NBER WORKING PAPER SERIES

NETWORK EFFECTS AND WELFARE CULTURES

Marianne Bertrand
Erzo F.P. Luttmer
Sendhil Mullainathan

Working Paper 6832
http://www.nber.org/papers/w6832

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 1998

We thank Kate Baicker, Amber Batata, Sandy Black, George Borjas, David Cutler, Alan Durell, Drew Fudenberg, Ed Glaeser, Kei Hirano, Matt Kahn, Jeff Kling, Ellen Meara, Doug Staiger, Jeff Wurgler, Aaron Yelowtiz, and participants of the Harvard labor and Urban Seminars for helpful discussions. We are especially gratefully to Larry Katz and Caroline Minter-Hoxby for their insightful comments. The views expressed here are those of the author and do not reflect those of the National Bureau of Economic Research.

Network Effects and Welfare Cultures
Marianne Bertrand, Erzo F.P. Luttmer,
and Sendhil Mullainathan
NBER Working Paper No. 6832
December 1998
JEL No. D83, H53, I3, R20

ABSTRACT

This paper empirically examines the role of social networks in welfare participation. Social theorists from across the political spectrum have argued that network effects have given rise to a culture of poverty. Empirical work, however, has found it difficult to distinguish the effect of networks from unobservable characteristics of individuals and areas. We use data on language spoken to better infer an individual's network *within* an area. Individuals who are surrounded by others speaking their language have a larger pool of available contacts. Moreover, the network influence of this pool will depend on their welfare knowledge. We, therefore, focus on the differential effect of increased contact availability: does being surrounded by others who speak the same language increase welfare use more for individuals from high welfare using language groups? The results strongly confirm the importance of networks in welfare participation.

We deal with omitted variable bias in several ways. First, our methodology allows us to include local area and language group fixed effects and to control for the direct effect of contact availability; these controls eliminate many of the problems in previous studies. Second, we instrument for contact availability in the neighborhood with the number of one's language group in the entire metropolitan area. Finally, we investigate the effect of removing education controls. Both instrumentation and removal of education controls have little impact on the estimates.

Marianne Bertrand
Department of Economics
Princeton University
Princeton, NJ 08544
and NBER
mbertran@princeton.edu

Erzo F.P. Luttmer
Harris School of Public Policy
University of Chicago
Chicago, IL 60637
luttmer@uchicago.edu

Sendhil Mullainathan
Department of Economics
MIT
Cambridge, MA 02143
and NBER
mullain@mit.edu

1 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 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

[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.

[3]See Banerjee(1992), Bikhchandani, Hirshleifer and Welch(1992), Bulow and Klemperer (1994) and Ellison and Fudenberg(1993,1995) for examples of work on information cascades and social learning. Bénabou(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), Moffitt (1983) and Nechyba (1996) are examples from the Labor and Public Economics literature.

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 U.S. 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 that 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 *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

[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% of them list at least one Armenian, and 35.6% list all Armenians. Asked about their other friends, more than 78% 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 US who speak that language at home.

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

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% 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.

Can our findings be explained by factors other than networks? Since contact with one's language group is itself a choice variable, omitted variable biases may again arise. Our methodology allows us to control for local area fixed effects, language group fixed effects and (since we focus on the interaction term) the direct effect of contact availability. This eliminates 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.

We investigate any remaining omitted variable biases by (i) instrumenting and (ii) dropping controls. Individuals living in a metropolitan area containing few of their language group will find it much more difficult to self-segregate. Therefore, the number of one's language group in one's metropolitan area provides a potential instrument. If self-segregation caused our results, then we would expect the coefficient to fall when we instrument. In contrast, the point estimate hardly changes.[7] In fact, our calculations indicate that self-segregation between MSAs must be *greater* than self-segregation within MSAs for it to explain our results. In a similar vein, we investigate the effect of dropping important controls. Suppose selection on unobserved characteristics is similar to selection on observed ones. Then the change in coefficients when we drop controls provides information about the importance and sign of the omitted variable bias. When we perform this exercise by dropping the education dummies, we find that the coefficients do not change.[8] Both of these techniques have previously been used by researchers to argue *against* the existing evidence for network effects. For example, Evans, Oates and Schwab (1992) instrument for neighborhood outcomes with metropolitan characteristics. They find that this eliminates the strong positive correlation between individual and neighborhood outcomes. Similarly, many have pointed out that adding education or other important controls significantly reduces estimated network effects.[9] Therefore, finding that our results are robust to these criticisms confirms the importance of looking within neighborhoods to disentangle network effects from unobservable factors.[10]

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

[7]Moving within metropolitan areas is much easier than moving between them. We, therefore, expect more sorting within metropolitan areas than between them. The reduced endogeneity implies that instrumenting with MSA contact availability will reduce any bias caused by choice of where to live. The difference between the OLS and IV estimates provides information about the sign and size of this bias.

[8]We also estimate an explicit sorting equation. We find that individuals sort into high and low contact availability areas based on observables. However, we find no systematic support for differential selection on the basis of the mean welfare use of the language group.

[9]Murphy and Topel (1990) have made this argument in the literature on inter-industry wage differentials. They note that the removal of marital status and other controls inflates estimated industry wage differentials significantly, and use this evidence to argue that the estimated differentials are largely driven by omitted variable biases.

[10]In section 4.8, we discuss the possibility that a bureaucratic channel, rather than network effects, drive our results. Welfare offices may better serve the needs of non-English speakers when many such people use welfare. To investigate this possibility, we look at ethnic variation within language group, which allows us to control for any bureaucratic externalities. We find qualitatively similar results despite these controls.

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. Finally, we investigate any remaining omitted variable biases by instrumenting and exploring selection on observables. We believe that these innovations represent significant progress on the difficult problem of distinguishing network effects from unobserved differences between individuals, areas and groups.

The rest of the paper is organized as follows. In section 2, we discuss the empirical strategy. The data is described in section 3. In section 4, we present the results and specification checks. Section 5 concludes.

2 Empirical Methodology

To explain our methodology, 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 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.[11] 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 two similar omitted variable biases. (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 $\hat{\alpha}$.[12] 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.

Even randomized experiments may not solve these problems. In the *Gautreaux* experiment, individuals were assigned to neighborhoods in Chicago. Rosenbaum (1995) contends that this assignment was in essence random. Even if one believes this, the randomization does not account for omitted neighborhood characteristics. Rosenbaum finds that women allocated to better neighborhoods experience better outcomes. This, however, does not provide unbiased evidence for networks

[11] 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 a strong correlation between individual and mean neighborhood outcomes.

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

since these neighborhoods may be closer to jobs, have more lenient welfare offices or provide better schooling. While Rosenbaum provides useful evidence about the importance of neighborhood effects, it is hard to learn about networks from his paper.

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 *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:[13]

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

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 correlated with $\overline{Welf}_{(-1)k}$.[14] (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 an 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

[13] 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.

[14] 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.

two previous equations. In equation (1), one can include ethnic fixed effects, while in equation (2), one can include neighborhood fixed effects.[15] 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.[16] We measure $Netw_{ijk}$ using the number of people the individual interacts with in combination with the attitudes and knowledge of those people towards welfare. Thus our network measure includes "quantity" and "quality" of contacts. If interactions occur mainly within language groups, we can write:

$$Netw_{jk} \approx \left(\begin{array}{l} \text{\# of people} \\ \text{from language} \\ \text{group } k \text{ who} \\ \text{live in area } j \end{array} \right)_{jk} \times \left(\begin{array}{l} \text{Welfare knowledge} \\ \text{and attitudes of} \\ \text{others from} \\ \text{language group} \\ k \text{ who live in area } j \end{array} \right)_{jk}$$

The number of people from language group k living in area j measures *contact availability*, denoted by CA_{jk}, or sometimes CA for simplicity.[17] 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

[15] Borjas (1995) investigates the effect of adding (tract level) neighborhood fixed effects to an ethnic capital regression. He finds that the coefficient drops significantly when neighborhood fixed effects are added. Combined with the evidence he presents on segregation of ethnic groups, he concludes that this provides evidence that ethnic capital acts through neighborhoods. With respect to local networks, the results of this paper are harder to interpret. Since he uses tract level data, he has approximately 26 people in each tract. Once one focuses on ethnics, this number becomes quite small, making it hard to compute local contact availability measures with any degree of accuracy.

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

[17] 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 U.S. that speaks the language. Our results are insensitive to this choice, but the alternative measure has several nice properties. For example, it equals one if people are distributed uniformly.

$\overline{Welf}_{(-i)jk}$. 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.[18] We, therefore, estimate:[19]

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

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. 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.

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

[18] We do not use $\overline{Welf}_{(-i)k}$, the mean welfare use of the whole language group minus individual i, because, given sample sizes, removing any one individual does not affect the overall mean.

[19] Though $Welf_{ijk}$ 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 (see Table A1 in the Appendix).

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 section 4.2, the comparison of the IV and OLS estimates leads us to believe that our results cannot completely be explained by differential selection.

3 Data

We use the 5% 1990 Census Public Use Micro Sample (PUMS). The two most precise geographic indicators in this data are the Public Use Microdata Area (PUMA) and the Metropolitan Statistical Area (MSA).[20] 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.[21] For the aggregate counts by

[20]There is a 1970 Census 1% PUMS which matches individuals by tracts and provides information about that tract matched from the 15% Census. We did not use that data for two reasons. First, in the 1% sample, our sample size would be only 1/5 as large. Second the 1% match does not provide us with matched language (or even ethnic) breakdowns. We would be forced to compute our measures from within the 1% sample. Since each tract has approximately 26 people, and most of these speak only English, the resulting measures would be very noisy. See Borjas (1995) for details.

[21]It would also be nice to have information about other speakers of a language, e.g. second generation immigrants who only speak English at home but are still conversant in the home tongue. Of course, since these people are less

MSA or PUMA of the number of people in a language group, we sum this variable across the entire 5% 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 U.S. from that language group.[22] In most specifications we use the log of this ratio.[23] 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 that belong to language group k, A_j is the number of people that live in area j, L_k is the total number of people in the U.S. that belong to language group k, and T is the total number of people in the U.S.

We divide by the language group's proportion in the U.S. because it instills the measure with several nice properties. For example, if individuals are uniformly distributed across areas, the measure would equal one for all people.[24] The results are insensitive to this division.[25]

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.[26] 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 2,000 people sampled in the 5% PUMS, which represents 400,000 people in the United States.[27] The rationale

likely to have strong ties to their ethnic group, this omission is likely not too serious.

[22] Areas are either PUMAs or MSAs.

[23] We extensively check the robustness of our results to the choice of this measure. See section 4.3.

[24] Or zero, once we take logs.

[25] In the log formulation, dividing by the language group's proportion in the U.S. does not affect the results since the divisor is absorbed by the language group fixed effects.

[26] Individuals in our sample may also speak English but they need to speak a language other than English at home.

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

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). We test the sensitivity of these sample selection criteria in Section 4.3.

The variable "welfare use" is a dummy variable that equals one if the individual received any income from public assistance (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% PUMS, and who live in a single MSA.

3.1 Summary Statistics

Table 1 summarizes the main variables. The women in our sample resemble the average U.S. women of the same age except in three respects. First, 5.8% of the women in our sample receive welfare whereas this figure is only 4.7% for the U.S. 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% versus 22%).[28] 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 2, we give selected summary statistics for each of the 42 language groups. The most striking fact is that more than 50% of our sample speak Spanish.[29] The remaining languages 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 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-Kmer speakers, on the other hand, have the highest levels of welfare recipiency. Around 30% of these women use welfare. They are also characterized by extremely high numbers of high school dropouts and tend to be younger.[30] 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.

[28]This raises concerns that our four education dummies do not capture enough of the variation in education level. Hence, we also replaced the 4 education dummies by a finer partition of 7 education groups. More specifically, we split the high school dropouts in 4 different groups: less than first grade, 1st to 4th grade, 5th to 8th grade and 9th to 12th grade (without diploma). The results were unchanged.

[29]In Table 6, we investigate the effects of excluding Spanish speakers from our regressions.

[30]We investigate the effects of dropping these two groups in Table 6.

4 Empirical Results

4.1 Differences-in-Differences

Before discussing the basic results, it is useful to present a simple differences-in-differences calculation. Suppose 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 welfare groups is the analogue of using language fixed effects. Similarly, the control for contact availability becomes the difference between low and high contact availability. Finally, the interaction term becomes the difference of these differences.

Table 3 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.

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.[31]

Panels C and D show that the results are not completely driven by functional form.[32] 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% more likely to use welfare than low CA ones, while those from high welfare language groups are approximately 44% more likely.

4.2 Basic Results

Table 4 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). The mean welfare use in the interaction term is taken in deviation from the global mean welfare use in the sample: $CA * \left(\overline{Welf_k} - \overline{Welf}\right)$. This facilitates interpretation of the coefficient on the (non-interacted)

[31]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 the PUMA level specification and to 0.02406 (Standard Error: .0107) for the MSA level specification. The demographic controls consist of 3 race dummies, a quadratic in age, 4 education dummies, 6 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 Section 4.2, we discuss the choice of these controls.

[32]See Section 4.3 for a discussion of functional form issues.

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.[33]

The demographic controls include 4 education dummies, age, age squared, 3 race dummies, 6 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.[34] The first three sets of controls—race, education and age—clearly belong in the equations. The second set of 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 4, 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.[35] The negative effect of having a child present is the only anomaly. However, the sum of the coefficients on child present

[33]When we examined uncorrected standard errors (not shown), they were smaller, indicating that there is some correlation within language-area cells.

[34]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.

[35]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.

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. In all six cases, our measure of network effects is positive and significant.

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. 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 alternative hypothesis, comparing the OLS to 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 4, we find that selection between MSAs would be larger than selection within MSAs.[36] Because it is much easier to move within

[36] 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: $W = \varepsilon$. 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:

$$\hat{\alpha}_{OLS} = \frac{N_P'\varepsilon}{N_P'N_P} = \frac{(\gamma' N_M' + \eta')\,\varepsilon}{N_P'N_P} = \frac{\gamma' N_M'\varepsilon}{N_P'N_P} + \frac{\eta'\varepsilon}{N_P'N_P} \hat{=} \hat{\rho}_M + \hat{\rho}_P$$

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:

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

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

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.

Columns (3) and (4) replicate columns (1) and (2) but with the education dummies dropped. If the process governing differential selection on unobservable characteristics is similar to the selection process for observable ones, then dropping important observables provides information about omitted variable bias. As columns (3) and (4) show, dropping observable characteristics – the education dummies – hardly affects the estimated coefficient, alleviating worries about biases generated by differential selection.[37]

In column (5) and (6), we give estimates for networks 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 more noisy measure of social networks than PUMA level contact availability. Also for these specifications, we continue to find positive and significant network effects.[38] Column (6) replicates column (5) but without the education dummies. Again, the point estimate stays essentially the same.

This table establishes our 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)). As we discussed, this procedure is

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.

[37]In Table 6, we investigate the effects of dropping controls more thoroughly.

[38]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.

valid under the null hypothesis of no network effects. Third, dropping important controls–education dummies–does not change our coefficients (columns (3), (4) and (6)).

The coefficient α is hard to interpret. To provide a measure of the magnitude of the network effects, we perform two thought experiments. First, we ask what is the differential effect of increasing contact availability by two standard deviations for a person from a low welfare using language group compared to the effect for a person from a high welfare using language group. Welfare use for a group one standard deviation below the mean is 2.3% and welfare use for a group one standard deviation above the mean is 9.3%.[39] The standard deviation of contact availability within groups is 0.493; so for an α of 0.175, the effect is 2(.493)(.093)(.175)=.0161 for the person from the high welfare using group but only 2(.493)(.023)(.175) = 0.0040 for the person from the low welfare using group.[40] The difference, 0.0121, is 20.8% of welfare use. We report this number as the response to a shock in contact availability.

In the second though experiment, 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:

$$Welf_{ijk} = \xi + \left(CA_{jk} * \overline{Welf_k}\right)\alpha + X_i\beta + \gamma_j + \delta_k + CA_{jk}\,\theta + \varepsilon_{ijk} \tag{4}$$

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

[39]The population weighted standard deviation in welfare participation between language groups is 0.035. This differs from the standard deviation reported in Table 1, which is the individual standard deviation of welfare participation.

[40]Because this is the standard deviation of the *within* group contact availability, it is lower than the one reported in Table 1 which also includes variation in contact availability between language groups.

the increase in ξ because networks result in accelerator effects which 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.[41] 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:

$$\frac{1}{1-\alpha\overline{CA}_k} - 1 \tag{6}$$

This equation implies that a policy that increases welfare use by 1 percentage point in the absence of networks actually increases welfare use of language group k by $\frac{1}{1-\alpha\overline{CA}_k}$ 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% when we use the PUMA regressions and about 15% for the MSA regressions.

These estimates may understate the total network effects. Given the large number of positive omitted variable biases, we have taken a conservative approach.[42] Many of the variables which serve

[41] 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.

[42] 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.

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.[43] We ignore them because their impact likely includes other factors—personal 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 non language-based networks also exist. One should, therefore, keep in mind that our quantitative estimates do not capture all aspects of social networks.

4.3 Specification Checks

How sensitive are our results to functional form and sample choice? Table 5 examines the impact of changing the functional form assumptions.[44] One might reasonably be concerned that details of our specification might potentially distort our results. For example, it might be the case that the true model is a probit. In this case, our network measure, an interaction term, might proxy for these higher order non-linear terms, generating spurious coefficients. We have already discussed this to some degree in our diffs-in-diffs estimation (Table 3). In Table 5, we investigate this issue more thoroughly. In all specifications, we continue to find positive and significant network effects, though the quantitative size of these estimates is sometimes smaller.

In row (1), we begin by replicating columns (1) and (5) from Table 4 for ease of comparison. Rows (2) and (3) estimate logit and probit models respectively. They do not include PUMA fixed effects, however, due to the computational complexity of estimating logits and probits with over a thousand fixed effects. Row (4) informs us that dropping PUMA fixed effects does not significantly

[43]In fact, as discussed in Section 2, previous work on networks has focused on exactly these effects.

[44]Not reported in this table, but available from the authors, are the effects of adding other controls, such as a quartic in age, a dummy for having at least one child under the age of 6, more education dummies, immigrant status, year of immigration and English knowledge controls. These do not alter the findings either.

change our results in the linear probability model and one might tentatively think that the same would be true for the logit and probit specifications. In both the MSA and PUMA estimations, the probit and logit models produce positive and significant coefficients.[45]

Rows (5) to (8) alter the network measures. Recall that we measure networks through the interaction of contact availability and mean welfare of one's language group. In row (5), we replace mean welfare by log of mean welfare in the interaction term. The coefficients remain positive and significant. In rows (6), (7) and (8), we alter the way we measure contact availability. Recall that in the rest of the paper, we measure contact availability by:

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

In row (6), we use instead $\frac{C_{jk}/A_j}{L_k/T}$ as our measure. In other words, we use levels instead of logs. Row (7) uses $ln\left(\frac{C_{jk}}{A_j}\right)$ as the contact availability measure. This last measure is such that small language groups will always have small contact availability. Row (8) uses $ln(C_{jk})$, the log of the (unadjusted) number of people in an area-language cell, to measure the quantity of contacts. This allows to incorporate possibly changing returns to scale. All these changes in the way we measure contact availability result in positive and significant estimates at both the PUMA and MSA levels.

Finally, whenever one uses the interaction of two terms to identify an effect, concerns arise that, if either of the terms enters into the equation non-linearly, the missing higher order terms may generate an omitted variable bias. We investigate this directly by including a quartic in CA (row (9)).[46] Again, the coefficients do not change.

The issue of functional form is a tricky one since one can never establish with certainty that some yet untried specification will not alter the findings.[47] What bolsters our confidence, however,

[45]The marginal impact of these estimates, however, are smaller.

[46]We do not try higher order terms in $Welf_k$ because they would be absorbed by the language fixed effects.

[47]Non-parametric methods such as maximum score estimator might be of some assistance here, but unfortunately,

is that in all the specifications that we have tried, the estimates of network effects remain positive.

We now investigate the effect of changing samples. Table 6 displays the coefficients on the estimated network effects for different subsamples of our original data set. In Table 2, we saw that more than 55% 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 2, we also saw that the Miao and Mon-Kmer had extremely high welfare recipiency. These outliers may also drive our results. Therefore, in row (3), we exclude the Miao and Mon-Kmer 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 in child-bearing 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 women with children. On the other hand, the effect is smaller for single women with children.[48]

they do not seem practically possible in our case (see Greene (1990), p. 659).

[48] One potential explanation for this is that knowledge about AFDC is more widespread than knowledge about

4.4 Impact of Removing Controls on Estimates

In Table 4, we found that removing education dummies did not affect our network effect estimates. If unobservable characteristics about individuals drove our results, one would expect that increasing the set of unobservable characteristics by treating observable characteristics as unobservable would have a large impact on the estimate of network effects.

Table 7 investigates this issue more carefully. We begin with a sparse regression which 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 4. 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. This is consistent with our discussion in Section 4.5, where we argued the importance of neighborhood fixed effects. 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.[49] Although these controls decrease the coefficient by more then the education and exogenous controls, the drop in the coefficient is not

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.

[49]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.

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.

4.5 Explicit Selection Equations

We have already discussed in Table 4 several of our techniques for dealing with omitted variable biases. In this section, we investigate this issue by estimating explicit selection equations. Table 8 provides additional evidence against differential selection of individuals into high and low contact availability areas depending on the mean welfare of their language group. We regress residential choice, as measured by the contact availability in the area, on a set of demographic characteristics and on the interaction of these demographics with mean welfare use of the language group. If the results were driven by differential selection on unobservables that increase welfare use, the coefficient on the interaction term of the unobserved characteristics with mean welfare of the language group should be positive. As unobservables are (of course) not available, we proceed as in Section 4.4 and instead use observed characteristics. This approach is valid if the selection on unobservables is governed by a similar mechanism as selection on observables.

Columns (2) and (4) estimate these equations without PUMA fixed effects. They show that in the absence of PUMA fixed effects, there would be differential selection of the kind that could yield spurious estimates of network effects. This highlights the importance of controlling for PUMA fixed effects, which we do in all other specifications. In contrast, the regressions in columns (1) and (3) estimate the selection equations with PUMA fixed effects. They show no pattern of selection consistent with omitted variable biases corrupting our estimates. In column (1), we find a non-monotonic relationship between education and selection. In column (3), which is at the MSA level, we find a relationship between education and selection, but it is the exact opposite of the pattern one would expect if omitted variable biases corrupted our results. Less educated women are more likely to live in a high contact area if the mean welfare participation of their language group is *low*.[50] In conclusion, the results in this section indicate that, if anything, sorting on observables seems to bias our coefficients in the wrong direction. However, sorting on unobservables might be qualitatively different from sorting on observables so this does not completely rule out the possibility of differential selection.[51]

4.6 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. To this end, in Table 9, 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.[52] 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.[53] Similarly, columns (3) and (4) show that these women have a significantly

[50]That differential selection should operate in this reverse way is puzzling, but the results seem robust. We have estimated these regressions using continuous education variables, and further separating the high school dropout dummy into 4 more dummies. We also allowed other covariates to be interacted with the mean welfare. The findings carry through in all these cases.

[51]The non-interacted coefficients are not reported but these indicate that there is significant selection on observables, but this selection is not differential.

[52]The independent variable of interest remains CA interacted with mean welfare. One could imagine interacting CA with the relevant characteristic, for example with mean single motherhood in column (1). We did not do this for two reasons. First, to maintain consistency and comparability. Second, we are investigating the effects of a culture of poverty which we measure with mean welfare use.

[53]However, the quantitative impact of this channel is quite small. The coefficient on single motherhood in the original regression (Table 4, col. (1)) is .1947. Our measure of the impact of networks on single motherhood is .0896 (Table 9, col. (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 4, col (1)).

lower probability of being married. These results combined with the previous ones tell 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 towards AFDC (since it is of the greatest policy importance), 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 operate through welfare programs other than AFDC. However, the estimates of network effects for just AFDC are still important and very significant.

4.7 Distribution of Effects

Table 10 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.[54] 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.[55] Both these findings are consistent with our

[54]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.

[55]In fact, at the average number of years since entry (13.3 years), there is about no difference left in network effects

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.

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. 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 $CA_{jk} * (\overline{Welf}_k - \overline{Welf})$ 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.[56] Two oppositing 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

between foreign-born and US-born women.

[56]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.

English fluency of one's contacts *reduces* network effects.[57]

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.

4.8 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".[58] 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 that share the same country of origin are more likely to be in contact with each other. One can then estimate a regression

[57] 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 is 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 section 4.8.

[58] We are grateful to Aaron Yelowitz for suggesting this alternative interpretation to us.

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.[59]

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.[60] 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 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 consists 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.[61] 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 11 displays our results. Columns (1) and (2) are equivalent to columns (1) and (5) in Table

[59]For example, Gujarati speakers are by and large confined to only one state in India.

[60]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.

[61]We use the hispanic variable in the 1990 Census. For example, individuals that report Latin America as their hispanic origin are excluded from the sample since this is not a specific country.

4. 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 11 includes PUMA fixed effects that capture the differential accommodation of Spanish speakers between local welfare offices. The magnitude of effect estimated in these regressions are similar to the magnitudes computed for Table 4. 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.

5 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. Micro-estimates 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 argue for 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.

6 References

Alba, Richard D. (1990), *Ethnic Identity: The Transformation of White America*, New Haven: Yale University Press.

Bakalian, Anny (1993), *Armenian-Americans: From Being to Feeling American*, New Brunswick: Transaction Publishers.

Bénabou, Roland (1996), "Heterogeneity, Stratification, and Growth: Macroeconomic Implications of Community Structure and School Finance", *American Economic Review*, 86(3), (June), pp. 584-609.

Besley, Timothy and Stephen Coate (1992), "Understanding Welfare Stigma: Taxpayer Resentment and Statistical Discrimination", *Journal of Public Economics*, 48(2), (July), pp. 165-183.

Banerjee, Abhijit (1992), "A Simple Model of Herd Behavior", *Quarterly Journal of Economics*, 107(3), pp. 797-818.

Bikhchandani, Sushil, David Hirshleifer, and Ivo Welch (1992), "A Theory of Fads, Fashion, Custom, and Cultural Change in Informational Cascades", *Journal of Political Economy*, 100(5), (October), pp. 992-1026.

Borjas, George J. (1992), "Ethnic Capital and Intergenerational Mobility", *Quarterly Journal of Economics*, 107(1), pp. 123-150.

Borjas, George J. (1994), "Long-Run Convergence of Ethnic Skill Differentials: The Children and Grandchildren of the Great Migration", *Industrial and Labor Relations Review*, 47(4), pp. 553-573.

Borjas, George J. (1995), "Ethnicity, Neighborhoods, and Human-Capital Externalities", *American Economic Review*, 85(3), pp. 365-390.

Borjas, George J. and Lynette Hilton (1996), "Immigration and the Welfare State: Immigrant Participation in Means-Tested Entitlement Programs", *Quarterly Journal of Economics*, 111(2), (May), pp. 575-604.

Bulow, Jeremy and Paul Klemperer (1994), "Rational Frenzies and Crashes",*Journal of Political Economy*, 102(1), (February), pp. 1-23.

Case, Anne C. and Lawrence F. Katz (1991), " The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths", National Bureau of Economic Research Working Paper: 3705, (May).

Evans, William N., Wallace E. Oates and Robert M. Schwab (1992), "Measuring Peer Group Effects: A Study of Teenage Behavior", *Journal of Political Economy*, 100(5), (October), pp. 966-991.

Ellison, Glenn and Drew Fudenberg (1993), "Rules of Thumb for Social Learning", *Journal of Political Economy*, 101, pp. 612-43.

Ellison, Glenn and Drew Fudenberg (1995), "Word-of-Mouth Communication and Social Learning", *Quarterly Journal of Economics*, 110(1), pp. 93-126.

Granovetter, Mark S. (1985), "Economic Action and Social Structure: The Problem of Embeddedness", *American Journal of Sociology*, 91, pp. 481-510.

Green, William H. (1990), *Econometric Analysis*, New York: Macmillan Publishing Company.

Jencks, Christopher and Susan E. Mayer (1990), "The Social Consequences of Growing up in a Poor Neighborhood", in Laurence E. Lynn and Michael G.H. McGeary (eds.), *Inner-City Poverty in the United States*, Washington, D.C.: National Academy Press, pp. 111-186.

Lazear, Edward P. (1995), "Culture and Language", National Bureau of Economic Research Working Paper: 5249, (September).

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

Murphy, Kevin M. and Robert H. Topel (1990), "Efficiency Wages Reconsidered: Theory and Evidence", in Yoram Weiss and Gideon Fishelson (eds.), *Advances in the Theory and Measurement of Unemployment*, New York: St. Martin's Press, pp. 204-240.

Nechyba, Thomas (1996), "Social Approval, Values and AFDC: A Re-examination of the Illegitimacy Debate", mimeo, Stanford University.

Lucas, Robert (1988), "On the Mechanics of Economic Development", *Journal of Monetary Economics*, 22(1), pp. 3-42.

Romer, Paul (1986), "Increasing Returns and Long Run Growth", *Journal of Political Economy*, 94(5), (October), pp. 1002-1037.

Rosenbaum, James E. (1995), "Changing the Geography of Opportunity by Expanding Residential Choice: Lessons from the Gautreaux Program", *Housing Policy Debate*, 6(1), pp. 231-261.

Wilson, William J. (1987), *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*, Chicago: The University of Chicago Press.

Table 1: Summary Statistics

Variable:	Mean	Std. Dev.	U.S. 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
Poor English	.7673	.4225	.0399

Variable:	Mean	Std. Dev.	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

Notes:

1. Data for columns 1 and 2 in the top panel is 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*).
2. Data for the U.S. mean is 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*).
3. 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*).
4. "Welfare" is a dummy variable that equals 1 if the woman receives public assistance. "Child Present" is a dummy that equals 1 if the woman has some own children at home. "Number of Kids" is the number of children ever born. "Poor English" equals 1 for individuals who speak English "not well" or "not at all" and 0 for those who speak it "well" or "very well".

Table 2: 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
Gujarati	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

Notes:

1. Data is 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*).
2. Group Size in 5% Census is the number of individuals that speak the language at home in the entire 5% 1990 Census.
3. The language group Chinese includes the Chinese dialects of Cantonese, Yueh and Min, but excludes Mandarin and other Chinese dialects. Tagalog is spoken Manila and the its adjacent Provinces, and Ilocana is spoken in northern Luzon in the Philippines. Mon-Khmer is spoken in Cambodia. Miao (also called Hmong) is a language spoken in the mountenous regions of Southern China and adjacent areas of Vietnam, Laos and Thailand. Gujerati 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.

Table 2 (cont.): 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:						
Rumanian	877	3041	4.2	30.79	34.70	61.46
Serbocroatian	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

Table 3: Differences-in-Differences Estimation

Panel A
Differences
PUMA Level Contact Availability

	Low CA	High CA	Δ CA
Low Welfare LG	0.0205	0.0226	0.0021
	(0.0005)	(0.0006)	(0.0008)
High Welfare LG	0.0645	0.0928	0.0284
	(0.0007)	(0.0008)	(0.0011)
Δ LG	0.0440	0.0702	**0.0263**
	(0.0009)	(0.0010)	(0.0013)

Panel B
Differences
MSA Level Contact Availability

	Low CA	High CA	Δ CA
Low Welfare LG	0.0200	0.0232	0.0032
	(0.0005)	(0.0006)	(0.0008)
High Welfare LG	0.0687	0.0875	0.0188
	(0.0008)	(0.0008)	(0.0011)
Δ LG	0.0487	0.0642	**0.0155**
	(0.0009)	(0.0010)	(0.0013)

Panel C
Ratios
PUMA Level Contact Availability

	Low CA	High CA	CA_{High}/CA_{Low}
Low Welfare LG	0.0205	0.0226	1.1022
	(0.0005)	(0.0006)	(0.0392)
High Welfare LG	0.0645	0.0928	1.4399
	(0.0007)	(0.0008)	(0.0200)
LG_{High}/LG_{Low}	3.1485	4.1130	**1.3064**
	(0.0864)	(0.1094)	(0.0499)

Panel D
Ratios
MSA Level Contact Availability

	Low CA	High CA	CA_{High}/CA
Low Welfare LG	0.0200	0.0232	1.1624
	(0.0005)	(0.0006)	(0.0414)
High Welfare LG	0.0687	0.0875	1.2734
	(0.0007)	(0.0008)	(0.0177)
LG_{High}/LG_{Low}	3.4350	3.7631	**1.0955**
	(0.0935)	(0.1010)	(0.0419)

Notes:

1. Data 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*).
2. Welfare use is measured as the fraction of people receiving any form of public assistance.
3. 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.
4. The bold face numbers in Panel A and B are the difference-in-difference estimates. The bold face numbers in Panel C and D are the ratio-of-ratios estimates. Standard errors are in parentheses.

Table 4: Main Results

Dependent Variable: Welfare Participation

	(1)	(2)	(3)	(4)	(5)	(6)
CA Measure:	Log PUMA	Log PUMA	Log PUMA	Log PUMA	Log MSA	Log MSA
Estimation Technique:	OLS	IV	OLS	IV	OLS	OLS
Contact Availability * Mean Welfare of LG	**.1751******	**.1638******	**.1793******	**.1689******	**.1444******	**.1474******
	(.0258)	**(.0281)**	**(.0257)**	**(.0287)**	**(.0282)**	**(.0280)**
Contact Availability	.0024***	-.0020*	.0041****	-.0001	-.0021	-.0004
	(.0009)	(.0011)	(.0009)	(.0010)	(.0015)	(.0011)
HS Dropout	.0469****	.0477****	-	-	.0475****	-
	(.0018)	(.0018)			(.0062)	
HS Graduate	.0162****	.0168****	-	-	.0165****	-
	(.0011)	(.0011)			(.0021)	
Some College	.0037****	.0040****	-	-	.0038****	-
	(.0008)	(.0008)			(.0010)	
Single Mother	.1947****	.1946****	.1949****	.1948****	.1947****	.1949****
	(.0044)	(.0044)	(.0044)	(.0044)	(.0083)	(.0084)
Child present	-.0043****	-.0041****	-.0032***	-.0030**	-.0042	-.0030
	(.0012)	(.0012)	(.0012)	(.0012)	(.0026)	(.0027)
Number of Children	.0145****	.0146****	.0174****	.0175****	.0146****	.0175****
	(.0005)	(.0005)	(.0005)	(.0005)	(.0010)	(.0013)
Married, spouse absent	.0405****	.0407****	.0435****	.0437****	.0407****	.0437****
	(.0028)	(.0028)	(.0029)	(.0029)	(.0085)	(.0089)
Widowed	.0403****	.0403****	.0435****	.0436****	.0405****	.0437****
	(.0044)	(.0044)	(.0044)	(.0045)	(.0047)	(.0049)
Divorced	.0183****	.0182****	.0176****	.0175****	.0182****	.0175****
	(.0026)	(.0026)	(.0026)	(.0026)	(.0048)	(.0048)
Separated	.0830****	.0830****	.0862****	.0863****	.0831****	.0864****
	(.0040)	(.0040)	(.0041)	(.0041)	(.0092)	(.0098)
Never Married	.0392****	.0394****	.0404****	.0406****	.0393****	.0405****
	(.0020)	(.0020)	(.0020)	(.0020)	(.0058)	(.0060)
Age	.0074****	.0074****	.0038****	.0037****	.0074****	.0038****
	(.0004)	(.0004)	(.0003)	(.0003)	(.0011)	(.0006)
$Age^2/100$	-.0105****	-.0105****	-.0058****	-.0057****	-.0105****	-.0058****
	(.0005)	(.0005)	(.0004)	(.0004)	(.0014)	(.0007)
White	-.0054****	-.0057****	-.0082****	-.0086****	-.0059****	-.0088****
	(.0012)	(.0012)	(.0012)	(.0012)	(.0018)	(.0020)
Black	.0069**	.0061*	.0032	.0023	.0058	.0019
	(.0035)	(.0035)	(.0035)	(.0036)	(.0076)	(.0076)
PUMA F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Language Group F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.174	-	.169	-	.174	.168
Response to Welfare Shock	26.6%	24.2%	27.4%	25.3%	14.6%	14.9%
Response to *CA* Shock	20.8%	20.0%	21.3%	20.0%	12.1%	12.3%

Notes: See next page.

Notes:

1. Data 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*).
2. Welfare Participation is a dummy variable that equals 1 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".
3. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which CA measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%
4. Language Fixed Effects are 42 language dummies. PUMA Fixed Effects are 1196 dummies for the PUMAs represented in the sample.
5. "Mean Welfare of LG" is expressed as a deviation from the sample mean (over all language groups).
6. The thought experiments underlying the response to the welfare shock and the response to the CA shock are explained in the text.
7. 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. The F-statistics for the IV in column (4) are similar.

Table 5: Functional Form Checks

*Reported: Coefficient on the interaction term (CA*Mean Welfare of LG)*

Dependent Variable: Welfare Participation

Change in Functional Form: / *CA Measure:*	Log PUMA (1)		Log MSA (2)	
(1) Specification as in table 3	.1751****	(.0258)	.1444****	(.0282)
(2) Logit without PUMA F.E	1.4827****	(.2697)	1.2920****	(.2773)
(3) Probit without PUMA F.E.	.8456****	(.1371)	.6528****	(.1413)
(4) As (1) but without PUMA F.E.	.1743****	(.0242)	.1157****	(.0359)
(5) As (1) but mean welfare is replaced by log mean welfare in the interaction term	.0060****	(.0010)	.0040***	(.0014)
(6) As (1) but CA is measured in levels rather than in logs	.0050****	(.0015)	.0057***	(.0022)
(7) As (1) but CA is measured as $\ln(C_{jk}/A_j)$	3.4504***	(1.1487)	4.9331**	(2.1821)
(8) As (1) but CA is measured as $\ln(C_{jk})$	.1554****	(.0240)	.07107***	(.0261)
(9) As (1) but a quartic polynomial in CA is included as a control	.1841****	(.0258)	.1481****	(.0283)

Notes:

1. Data 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*).
2. 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.
3. Welfare Participation is a dummy variable that equals 1 if the individual receives any form of public assistance. Demographic controls include 4 education dummies, 6 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.
4. The Contact Availability (CA) measure used throughout the paper and in specification (1) is defined as $CA_{jk} = ln[(C_{jk}/A_j) \ / \ (L_k/T)]$ where C_{jk} is the number of people of language group k in area j, A_j is the number of people in area j, L_k is the number of people in language group k, and T is the total number of people in the U.S.
5. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which CA measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%

Table 6: Sample Choice

*Reported: Coefficient on the interaction term (CA*Mean Welfare of LG)*

Dependent Variable: Welfare Participation

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

Notes:

1. 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 right column.
2. 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.
3. Welfare Participation is a dummy variable that equals 1 if the individual receives any form of public assistance. The Contact Availability (CA) measures are defined in detail in the text.
4. Demographic controls include 4 education dummies, 6 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.
5. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which LGC measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%

Table 7: Sensitivity of Results to Addition of Controls

*Reported: Coefficient on the interaction term (CA*Mean Welfare of LG)*

Dependent Variable: Welfare Participation

CA Measure: / *Controls:*	Log PUMA (1)	Log MSA (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)

Notes:

1. Data 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*).
2. All regressions include contact availibility. Only the coefficient on the interaction term is reported.
3. Welfare Participation is a dummy variable that equals 1 if the individual receives any form of public assistance. The Contact Availability (CA) measures are defined in detail in the text.
4. Exogenous Controls include a white dummy, a black dummy and a quadratic in age. Education is composed of 4 education dummies. In addition to the exogenous controls and Eduction, All Controls include Marital Status, Child Present and Number of Children. Marital Status is composed of 6 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.
5. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which CA measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%

Table 8: Residential Choice				
Dependent Variable:	Log PUMA CA		Log MSA CA	
	(1)	(2)	(3)	(4)
HS Drop Out*Mean Welfare in LG	−.2680** (.1252)	3.0693**** (.1957)	−1.4345**** (.1042)	.8045**** (.1653)
HS Degree*Mean Welfare in LG	−.3570*** (.1333)	2.4451**** (.2090)	−1.3657**** (.1110)	.4730*** (.1765)
Some College*Mean Welfare in LG	.6826**** (.1296)	2.9390**** (.2034)	−.3644**** (.1078)	1.2779**** (.1718)
Language F.E.	Yes	Yes	Yes	Yes
PUMA F.E.	Yes	No	Yes	No
Adjusted R^2	.670	.181	.659	.128

Notes:

1. Data 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*).
2. The dependent variable is the log Contact Availability measure at the PUMA level (columns 1 and 2) or at the MSA level (columns 3 and 4). The Contact Availability measures are defined in detail in the text.
3. The omitted category for the education interaction terms is "College or higher*Mean Welfare in LG"
4. In addition to the interaction terms reported in the table, all regressions contain the following independent variables: 4 education dummies, 6 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.
5. Standard errors are in parentheses. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%

Table 9: 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)
$Age^2/100$	-.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

Notes:

1. Data 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*).
2. Welfare Participation is a dummy variable that equals 1 if the individual receives any form of public assistance. Single Mother is a dummy variable that equals 1 for single mothers and Married is a dummy variable that equals 1 for married women. The Contact Availability (CA) measures are defined in detail in the text. The omitted education dummy is "College and More".
3. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which CA measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%
4. Language Fixed Effects are 42 language dummies. PUMA Fixed Effects are 1196 dummies for the PUMAs represented in the sample.
5. "Mean Welfare of LG" is expressed as a deviation from the sample mean (over all language groups).

Table 10: Distribution of Network Effects

Dependent Variable: Welfare Participation

Distributional Effect Based on:	*CA Measure:*	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)

Notes: See next page

Notes:

1. Data 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*). 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*).

2. 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 4 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.

3. Welfare Participation is a dummy variable that equals 1 if the individual receives any form of public assistance. The Contact Availability (CA) measures are defined in detail in the text. Demographic controls include 4 education dummies, 6 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.

4. Foreign Born is a dummy variable that equals 1 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 U.S. It equals zero for U.S. born individuals (Mean: 8.4 Std.: 10.0). The variable "Own English Fluency" equals 0 for individuals who speak English "not well" or "not at all" and 1 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)

5. The variables Foreign Born, Years since Entry, Poor English, English Fluency in Area-Language Cell and State AFDC Generosity enter in the regressions as a deviation from their sample mean.

6. Standard errors are in parentheses. They are corrected to allow for group effects within PUMA-language cells (22543 cells) or MSA-language cells (6197 cells), depending on which LGC measure is used. Asterisks indicate significance levels: * is 10%, ** is 5%, *** is 1%, **** is .1%

Table 11: Hispanic Sample

Dependent Variable: Welfare Participation

	(1)	(2)
CA Measure:	Log PUMA	Log MSA
Estimation Technique:	OLS	OLS
Contact Availability * Mean Welfare of LG	**.1238****** **(.0092)**	**.0871****** **(.0163)**
Contact Availability	.0017** (.0007)	.0021 (.0014)
PUMA F.E.	Yes	Yes
Language Group F.E.	Yes	Yes
Adjusted R^2	.197	.197
Magnitude of Effect	31.8%	16.0%

Notes:

1. Data is composed of all women between 15 and 55 years old in the 1990 Census 5% extract who speak Spanish at home and are from hispanic origin. Women living in mixed MSAs and non-MSAs are excluded from the sample. (*Sample size: 202990*).
2. The Contact Availability (CA) measure is defined as $CA_{jh} = ln[(\frac{C_{jh}}{A_j})/(\frac{L_h}{T})]$ 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 U.S.. The sample mean of CA_{jh} is 1.90 at the PUMA level and 1.57 at the MSA level.
3. "Mean Welfare of CG" is the mean welfare by country of origin. It is expressed in deviation from the sample mean.
4. Country of Origin Fixed Effects are 24 country of origin dummies. PUMA Fixed Effects are 1179 dummies for the PUMAs represented in the sample.
5. Welfare participation is a dummy variable that equals 1 if the individual receives any form of public assistance. The omitted education dummy is "College and More". The omitted marital status dummy is "Married, Spouse Present".
6. 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%, ** is 5%, *** is 1%, **** is .1%.
7. The magnitude of effect calculation is explained in the text.