05 percent. The third row contains column-by-column differences of the first two rows. Similarly, the third column contains row-by-row differences of the first two columns. For example, Panel A shows that the difference between living in a high CA area and a low CA area is 2.84 percentage points for the high welfare group. The entry in the third column and row represents the diffs-in-diffs calculation. Standard errors are in parentheses.

26. We investigate the effects of dropping these two groups in Table V.

TABLE III
DIFFERENCES-IN-DIFFERENCES

Dependent variable: welfare participation

Panel A Differences PUMA level contact availability	Low CA	High CA	ΔCA	Panel B Differences MSA level contact availability	Low CA	High CA	ΔCA
Low welfare	0.0205	0.0226	0.0021	Low welfare	0.0200	0.0232	0.0032
LG	(0.0005)	(0.0006)	(0.0008)	LG	(0.0005)	(0.0006)	(0.0008)
High welfare	0.0645	0.0928	0.0284	High welfare	0.0687	0.0875	0.0188
LG	(0.0007)	(0.0008)	(0.0011)	LG	(0.0008)	(0.0008)	(0.0011)
ΔLG	0.0400	0.0702	**0.0263**	ΔLG	0.0487	0.0642	**0.0155**
	(0.0009)	(0.0010)	(0.0013)		(0.0009)	(0.0010)	(0.0013)

Panel C Ratios PUMA level contact availability	Low CA	High CA	CA_{high}/CA_{low}	Panel D Ratios MSA level contact availability	Low CA	High CA	CA_{high}/CA_{low}
Low welfare	0.0205	0.0226	1.1022	Low welfare	0.0200	0.0232	1.1624
LG	(0.0005)	(0.0006)	(0.0392)	LG	(0.0005)	(0.0006)	(0.0414)
High welfare	0.0645	0.0928	1.4399	High welfare	0.0687	0.0875	1.2734
LG	(0.0007)	(0.0008)	(0.0200)	LG	(0.0007)	(0.0008)	(0.0177)
LG_{high}/LG_{low}	3.1485	4.1130	**1.3064**	LG_{high}/LG_{low}	3.4350	3.7631	**1.0955**
	(0.0864)	(0.1094)	(0.0499)		(0.0935)	(0.1010)	(0.0419)

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).

b. Welfare participation is measured as the fraction of people receiving any form of public assistance.

c. People belonging to language groups with an average welfare use below the median are classified under "Low welfare *LG*" and the rest is classified under "High welfare *LG*." People-living in area-language cells for which the Contact availability is below the median are classified as "Low *CA*," and the rest is classified under "High *CA*." Contact availability measures are defined in detail in the text.

d. The boldface numbers in Panel A and B are the difference-in-difference estimates. The boldface numbers in Panel C and D are the ratio-of-ratios estimates. Standard errors are in parentheses.

In Panel A contact availability is measured at the PUMA level, whereas it is measured at the MSA level in Panel B. All estimates show a positive and significant effect for the diffs-in-diffs calculation. This illustrates that contact availability raises welfare use more for high welfare-using language groups. Focusing on Panel A, we see that the difference between a high and low CA area is .0021 percentage points for a low welfare group, while it is .0284 for a high welfare group. These two numbers are different by an order of magnitude. A similarly large difference is seen in Panel B.[27]

27. We also ran the diffs-in-diffs with demographic controls and PUMA fixed effects. This raised the diffs-in-diffs estimate to 0.0406 (Standard error: .0059) for

Panels C and D show that the results are not completely driven by functional form. One might argue that finding a higher effect of CA for higher language groups depends intimately on how "higher" is defined. A diffs-in-diffs calculation that is larger in levels may be smaller in percentages. Panels C and D show that the same results hold when, instead of differencing cells, we take ratios. For example in Panel C, high CA individuals from low welfare language groups are approximately 10 percent more likely to use welfare than low CA ones, while those from high welfare language groups are approximately 44 percent more likely.

III.2. Basic Results

Table IV displays the main results. We estimate a linear probability model for welfare recipiency in which the right-hand side includes fixed effects for each language group, fixed effects for each PUMA, demographic controls, a measure of contact availability (CA), and the interaction of CA with the mean welfare use of the individual's language group (see equation (3)).[28] The mean welfare use in the interaction term is taken in deviation from the global mean welfare use in the sample: $CA * (\overline{Welf}_k - \overline{Welf})$. This facilitates interpretation of the coefficient on the (noninteracted) *CA* measure. Since the *CA* measure varies only at the PUMA-language or MSA-language cell level, the standard errors are corrected to allow for group effects within PUMA-language cells or MSA-language cells.

The demographic controls include four education dummies, age, age squared, three race dummies, six marital status dummies, a dummy for single motherhood, a dummy for the presence of own children at home, as well as a control for the number of children ever born.[29] The first three sets of controls—race, education, and age—clearly belong in the equations. The second set of

the PUMA level specification and to 0.02406 (Standard error: .0107) for the MSA level specification. The demographic controls consist of three race dummies, a quadratic in age, four education dummies, six marital status dummies, a control for the number of children born, a dummy for the presence of a child at home, and a dummy for single motherhood. In subsection III.2 we discuss the choice of these controls.

28. These regressions are unweighted.

29. The six marital dummies are married with spouse present, married with spouse absent, widowed, divorced, separated, never married. In the regressions the omitted variable is married with spouse present. The four education dummies are high school dropout, high school graduate, some college, and college and beyond. In the regressions the omitted variable is college and beyond. The three race dummies are Black, White, and other, with other omitted from regressions.

controls—marital status, fertility, and single motherhood—are more endogenous. Networks may also affect welfare participation by affecting these variables. For example, women may be more likely to take up AFDC if networks increase the probability of single motherhood. Nevertheless, we include these variables as covariates, since they may also control for unobserved characteristics of individuals. Including them in the regression can only lead us to *underestimate* the effect of networks. Therefore, finding evidence of networks in spite of controlling for these variables, only strengthens our case.

In Table IV the covariates display the expected signs. Higher education and being non-Black decreases probability of welfare use. Being single, having more kids, and being a single mother all increase probability of welfare use. Because of the quadratic term, age has a positive effect on welfare use for women under 35, and a negative effect for women over 35.[30] The negative effect of having a child present is the only anomaly. However, the sum of the coefficients on child present and number of children present is positive ($-.0043 + .0145 = .0102$). Therefore, even if a woman moves from having zero to one child, the marginal impact is still positive.

Because we do not know a priori the reach of social networks, we present evidence for network effects using both contact availability at the PUMA level and at the MSA level. Columns (1) and (2) present estimates of network effects when we measure contact availability at the PUMA level. The first column shows that the coefficient on the interaction term is highly significant for the OLS regression. In column (2) we instrument the interaction term at the PUMA level with the interaction term at the MSA level.[31] We use this IV estimation to assess the alternative hypothesis that no network effects exist and that differential selection is the sole reason for finding a positive OLS coefficient. Under this alternative hypothesis, the OLS estimate is positive because of selection within MSAs and selection between MSAs, whereas the IV is only biased due to selection between MSAs. Hence, under the alterna-

30. The positive effect of age for women under 35 is slightly puzzling since we are controlling for number of children and children present. One would expect that if two individuals have had the same number of children, the younger one should be more likely to use welfare.

31. Moving within metropolitan areas is much easier than moving between them. Therefore, the IV should reduce any bias caused by choice of where to live. Evans, Oates, and Schwab [1992] have used this instrument to criticize standard tests for neighborhood or network effects, showing that the IV eliminates the strong effects estimated by OLS.

tive hypothesis, comparing the OLS with the IV estimate allows us to infer the relative magnitude of selection within MSAs to selection between MSAs. When we make this comparison using our estimates in Table IV, we find that selection between MSAs would be larger than selection within MSAs.[32] Because it is much easier to move within MSAs than between MSAs, one would have expected the exact opposite. Hence, we take the small difference between the OLS and IV estimate as evidence against the hypothesis that our results are completely driven by differential selection.

In column (3) we estimate network effects when contact availability is measured as the MSA level. These estimates are not affected by potential differential selection within MSAs, but contact availability measured at the MSA level may be a noisier measure of social networks than PUMA level contact availability. Also for these specifications, we continue to find positive and significant network effects.[33] The MSA results, therefore, produce

32. To understand this, consider the model under the alternative hypothesis of no network effects. To simplify notation, we suppress the control variables. Each variable is the residual of the regression of that variable on all the suppressed controls. We denote the MSA level interaction term by N_M and the PUMA level interaction term by N_P. One can always decompose N_P into a part that is explained by N_M and an error term: $N_P = \gamma N_M + \eta$ such that $E[\eta] = 0$ and $E[\eta N_M] = 0$. Under the alternative hypothesis, welfare recipiency (W) is solely determined by an error term: $W = \epsilon$. The bias in the OLS estimate can be decomposed into a part ($\hat{\rho}_M$) that is due to differential selection within MSAs and a part ($\hat{\rho}_P$) that is caused by differential selection between MSAs:

$$\hat{\alpha}_{OLS} = \frac{N'_P\epsilon}{N'_PN_P} = \frac{(\gamma' N'_M + \eta')\epsilon}{N'_PN_P} = \frac{\gamma' N'_M\epsilon}{N'_PN_P} + \frac{\eta'\epsilon}{N'_PN_P} \hat{=} \hat{\rho}_M + \hat{\rho}_P.$$

The IV estimate can be expressed as a function of $\hat{\rho}_M$ and the R^2 of the regression of N_P on N_M:

$$\hat{\alpha}_{IV} = \frac{\hat{N}'_P\epsilon}{\hat{N}'_P\hat{N}_P} = \frac{\gamma' N'_M\epsilon}{\hat{N}'_P\hat{N}_P} = \left(\frac{N'_PN_P}{\hat{N}'_P\hat{N}_P}\right)\frac{\gamma' N'_M\epsilon}{N'_PN_P} = \left(\frac{1}{R^2}\right)\hat{\rho}_M.$$

These two equations can be used to solve for the ratio of the bias due to differential selection within MSAs to the bias due to differential selection between MSAs:

$$\frac{\hat{\rho}_P}{\hat{\rho}_M} = \frac{\hat{\alpha}_{OLS} - \hat{\alpha}_{IV}R^2}{\hat{\alpha}_{IV}R^2} = \frac{0.1751 - 0.1636 \cdot 0.5963}{0.1636 \cdot 0.5963} = 0.795 < 1.$$

This indicates that self-selection between MSAs must be *greater* than self-selection within MSAs.

33. The smaller coefficients on the MSA level regressions should not be interpreted as the coefficients *dropping.* Recall that these are two different right-hand side variables.

TABLE IV
MAIN RESULTS

Dependent variable: Welfare participation

	(1)	(2)	(3)	(4)	(5)
CA measure:	Log PUMA	Log PUMA	Log MSA	—	—
Estimation technique:	OLS	IV	OLS	OLS	OLS
Contact availability*	.1751****	.1638****	.1444****	—	—
Mean welfare of *LG*	(.0258)	(.0281)	(.0282)		
Mean welfare of *LG*	—	—	—	.5353**** (.1280)	—
Mean welfare in PUMA	—	—	—	—	.6591**** (.0101)
Contact availability	.0024*** (.0009)	−.0020* (.0011)	−.0021 (.0015)	—	—
HS dropout	.0469**** (.0018)	.0477**** (.0018)	.0475**** (.0062)	.0446**** (.0033)	.0423**** (.0019)
HS graduate	.0162**** (.0011)	.0168**** (.0011)	.0165**** (.0021)	.0154*** (.0051)	.0135**** (.0011)
Some college	.0037**** (.0008)	.0040**** (.0008)	.0038**** (.0010)	.0016 (.0015)	.0025*** (.0008)
Single mother	.1947**** (.0044)	.1946**** (.0044)	.1947**** (.0083)	.2066**** (.0129)	.1953**** (.0048)
Child present	−.0043**** (.0012)	−.0041**** (.0012)	−.0042 (.0026)	−.0053** (.0021)	−.0039**** (.0012)
Number of children	.0145**** (.0005)	.0146**** (.0005)	.0146**** (.0010)	.0147**** (.0012)	.0144**** (.0006)
Married, spouse absent	.0405**** (.0028)	.0407**** (.0028)	.0407**** (.0085)	.0430**** (.0107)	.0387**** (.0030)
Widowed	.0403**** (.0044)	.0403**** (.0044)	.0405**** (.0047)	.0419**** (.0068)	.0436**** (.0051)
Divorced	.0183**** (.0026)	.0182**** (.0026)	.0182**** (.0048)	.0174** (.0070)	.0180**** (.0027)
Separated	.0830**** (.0040)	.0830**** (.0040)	.0831**** (.0092)	.0895**** (.0114)	.0831**** (.0043)
Never married	.0392**** (.0020)	.0394**** (.0020)	.0393**** (.0058)	.0443**** (.0085)	.0392**** (.0021)
Age	.0074**** (.0004)	.0074**** (.0004)	.0074**** (.0011)	.0073**** (.0004)	.0070**** (.0004)
$\text{Age}^2/100$	−.0105**** (.0005)	−.0105**** (.0005)	−.0105**** (.0014)	−.0102**** (.0007)	−.0100**** (.0006)
White	−.0054**** (.0012)	−.0057**** (.0012)	−.0059**** (.0018)	−.0126**** (.0028)	−.0060**** (.0013)

TABLE IV
(CONTINTUED)

	Dependent variable: Welfare participation				
	(1)	(2)	(3)	(4)	(5)
Black	.0069**	.0061*	.0058	.0060	−.0040
	(.0035)	(.0035)	(.0076)	(.0096)	(.0032)
PUMA F.E.	Yes	Yes	Yes	No	No
Language group F.E.	Yes	Yes	Yes	No	No
Adjusted R^2	.174	—	.174	.145	.163
Response to welfare shock	26.6%	24.2%	14.6%	115.2%	193.3%

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).

b. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. The Contact Availability (*CA*) measures are defined in detail in the text. The omitted education dummy is "College and more." The omitted marital status dummy is "Married, spouse present."

c. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22,543 cells) in columns (1) and (2), for group effects within MSA-language cells (6197 cells) in column (3), for group effects within language cells (42 cells) in column (4) and for group effects within PUMAs (1196 cells) in column (5). Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

d. Language fixed effects are 42 language dummies. PUMA fixed effects are 1196 dummies for the PUMAs represented in the sample.

e. "Mean welfare of *LG*" is expressed as a deviation from the sample mean (over all language groups).

f. The thought experiments underlying the response to the welfare shock and the response to the *CA* shock are explained in the text.

g. In the IV regressions, contact availability at the MSA level and the interaction of MSA level contact availability with mean welfare use in the language group are used as instruments for contact availability at the PUMA level and the interaction of PUMA level contact availability with mean welfare use in the language group. The hypothesis that in specification (2) these two instruments are jointly zero in the first stage for PUMA level contact availability is easily rejected: $F(2,395947) = 6247$ (p-value: .0000). A similar test for the first stage for the PUMA level interaction term is also rejected: $F(2,395947) = 1158$ (p-value: .0000). These F-statistics are corrected to allow for group effects within PUMA-language cells.

estimates quite similar to the PUMA specifications. We will continue to report them because they are the appropriate ones if one believes that networks operate within MSAs.

The coefficient on our networks measure (α) in columns (1) to (3) is hard to interpret. To provide a measure of the magnitude of the network effects, we ask how much network effects would magnify a policy shock affecting welfare participation. To incorporate welfare policies explicitly, we add the variable ξ to the model:

$$(4) \quad Welf_{ijk} = \xi + (CA_{jk} * \overline{Welf_k})\alpha + X_i\beta + \gamma_j + \delta_k + CA_{jk}\theta + \epsilon_{ijk}.$$

The variable ξ is a measure of policies that influence welfare participation. It is scaled such that a one percentage point increase in ξ leads to a one percentage point increase in welfare participation *in the absence of network effects.* However, the *equilibrium* increase in welfare participation exceeds the increase in ξ because networks result in accelerator effects that magnify the impact of the change. An increase in the policy variable ξ raises $\overline{Welf_k}$ which in turn raises each individual's welfare probability through the network effect, creating a feedback.[34] Algebraically, we average both sides of the equation for each language group and differentiate with respect to ξ, which gives us

$$\frac{d\overline{Welf}_k}{d\xi} = 1 + \overline{CA}_k * \frac{d\overline{Welf}_k}{d\xi}\alpha, \tag{5}$$

where $\overline{CA}_k$ is the mean of CA_{jk} within each language group. Solving equation (5) gives us each language group's change in welfare use in response to a policy change. Since the direct effect of the policy change is included, we subtract 1 from this formula to derive the extra change induced by networks:

$$1/(1 - \alpha\overline{CA}_k) - 1. \tag{6}$$

This expression implies that a policy that increases welfare use by one percentage point in the absence of networks actually increases welfare use of language group k by $1/(1 - \alpha\overline{CA}_k)$ percentage points. To get the response for the economy as a whole, we take the weighted mean of this over all the language groups. These computations show that networks may raise the responsiveness of welfare use to policy shocks by about 27 percent when we use the PUMA regressions and about 15 percent for the MSA regressions.

It is important to keep in mind that these estimates may understate the total network effects. Given the large number of positive omitted variable biases, we have taken a conservative approach.[35] Many of the variables that serve as controls in our regressions may proxy for networks in their own right. For example, we control for both neighborhood and language group fixed effects, both of which may proxy for networks. We ignore them because their impact likely includes other factors—personal

34. This calculation takes the model literally in the sense that it assumes that the actual level of welfare participation directly determines the quality of one's contacts. A broader interpretation of the model is that welfare participation is just a good proxy for the quality of one's contacts. In this case, a policy that increases overall welfare participation may not change the average quality of contacts and social networks may not multiply the response to the policy shock. We are grateful to a referee for pointing this out.

35. The conservative methodology reflects our goal of investigating the existence of networks ($\alpha > 0$), rather than quantifying them. Convincingly demonstrating existence raises enough difficulties that we defer precise quantification to later work.

characteristics—as well as networks. Moreover, we only consider networks operating between people speaking the same language at home. It is very reasonable to believe that nonlanguage-based networks also exist. One should, therefore, keep in mind that our quantitative estimates do not capture all aspects of social networks.

We have emphasized the importance of carefully isolating other possible sources of omitted variable biases. What estimates would we have produced if we had taken the naive approach and regressed own welfare participation on participation of the language group or of the neighborhood? Columns (4) and (5) present the results of this exercise. In column (4) we replace our contact availability measures (both the direct effect and interaction term) with the mean welfare use of the language group (excluding the individual). In column (5) we replace them with the mean welfare use in the PUMA (again excluding the individual). The magnitude of these results is striking. As before, we compute the response to a welfare shock implied by these estimates. The mean language group estimator predicts that shocks are amplified by 115 percent. The mean neighborhood estimator predicts that shocks are amplified by 193 percent. These contrast sharply with our estimates of 15 to 25 percent. The estimates underline the differences between our methodology and more naive ones. As noted above, these large differences in estimates may well be due to omitted variable biases. Also as noted above, they may simply be due to the fact that we estimate a smaller fraction of overall network effects. In either case, they underline how our estimator attempts to produce a lower bound.

In conclusion, this table establishes three main findings. First, we estimate positive and significant network effects in welfare use (column (1)). Second, after instrumenting for contact availability with MSA level availability and comparing the IV and OLS coefficients, we find it implausible that our results can be fully driven by differential selection (column (2)). Third, we find that our methodology produces quantitatively much more moderate estimates than naive estimates.

III.3. Specification Checks

How sensitive are our results to functional form and sample choice? In unreported regressions we have estimated a wide set of regressions to examine functional form issues. Probit and logit estimators produce positive and significant coefficients. Different measures of contact availability also all produce positive and significant measures.[36] We also added several other controls, none of which affected our results.[37] Details of these regressions can be found in our working paper [Bertrand, Luttmer, and Mullainathan 1998].

We now investigate the effect of changing samples. Table V displays the coefficients on the estimated network effects for different subsamples of our original data set. In Table II we saw that more than 55 percent of our sample were Spanish speakers, raising concerns that our results are driven completely by this one group. In row (2) we drop Spanish speakers and continue to find our results. The coefficient on $CA * (\overline{Welf}_k - \overline{Welf})$ is actually bigger in this subsample. In Table II we also saw that the Miao and Mon-Khmer had extremely high welfare recipiency. These outliers may also drive our results. Therefore, in row (3) we exclude the Miao and Mon-Khmer speakers from the regression. Again, we continue to find positive and significant effects.

By including all women between age 15 and 55, our usual sample draws from a wide band of ages. AFDC, however, is restricted to women with dependent children. Since women of childbearing ages are most likely to be eligible for this program, we examine different age groups. Row (4) includes only women between 15 and 35, and (5) includes only women between 15 and 45. Lowering the threshold in this manner does not affect the qualitative findings. They are smaller for the 15–35 group, but essentially the same for the 15–45 group. In row (6) we raise the lower threshold and focus on women between 25 and 55. Again, we find the same qualitative findings, but the coefficients are larger. Rows (4), (5), and (6), therefore, show that while the network effects are present in all age groups, they are slightly stronger for the older women.

We also vary the fertility and marital composition of our sample. Currently, we include all women in the relevant ages. In rows (7) and (8) we use all women with kids and single women with kids, respectively. We find that the effect is larger for all

36. We have tried the level version of our current log measure of contact availability measure, $(C_{jk}/A_j)(L_k/T)^{-1}$, the unadjusted fraction in the area that is in one's language group, C_{jk}/A_j, the log of this measure, and the unadjusted log of the number of people in one's area-language cell, $\ln(C_{jk})$.

37. We tried quartics of the CA measure as, a quartic in age, a dummy for having at least one child under the age of six, more education dummies, immigrant status, year of immigration, and English knowledge controls.

TABLE V
SAMPLE CHOICE
Reported: Coefficient on the Interaction Term (CA · Mean Welfare of LG)

Dependent variable: welfare participation	*CA* measure		
Change in sample:	Log PUMA (1)	Log MSA (2)	Sample size
(1) Original sample (as in Table IV)	.1751****	.1444****	397,200
	(.0258)	(.0282)	
(2) Spanish speakers excluded	.2266****	.2031****	159,618
	(.0230)	(.0278)	
(3) Miao and Mon-Khmer speakers excluded	.1664****	.1008***	394,496
	(.0259)	(.0391)	
(4) Only 15 to 35 year old women	.1265****	.1068****	235,536
	(.0239)	(.0233)	
(5) Only 15 to 45 year old women	.1698****	.1421****	332,357
	(.0253)	(.0266)	
(6) Only 25 to 55 year old women	.2173****	.1775****	292,025
	(.0309)	(.0365)	
(7) Only women with children	.2117****	.1690****	185,521
	(.0371)	(.0369)	
(8) Only single women with children	.1410*	.0730	38,115
	(.0728)	(.0704)	

a. The original sample is composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*). The sample sizes of the various subsamples are noted in the rightmost column.

b. All regressions are regressions of welfare participation on demographic controls, 42 language group fixed effects, 1196 PUMA fixed effects, contact availability, and contact availability interacted with mean welfare use by language group. Only the coefficient on the interaction term is reported.

c. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. The contact availability (*CA*) measures are defined in detail in the text.

d. Demographic controls include four education dummies, six marital status dummies, a White dummy, a Black dummy, a quadratic in age, a dummy for single mother, a dummy for the presence of own children at home, as well as a control for the number of kids ever born.

e. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22,543 cells) or MSA-language cells (6,197 cells), depending on which *CA* measure is used. Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

women with children. On the other hand, the effect is smaller for single women with children.[38]

III.4. Impact of Removing Controls on Estimates

If unobservable characteristics about individuals drove our results, one would expect that increasing the set of unobservable

38. One potential explanation for this is that knowledge about AFDC is more widespread than knowledge about other forms of assistance, e.g., Housing Assistance. For this reason, one might think that networks matter less since this information may already be known through other sources.

TABLE VI
SENSITIVITY OF RESULTS TO ADDITION OF CONTROLS
Reported: Coefficient on the Interaction Term (CA · Mean welfare of LG)

Dependent variable: welfare participation		
CA measure:	Log PUMA	Log MSA
Controls:	(1)	(2)
(1) Language F.E.	.2337****	.1301***
	(.0281)	(.0428)
(2) Language and PUMA F.E.	.2165****	.1714****
	(.0279)	(.0321)
(3) (2) + exogenous controls	.2106****	.1654***
	(.0277)	(.0314)
(4) (3) + education	.2028****	.1606****
	(.0277)	(.0359)
(5) All controls	.1751****	.1444****
	(.0258)	(.0282)

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).
b. All regressions include contact availability. Only the coefficient on the interaction term is reported.
c. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. The contact availability (*CA*) measures are defined in detail in the text.
d. Exogenous controls include a White dummy, a Black dummy, and a quadratic in age. Education is composed of four education dummies. In addition to the exogenous controls and education, all controls include marital status, child present, and number of children. Marital status is composed of six marital status dummies. Child present is a dummy for the presence of own children at home and number of kids is the number of kids ever born.
e. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22,543 cells) or MSA-language cells (6,197 cells), depending on which CA measure is used. Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

characteristics by treating observable characteristics as unobservable would have a large impact on the estimate of network effects. We investigate this in Table VI. We begin with a sparse regression that has only the contact availability measure, language fixed effects, and the interaction between *CA* and mean welfare of the language group in row (1). The coefficients in row (1) are higher in the PUMA specification and lower in the MSA specification than the corresponding coefficients in Table IV. We then add PUMA fixed effects in row (2). We find that adding PUMA fixed effects does indeed lower the coefficient in the PUMA-level regression. In the MSA level regression, however, the addition of PUMA fixed effects actually *raises* the coefficient. In row (3) we add controls that are clearly exogenous: age, age squared, a White dummy, and a Black dummy. The coefficient hardly changes. In row (4) we add education controls. Again, the coefficient decreases slightly. In row (5) we add the remaining controls: the number of children ever born, marital status dummies, a dummy for single motherhood, and a dummy for whether a child is present at home. Because these controls are likely to be a function of network effects themselves, it comes as no surprise that they lower the estimated coefficient.[39] Although these controls decrease the coefficient by more then the education and exogenous controls, the drop in the coefficient is not very dramatic. In conclusion, inclusion of the education controls and variables such as age, does not affect the coefficient. On the other hand, inclusion of the potentially endogenous marital status, fertility, and single motherhood controls does change the coefficient.[40]

III.5. Network Mechanisms

What are the mechanisms through which the networks operate? We have so far demonstrated that conditional on marital status, fertility, and single motherhood, networks influence welfare use. In this section we ask whether networks affect these fertility and marital status decisions. We already saw a hint that networks might influence these variables in Table VI. When we added marital status and fertility controls, the estimate dropped significantly (rows 4 and 5). In Table VII we analyze this more explicitly. Columns (1) and (2) use single motherhood as the dependent variable, and columns (3) and (4) use a dummy for being married as the dependent variable. For the regressions with single motherhood, the coefficient on the interaction term is significant and positive, indicating a higher likelihood of being a single mother for women who have many contacts in a high welfare-using language group.[41] Similarly, columns (3) and (4) show that these women have a significantly lower probability of being married. These results combined with the previous ones tell

39. Given that these controls are likely to be endogenous, why are they included as regressors? Their inclusion biases the results down. Consequently, their inclusion can at best strengthen our case, since they make it less likely that we find network effects.

40. An alternative to examining the effect of adding controls is to estimate explicit sorting equations. In our working paper we examined whether demographic variables differentially predicted sorting as a function of a language group's welfare use [Bertrand, Luttmer, and Mullainathan 1998]. The results there also showed no consistent pattern of differential sorting that would bias our results.

41. However, the quantitative impact of this channel is quite small. The coefficient on single motherhood in the original regression (Table IV, column (1)) is .1947. Our measure of the impact of networks on single motherhood is .0896 (Table VII, column (1)). Multiplying these together implies that the impact of networks as they operate through single motherhood is .0174. This is only a tenth of the total measured impact of networks, .1751 (Table IV, column (1)).

TABLE VII
NETWORK MECHANISMS

Dependent variable:	*Single mother*		*Married*		*Single mother · Welfare participation*	
	(1)	(2)	(3)	(4)	(5)	(6)
CA measure:	Log PUMA	Log MSA	Log PUMA	Log MSA	Log PUMA	Log MSA
Contact availability* Mean welfare of *LG*	.0896****	.0493***	−.0621***	−.0434*	.0611****	.0362***
	(.0141)	(.0176)	(.0207)	(.0408)	(.0111)	(.0135)
Contact availability	−.0038***	−.0049*	.0118****	.0087****	−.0028****	−.0044****
	(.0009)	(.0012)	(.0014)	(.0021)	(.0006)	(.0009)
HS dropout	.0550****	.0555****	.0317****	.0328****	.0371****	.0376****
	(.0021)	(.0055)	(.0033)	(.0072)	(.0016)	(.0054)
HS graduate	.0382****	.0385****	.0363****	.0370****	.0183****	.0185****
	(.0016)	(.0032)	(.0028)	(.0038)	(.0009)	(.0025)
Some college	.0299****	.0301****	−.0141****	−.0137****	.0092****	.0094****
	(.0014)	(.0022)	(.0026)	(.0030)	(.0006)	(.0015)
Age	.0230****	.0230****	.0913****	.0915****	.0093****	.0093****
	(.0006)	(.0021)	(.0007)	(.0022)	(.0005)	(.0016)
$\text{Age}^2/100$	−.0319****	−.0319****	−.1095****	−.1095****	−.0130****	−.0131****
	(.0000)	(.0000)	(.0010)	(.0027)	(.0000)	(.0000)
White	−.0205****	−.0208****	.0072****	.0069**	−.0096****	−.0098****
	(.0016)	(.0039)	(.0022)	(.0032)	(.0009)	(.0021)
Black	.0726****	.0717****	−.1152****	−.1156****	.0131****	.0124**
	(.0044)	(.0154)	(.0057)	(.0199)	(.0025)	(.0059)
PUMA F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Language group F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.062	.062	.242	.242	.054	.054

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*).

b. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. Single mother is a dummy variable that equals one for single mothers, and married is a dummy variable that equals one for married women. The Contact availability (*CA*) measures are defined in detail in the text. The omitted education dummy is "College and more."

c. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22,543 cells) or MSA-language cells (6,197 cells), depending on which *CA* measure is used. Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

d. Language fixed effects are 42 language dummies. PUMA fixed effects are 1196 dummies for the PUMAs represented in the sample.

e. "Mean welfare of *LG*" is expressed as a deviation from the sample mean (over all language groups).

us that welfare networks operate through fertility and marital status decisions as well as by increasing the propensity to receive welfare *conditional* on marital status and fertility.

In columns (5) and (6) we attempt to refine our welfare measure. Currently, it includes all forms of public assistance. This could include many types of aid besides Aid to Families with Dependent Children. To turn our focus more toward AFDC, we construct a proxy for AFDC recipiency by interacting single motherhood with public assistance recipiency. We find significant and positive network effects on this proxy. The coefficient is smaller than our previous estimates, suggesting that part of earlier estimates of network effects operates through welfare programs other than AFDC. However, the estimates of network effects for just AFDC are still important and very significant.

III.6. Distribution of Effects

Table VIII analyzes the various determinants of the strength of network effects. This serves two purposes. First, it offers a reality check for our estimates. We have strong expectations about how some variables should affect networks. Second, such a breakdown can provide interesting information about what catalyzes networks.

In the first row we estimate how the strength of the network effect varies with immigrant status and length of stay in the United States.[42] We find that network effects are significantly stronger for foreign-born women who have recently entered the United States. This foreign-born effect tends to diminish with the number of years since entry.[43] Both these findings are consistent with our intuition. First, newcomers are likely to be more engaged with their ethnic group. Our measure of networks is better for newcomers since ethnicity plays a larger role in their friendship. Second, information about welfare provided by networks should be most important for newcomers to this country. They will be the ones who will know the least about the myriad welfare programs in the United States. Moreover, the decline in the effect with time spent in the United States also provides some suggestive evidence against selection driving our results. If those who stay in the United States have had greater time to sort themselves, we would expect the effects to *rise,* not fall, with time since entry.[44]

42. The first three rows also include a foreign-born dummy, year of immigration dummies, and knowledge of English dummies as controls. Moreover, in addition to a third-order interaction term, each row also contains all relevant second-order interaction terms.

43. In fact, at the average number of years since entry for foreign-born individuals (13.3 years), there is about no difference left in network effects between foreign-born and U. S.-born women.

44. This is only suggestive since one might argue that the selection on where a migrant enters itself is severe.

TABLE VIII
DISTRIBUTION OF NETWORK EFFECTS

Dependent variable: Welfare participation		
CA measure: *Distributional effect based on:*	Log PUMA *CA* (1)	Log MSA *CA* (2)
(1) *Foreign born and year of immigration*		
$(CA_{jk} * \overline{Welf_k})$	.1537****	.0997***
	(.0266)	(.0386)
$(CA_{jk} * \overline{Welf_k})$ * foreign born	.1103**	.1270*
	(.0479)	(.0696)
$(CA_{jk} * \overline{Welf_k})$ * years since entry	−.0076***	−.0067*
	(.0025)	(.0036)
(2) *Own level of English*		
$(CA_{jk} * \overline{Welf_k})$	.2173****	.1825****
	(.0404)	(.0422)
$(CA_{jk} * \overline{Welf_k})$ * own English fluency	−.0755**	−.0636**
	(.0302)	(.0359)
(3) *Mean level of English of available contacts*		
English fluency in area-language cell	.0214***	.0598****
	(.0070)	(.0184)
$(CA_{jk} * \overline{Welf_k})$	.3412****	.2969****
	(.0659)	(.0745)
$(CA_{jk} * \overline{Welf_k})$ * English fluency in area-language cell	−.3330****	−.2981***
	(.0830)	(.1068)
(4) *Generosity of state AFDC benefits*		
$(CA_{jk} * \overline{Welf_k})$	−.0022	.0429
	(.0760)	(.0982)
$(CA_{jk} * \overline{Welf_k})$ * state AFDC generosity	.5079**	.1830
	(.2531)	(.3176)

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who do not speak English at home and whose language group counts at least 2000 individuals in the 1990 Census 5% extract. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 397,200*). In regression (4) only individuals living in the contiguous 48 states and Alaska are included in the sample because our AFDC benefit level variable is only available for these states (*sample size: 393,315*).

b. All regressions are regressions of welfare participation on demographic controls, 42 language group fixed effects, 1196 PUMA fixed effects, contact availability, and contact availability interacted with mean welfare use by language group. In each of the four specifications the interaction term, contact availability, and welfare use by language group are interacted with an additional variable. In other words, all relevant second-order interaction terms are included. Only selected coefficients are reported.

c. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. The Contact availability (*CA*) measures are defined in detail in the text. Demographic controls include four education dummies, six marital status dummies, a white dummy, a black dummy, a quadratic in age, a dummy for single mother, a dummy for the presence of own children at home as well as a control for the number of kids ever born. Language fixed effects are 42 language dummies. PUMA fixed effects are 1196 dummies for the PUMAs represented in the sample.

d. Foreign born is a dummy variable that equals one if the individual was foreign born (Mean: 0.63; Std.: 0.48). Year since entry is a variable that measures how many years a foreign-born individual has resided in the United States. It equals zero for U. S. born individuals (mean: 8.4; Std.: 10.0). The variable "own English fluency" equals zero for individuals who speak English "not well" or "not at all" and one for those who speak it "well" or "very well" (Mean: 0.77; Std.: 0.42). English fluency in an area-language cell is measured by the fraction of people for that language group living in that area who speak English "well" or "very well" (Mean: 0.77; Std.: 0.42). State AFDC generosity is measured by 12 times the maximum monthly AFDC payments in 1990 to a family of three divided by 52 times the average weekly earnings in manufacturing in that state. (Source: Green Book [1993] for AFDC benefits and U. S. Department of Labor for weekly earnings. Mean: 0.25 Std.: 0.09.)

e. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22,543 cells) or MSA-language cells (6,197 cells), depending on which *CA* measure is used. Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

Rows (2) and (3) analyze how the strength of network effects varies with English knowledge. Row (2) studies whether networks are more important for individuals more fluent in English, where English fluency is defined as speaking English "well" or "very well." We find that network effects are weaker for people speaking better English. Again, this matches intuition. Very interestingly, network effects stay large for women who speak good or excellent English. The estimated coefficient on $CA_{jk} * \overline{Welf}_k$ is two-thirds as large for women who speak good or excellent English as for women who do not. Network effects based on language spoken at home seem to be strong even for individuals who are conversant in English. This likely reflects the fact that even fluent English speakers prefer to associate with others who speak their native tongue.

Row (3) shows the effect of the English fluency of contacts.[45] Two opposing forces may be at play. On the one hand, increased English fluency makes it more likely that potential contacts can help in navigating the welfare system, suggesting an increased network effect. On the other hand, increased English fluency of contacts may reflect the fact that these contacts are of higher "quality." They may thus be less likely to provide information about welfare, and more likely to provide information about job opportunities. This effect suggests a negative impact of mean English fluency. Row (3) demonstrates a negative effect, supporting this last story. Increased English fluency of one's contacts *reduces* network effects.[46]

Finally, in row (4) we investigate how the strength of the network effect varies with generosity of AFDC benefits. We measure generosity as the maximum state annual benefits for a household of three divided by the state mean annual manufacturing sector wage in 1990. Again, generosity can affect networks either way. Increased generosity might make people more knowledgeable through sources other than networks. On the other hand, increased generosity may catalyze networks by making friends more eager to inform about welfare. We find that network effects strengthen with welfare generosity.

45. Mean English fluency of contacts is defined as the proportion of *our sample* in that language group and in that area who speak English either well or very well.

46. One might be concerned by how small network effects are when the mean English fluency of a language group in an area is high. An implication of row (3) is indeed that there are no network effects if all the members of a language group in an area speak good or excellent English. This last result raises the possibility of an alternative interpretation for our results based on a "bureaucratic channel." We extensively address this concern in subsection III.7.

III.7. The Bureaucratic Channel

In the previous sections we have attempted to deal with potential omitted variable biases in our regressions. Even in the absence of these biases, however, an alternative explanation potentially drives our results. The heavy concentration of a high welfare-using language group in an area may lead the welfare office in that area to hire a social worker who speaks that language. Individuals in that language group and area will find the administrative procedures to access welfare less burdensome and are thus more likely to participate. We refer to this as a "bureaucratic channel."[47] This channel also predicts a positive coefficient on the interaction term between contact availability and mean welfare use of a language group.

We investigate this possibility by focusing on Spanish speakers and exploiting differences in their country of origin. Suppose that, among the Spanish speakers, people who share the same country of origin are more likely to be in contact with each other. One can then estimate a regression similar to equation (3) but where CA and $\overline{Welf}_h$ are measured by country of origin rather than by language and where one replaces language group fixed effects by fixed effects for each country of origin h. We chose Spanish speakers for two reasons. First, they are by far the biggest language group in our sample, and this becomes essential when looking within groups. Second, they have a diverse background, with Spanish speakers hailing from many parts of the world. In contrast, many of the other language groups are extremely isolated geographically.[48]

Since country now proxies for contacts *within* Spanish speakers, the network effects model continues to predict a positive and significant coefficient on the $CA * \overline{Welf}_h$ term.[49] The bureaucratic channel model, on the other hand, predicts no effect. The relevant variables that determine whether a welfare office will hire a social worker fluent in Spanish are the concentration of Spanish speakers and the welfare proneness of the Spanish speakers in an area. Both of these variables—concentration of Spanish and mean welfare use of Spanish in an area—are constant within a local

47. We are grateful to Aaron Yelowitz for suggesting this alternative interpretation to us.
48. For example, Gujarathi speakers are by and large confined to only one state in India.
49. In other words, the availability of Spanish-speaking contacts in one's local area that share the same country of origin increases welfare participation if individuals from that country are on average high welfare users.

area and are thus fully captured by the area fixed effects. The use of country of origin for Spanish speakers thus allows us to distinguish between the two models.

The data used consist of women in the original data set who speak Spanish at home. We further restrict the sample to include only those whose ancestry is classified as Hispanic by the Census and whose ancestry can be linked to a specific country.[50] These exclusions make the sample smaller than the set of all Spanish speakers. In the end, we are left with 24 different groups of hispanic origin and 202,990 observations.

Table IX displays our results. Columns (1) and (2) are equivalent to columns (1) and (5) in Table IV. The estimated coefficient on the interaction term $CA * (\overline{Welf}_h - \overline{Welf})$ is positive and significant both at the PUMA level and the MSA level. As stated, this finding is consistent with network effects, but harder to reconcile with the bureaucratic channel explanation because each regression in Table IX includes PUMA fixed effects that capture the differential accommodation of Spanish speakers between local welfare offices. The magnitudes of effect estimated in these regressions are similar to the magnitudes computed for Table IV. In conclusion, these results support networks and do not support a bureaucratic channel. They are also of independent interest because they use a different variable—country of origin—to implement the same methodology.

IV. Conclusion

Evidence on the existence of network effects is of great importance for both theory and policy. Theorists in many fields are beginning to incorporate social networks into their models. Finding evidence of network effects increases the practical relevance of such models. From a policy point of view, optimal welfare policy can look very different in the presence of networks. Microestimates of the welfare participation response to an increase in benefits can be too low since networks can increase elasticities through multiplier effects. Similarly, the benefits of job training and placement programs may extend beyond the individuals directly being helped. Evidence for network effects also argues for

50. We use the Hispanic variable in the 1990 Census. For example, individuals who report Latin America as their Hispanic origin are excluded from the sample since this is not a specific country.

TABLE IX
HISPANIC SAMPLE

Dependent variable: welfare participation		
	(1)	(2)
CA measure:	Log PUMA	Log MSA
Estimation technique:	OLS	OLS
Contact availability * Mean welfare of CG	.1238****	.0871****
	(.0092)	(.0163)
Contact availability	.0017**	.0021
	(.0007)	(.0014)
Demographic controls	Yes	Yes
PUMA F.E.	Yes	Yes
Country of origin F.E.	Yes	Yes
Adjusted R^2	.197	.197
Response to welfare shock	31.8%	16.0%

a. Data are composed of all women between 15 and 55 years old in the 1990 Census 5% extract who speak Spanish at home and are of Hispanic origin. Women living in mixed MSAs and non-MSAs are excluded from the sample (*sample size: 202,990*).

b. The contact availability (*CA*) measure is defined as

$$CA_{jh} = \ln\left[\left(\frac{C_{jh}}{A_j}\right)\bigg/\left(\frac{L_h}{T}\right)\right],$$

where C_{jh} is the number of Spanish speakers from country of origin h in area j, A_j is the number of people in area j, L_h is the number of Spanish speakers from country of origin h, and T is the total number of people in the United States. The sample mean of CA_{jh} is 1.90 at the PUMA level and 1.57 at the MSA level.

c. "Mean welfare of CG" is the mean welfare by country of origin. It is expressed in deviation from the sample mean.

d. Country of origin fixed effects are 24 country of origin dummies. PUMA fixed effects are 1179 dummies for the PUMAs represented in the sample.

e. Welfare participation is a dummy variable that equals one if the individual receives any form of public assistance. Demographic controls include four education dummies, six marital status dummies, a White dummy, a Black dummy, a quadratic in age, a dummy for single mother, a dummy for the presence of own children at home, as well as a control for the number of kids ever born.

f. Heteroskedasticity-consistent standard errors are in parentheses. They are corrected to allow for group effects within PUMA-country of origin cells (9823 cells) and MSA-country of origin cells (2549 cells), depending on which *CA* measure is used. Asterisks indicate significance levels: * is 10 percent, ** is 5 percent, *** is 1 percent, **** is .1 percent.

g. The response to welfare shock calculation is explained in the text.

the importance of housing reallocation and desegregation programs.

Empirical work, however, has found it difficult to distinguish networks from omitted variable bias. People with unobserved characteristics that increase welfare participation may disproportionately live in high welfare participation areas. Hence, the observation that neighborhood welfare participation rates are correlated with individual welfare participation may simply reflect omitted personal or neighborhood characteristics rather than a causal relationship.

In this paper we use information on language spoken at home to circumvent these identification problems. People tend to interact with others from their own language group. Hence, persons who live in areas with many of their own language group will have a larger pool of available contacts. They are thus more likely to be influenced by their language group. Rather than investigating the direct effect of being surrounded by one's language group, we investigate the differential effect. We ask: does increased contact availability raise welfare use more for individuals from high welfare language groups? In support of network effects, we find evidence for this differential effect of contact availability. We find highly significant and positive coefficients on the interaction between contact availability and mean welfare participation of one's language group.

We have used language to proxy for the structure of *within*-neighborhood contacts. This technique has allowed us to deal with many of the standard biases in the existing literature. We have investigated the existence of other omitted variable biases. Our results show that social networks seem to strongly influence welfare participation.

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