

Memo to the reviewers #2, 5-22-2003.

Memo: response to the reviewers' comments concerning "Social Capital and Job Search: Do Contacts Matter?"

I would like to thank the reviewers for their beneficial comments on this paper. It is very helpful to receive such serious and thoughtful comments, and I believe that the paper has improved as a result. In this memo, I first want to highlight the major changes to the paper, and then I will go through each reviewer's point in detail. Finally, at the end of the memo (pages 17-20) I address two important questions in detail--the relationship between the single firm and worker data, and the possibility that the use of contacts to search for work increases with unemployment duration.

Major changes or key points:

1) **Length.** In order to reduce the length of the paper, I cut 7 of the first 21 pages. The "Theories and Empirical Tests" section now begins on page 14 rather than page 21. I made further cuts in the rest of the paper, but also added some material in (see non-search, below). Overall, the paper is over 7 pages shorter than the previous version (31.5 pages versus 38 in the previous version). In addition, I would be willing to significantly compress the proofs in Appendices A and B to reduce the overall size of the paper.

2) **Network social capital.** Several reviewers pointed out that aspects of social capital such culture, language, dress, manners and group membership could affect the offer rate and wages of jobs found both with and without job contacts. This is correct. In this version of the paper, I have clarified my definition of social capital. I now use the term "network social capital" to refer to the information, help, or influence provided by friends and relatives. I stress that network social capital is a characteristic of relationships and ties with other people (i.e. it is not an individual characteristic), and can only be accessed through interaction (either deliberate or through incidental social contact) with social network members. The indirect test of causality proposed by this paper can only assess whether there is evidence that network social capital—information and influence from friends and relatives—affects labor market outcomes. I think using this term helps to clear up some potential misunderstandings about my test of the role of social capital.

3) **Non-search.** I have included a section discussing the findings on employed non-searchers (who may find their jobs through contacts), which was a part of the first version of this paper. See comment B4 below.

4) **Single firm studies and individual worker data.** Several reviewers asked whether the findings from the single firm studies could be reconciled with the findings from surveys of workers. The short answer is that the theoretical models used in the paper (the sequential and extensive search models) show that the beneficial effect of contacts should show up in surveys of workers. I discuss this question in detail on pages 17-18 of the memo and provide several examples to illustrate my point.

Specific points and comments

Reviewer A

A1. Q: The reviewer questions whether people with more advantaged social networks should be more likely to use alters in their job search, arguing that this would be true if only if people had perfect information about the value of using their networks.

I think this question arises due to confusion in the previous version of the paper concerning the difference between the methods you used to look for work and the specific method that resulted in your job. In the text, “using contacts to find work” refers to the particular method that resulted in the current job. I now clearly state this in footnote 4 on page 9. In general, most workers use multiple methods to search for work. As noted footnote 4 on page 9, 75% of workers used friends and relatives to search for work in the MCSUI data, although only 40% actually found their job through friends and relatives. Furthermore, of those who used contacts to search for work, 90% used at least one other method of job search. These results are reported in Tables A1 and A2 in this memo. Table A3 also reports the search methods used by those workers who did not use contacts to search for work. The majority looked in the newspaper, while large numbers also used walk-ins or sent resumes directly to employers.

If workers only used one method to look for work, then the reviewer is right that better-connected workers might only be more likely to use contacts to look for work if they had good information about the quality of their contact networks.

However, given that most workers use contacts to search for work—75% in the MCSUI data—and almost all workers who search using contacts also use another method, you do not need to invoke assumptions about perfect information to see how better connected workers should be more likely to obtain their job through contacts. Given that workers search through multiple methods, better-connected workers will be more likely to obtain work via contacts (because their contacts are better positioned to provide useful information and influence) even if they are not consciously aware of the fact that they are “well-connected.”

A2: The reviewer is concerned that well-connected people would simply be more likely to use all (or at least many) search methods.

If this were true, then it is possible that poorly-connected workers would be more likely to use contacts because they don’t use alternative methods (such as look at help-wanted signs and reading newspaper advertisements), while better-connected workers do. The question I would pose here is why would we expect workers with poor contacts to be less likely to use alternative methods? This seems irrational, since it is precisely those workers with poor connections that would most benefit from using alternatives methods of job search. In this case, one would be arguing that workers with higher levels of social capital are somehow smarter or more cognizant of the benefits of searching with multiple methods. This would be an individual characteristic associated with higher levels of social capital.

However, this hypothesis can be tested with the MCSUI data, which asks respondents which methods they used for their last job search. Tables A1-A3 (discussed in Comment A1) describe this data; most workers used informal methods to look for work. Table A2 shows the total number of methods used to search for work, other than contacts, for those workers who used contacts to search for work. The reviewer’s question asks whether better connected workers who

used contacts are more likely than poorly connected workers to use other methods. I have tested this in Table A4 with the MCSUI data. Model 1 is an OLS model of the number of non-contact search methods used for those workers who searched using contacts. The social capital measures (average education and proportion of friends in steady jobs) are not positively associated with the # of methods. Model 2 replicates this for all cases. Model 3 tests a logit model for the use of newspaper advertisements to look for work among workers who used contacts, and, similar to Model 1, no evidence of a positive effect of social capital is found.

Models 1 and 3 show that workers with higher levels of “social capital” who used contacts to search for work were not more likely to use other methods of search. Of course, it could be that the measures of social capital--based on the demographic characteristics of 3 friends--are not really capturing the beneficial aspects of network social capital (as I argue in the text). However, what seems clear is that these measures of social capital are not being biased by a tendency of poorly connected workers to use fewer alternative methods of job search.

A3. The reviewer argues that social capital may affect jobs that are found through formal search.

This point is well taken and is discussed above in key point #2 above. I can only test whether network social capital affects job outcomes.

A4. The reviewer is concerned about period and contextual variation in social capital effects because the data is drawn from different periods and geographic areas.

The data sets in the paper are used because they each have certain strengths. At the same time, no single data set has all the necessary components needed for this paper. The NLSY has longitudinal data on the use of contacts to obtain work (but no network data). The MCSUI and UPFLS data sets are the most prominent publicly available data sets that combine social network and labor market information, but they are not longitudinal. The DAS is the only publicly available data set that has the 3-digit occupation of the contact person. My use of data sets for this project was not intended to be “selective”. I have used the best publicly available data for each of these questions. Nonetheless, this is a fair question and in the following I discuss the DAS, MCSUI, and UPFLS data sets:

1) DAS. The biggest concern might be the 1970 DAS data (from Detroit, see appendix C). It’s a small data set and is over 30 years old. However, the only other data set that I know of that has the occupational prestige of the contact person is Nan Lin’s Albany data set, which is just as old (the data was collected in 1975) and just as small (219 respondents who used contacts). Because my criticism of exogenous social capital models (Point I of Figure 1) is a critique of the existing literature which uses either the 1970 DAS (Marsden and Hurlbert 1988) or the 1975 Albany data (i.e. Lai, Lin, and Leung 1998) I don’t think this is a criticism directed at me so much as the existing literature. More fundamentally, my criticism of this line of research is that the data itself is flawed. Information on contacts is only collected for those who used contacts to find work (see my discussion in the text in point I of Figure 1). This criticism holds whether the data were collected in 1970 or 2000.

2) MCSUI and UPFLS. The UPFLS covers Chicago, and the MCSUI data covers Boston, Atlanta, and Los Angeles (the Detroit sample did ask the social network questions). Together, I believe they constitute a good sample, but I would make no attempt to say that they are nationally representative. I don’t think that this affects the results, but I guess it could be considered an

open question. Given that there is no nationally representative data set that combines both social network data with job search data, these two data sets are as good as we can do at the moment. However, as argued in the paper, I believe that different types of network questions, particularly those that get at the extensiveness of job networks rather than 3 friends, will result in different findings. The purpose of this paper is to make the argument that the current data do not prove that network social capital matters, and that we need to evaluate how we should measure the beneficial aspects of contact networks. It is not so much the geographic range of the data I am worried about so much as the type of network data use to measure social capital. I don't think it gets at what we mean by network social capital.

A5. The reviewer mentions the Moving to Opportunity and quasi-experimental studies as better ways of testing for the effect of social capital.

I certainly agree that experimental data would be better. The difficulty is that it is hard to think of what such an experimental or quasi-experimental condition would be. Moving to Opportunity (MTO) works to test for neighborhood effects because you can give people a voucher to move to a more desirable area. Contact networks operate over space, so a simple geographic move such as MTO would not work. The goal of this paper is to see what we can conclude with the existing data. I know that null results are unpopular, but if it spurs sociologists to collect new data—either better network questions or some sort of quasi-experimental data—to prove this paper wrong, then it will have been worth it.

A6. The reviewer says that the potentially germane results of the randomly assigned roommates are dismissed without discussion.

It was not my intent to give the impression that I was dismissing the results of the studies of peer-effects using randomly assigned roommates. On the contrary, I think this is the ideal approach for cracking the problem of whether network social capital has a causal effect or not. However, randomly assigned roommates may work for peer effects on college grades (it is still a weak instrument and even then open to criticism as the amount of time you spend with your randomly assigned roommate is a function of how well you get along) but they do not work as an instrument for labor market social capital. What would? For example, a research study that randomly assigned workers in a specific occupation 1, 2, or 3 job contacts and then tried to see if then number of job contacts affect outcomes might do the trick. I can't think of any situation where this sort of data would occur naturally, such as colleges randomly assigning roommates. In the absence of this sort of ideal data, I argue in the paper that we can use theory (specifically, theories about how people look for work) to provide indirect tests of causality.

A7. The reviewer asks why social network effects would be negative in Table 6.

I assume that the reviewer is referring to the negative effect of using contacts on wages in Model 1 of Table 6. The short answer is that I do not view the direct relationship between the use of contacts and wages as a causal effect. The direct effect of using contacts on wages is misleading, as argued by Montgomery (1992) and discussed in the paper on pages 5-6. The negative coefficient may tell us something about the type of worker or job that is more likely to rely on contacts, rather than the causal effect of contacts. If low-paying jobs are more likely to be filled using informal methods, then this could downwardly bias the coefficient on contacts. This is discussed in point F of Figure 1. The possibility that certain jobs are more likely to recruit using informal methods is the reason all of the models of the probability of using contacts also include a variable measuring the % of jobs in the respondent's 3-digit census occupation that used contacts.

(i.e., the models in Table 7 ask whether, net of occupation level differences in the use of contacts, better connected workers are more likely to use contacts to find work).

A8. The text of the social capital questions from the UPFLS and MCSUI data have been included at the end of this memo.

A9. The reviewer suggests that Figure 1 reflects the scattered nature of the analytical section and that a more conventional approach of identifying and testing hypotheses sequentially might provide more clarity.

The second half of this paper is an "empirical essay" in that I am narrating the reader through a series of possible answers to the question "do contacts matter?". Because I am presenting multiple tests, I believe it is important that the empirical test come directly after the hypotheses, in order to keep preserve the logic of the discussion. Each test itself becomes a part of the discussion, rather than the culmination of the discussion, as in most papers. As I have discovered, writing a paper arguing for null results is more difficult than a paper showing a positive results. I could break the paper up into 4-5 smaller papers, each with a single data set, single hypotheses, and a single null result, but this would be at the expense of the underlying discussion that ties the results together. This, I would argue, is the benefit of Figure 1.

A10. I corrected the date for the DAS data.

Reviewer B

B1. The reviewer worries that the tone of the paper is overly definitive, and that some of the analyses are no better or worse than the studies they criticize.

The reviewer is quite right to be cautious and curious about whether the results presented here are robust to changes in the models. The irony is that I began this paper several years ago with the firm conviction that I would find evidence of the positive effect of contacts, and all the models tested here have been run and re-run under that assumption that the positive effect was hiding somewhere in the data. I am confident that all the empirical results in the paper will stand up because I have tested them carefully for robustness (with the aforementioned desire to find positive findings). Consequently, if/when this paper gets published I will post the data and computer files used in the analyses so that curious readers can quickly test alternative theories for themselves. Within the confines of a single paper it is difficult to present all alternative specifications. In this space I will, however, present the alternative models suggested by the reviewer.

I have modified the tone considerably from the previous version of the paper. As discussed above, I have added a section on the effect of non-search, which is a positive finding. I stress at the end of the paper that my critique is not that contacts do not matter, but that our measurement of network social capital is not picking up the presumed beneficial aspects of contact networks.

B2. The reviewer argues that the discussion of the null findings for the direct effect of contacts in Table 2 is overly definitive.

This is a case where I think the weight of accumulated evidence is rather definitive. Pages 3-4 of the paper discuss the literature on the direct effect of contacts on wages. Using the recently published reviews of this literature as a guide, I discuss the studies reviewed by Granovetter (1995), Marsden (2001), and Bartus (2001). Of the 12 studies they review, 8 show no effect of

contacts on wages. Moreover, of the 4 that do, each one has serious qualifications (see pages 3-4: of the four studies, one is with a sample of 260 cases, one is statistically insignificant, one is because of the choice of the omitted search category, and one is an effect of contacts with relative several years later). Overall, the evidence of no direct effect of contacts on wages is compelling.

The reviewer notes that on page 22 in the previous version I stated that "there have been conflicting findings" in this area. To reflect the more definitive nature of the evidence, and the summary evaluation of Marsden (2001), Lin (1999), Granovetter (1995), and Bartus (2001) I have deleted this sentence from the current version and substituted the following: "As indicated by the literature review in this paper, previous research has found little evidence of a direct effect of contacts on wages and job prestige (Marsden 2001, Lin 1999, Granovetter 1995, Bartus 2001)."

I should also note that while replicating the findings on wages, Models 2-8 of Table 2 also assess the direct effect of using contacts on future wages, unemployment duration, job satisfaction, union effects, and job duration (I have reinserted the results of the job duration model as a footnote to Table 2). These are all reasonable hypotheses about how contacts could affect labor market outcomes and there hasn't been much literature on these outcomes. Thus only Model 1 of Table 2 is a redundant to the current state of the literature on the subject.

B3. It is log-wages throughout. I have corrected this in all the tables.

B4. The reviewer points out that the models in Tables 2 and 4 that non-seekers have been included in the models. These are workers who found their last job without actively searching for work.

The point about non-search is important. As Granovetter argues, it is likely that non-searchers found their jobs through incidental social contact. As the reviewer points out, non-searchers are excluded from most models and data sets. I believe the inclusion of non-searchers is one of the strengths on the NLSY data. Page 19 of the paper now includes a discussion of the findings on non-searchers (this was part of an earlier version of the paper). As noted in the paper, the results in Table 4 for employed non-searchers indicates one way in which contacts may be important.

Does including non-searchers bias the results in Table 4? No. The question on the use of contacts in the 1994-1998 NLSY data was only asked for workers who actively searched for work (a note of this is now included in Appendix C). Therefore, including non-searchers and also including a dummy variable for non-search simply prevents non-searchers from being part of the default comparison group for workers who used contacts. In other words, the coefficient on contacts compares the average wage of jobs that were found via contacts versus jobs that were found without contacts but with active search. At the same time, including non-searchers in the model is important, as the reviewer points out, because Granovetter argues that "nonsearch" may actually largely due to the use of contacts. See the next point for an explicit test of the effect of including the non-search variable.

B5. The reviewer worries about the specification of the fixed effects models in Table 4 with respect to the search variables and the year dummies.

Table B1 of this memo re-estimates Model 4 of Table 4 excluding the search variables and/or the year dummies. This does not affect the results in Models 2 and 3 of Table B1 (other than making the coefficient on using contacts smaller). Similarly, excluding the search variables for the models in Table 2 does not affect the results. The reviewer is correct to note that the fixed effect

differences out the reservation wage. Indeed, if you believe that the use of contacts is endogenous to the level of network social capital (i.e. Montgomery's multiple method model) as discussed on pages 29-30 of the paper, then the null results in Table 4 are not surprising. That is why I think the fixed effects results are important; if you think the use of contacts is exogenous, then Table 4 (and B1) show that workers don't do any better when they obtain work with contacts than when they don't use contacts. The best way to explain that and still maintain a role of contacts in labor market outcomes is Montgomery's search model approach.

B6. The reviewer worries about the specification of the models in Table 3--the coefficient on missing occupational contacts, the lack of controls for experience, tenure, and union job, and the difference between layoffs and plant closings.

I have tested alternative specifications of Table 3 in Table B2. In Model B2.1 of Table B2 I add experience and experience squared to the model for the sample of workers who were unemployed in the NLSY due to plant closings or layoffs. These are measures of cumulative labor market experience. Comparing Model B2.1 to Model 2 of Table 3 we see that the coefficient on contacts doesn't budge. [I did not include controls for tenure in the previous job or union status in the previous job because it would involve going back to the NLSY work history data to reconstruct this variables--I don't think they will matter if cumulative experience doesn't, but if this is a sticky point I will go back and do it]. Models B2.2 and B2.3 exclude the 68 cases that were missing the 3-digit occupation code. Excluding these cases does not affect the results. Finally Models B2.4 and B2.5 address the issue of whether including layoffs affects the results. B2.4 only looks at the 151 cases where the unemployment was due to a plant closing, and B2.5 adds a dummy variable for plant closings. In neither model does the coefficient on contacts suggest qualitatively different results.

The results in Tables 2 and B2 are important, because they show that unemployed workers who used contacts did not have shorter periods of unemployment duration. In fact, the coefficient is positive and statistically significant. However, note that under the model espoused in this paper the models in Table 2 do not definitively answer the question of whether or not having good connections reduces unemployment duration. To answer this question, we would need data on social networks and unemployment duration. The social network data used in this paper, the MCSUI and UPFLS does not record duration of unemployment prior to present job. So, in that sense, I believe the question is still open.

B7. The reviewer argues that the paper rushes to quickly to get through Figure 1, and suggests that the paper would be better organized as a meta-analysis organized around Figure 1 with the empirics limited to holes in the current knowledge.

I have taken this comment quite seriously, so let me explain why I have decided to keep the original structure of the paper. First, with the exception of Model 1 of Table 2 (the direct effect of contacts on wages) every other empirical analysis in Tables 2-8 is either a new finding or, in the case of Table 5, a reversal of previous findings. [Granted, the effect of social capital on wages in Table 6 itself is not new, but it is part of the test of causality in conjunction with Table 7]. A meta analysis would work only for the direct effect of contacts on wages (see the response to question B2 above). The amount of space devoted my analysis of the direct effect of contacts on wages is very small. Aside from this, each of the empirical analyses is giving us something new.

Let me make a plea for an unconventional structure for this unconventional paper: I believe that the contribution of this paper is to provide a coherent discussion of the role of contacts and

network social capital in the labor market, narrated by Figure 1 and supported by each of the empirical analyses. To some degree, this comes down to a difference between a paper showing positive findings, and a paper, such as this one, that makes a case for non-findings in an already established literature. If, for example, I only present one of the nodes in Figure 1 (i.e. retesting the effect of contact's status in point I), then an easy response of a would be critic would simply be to note that I didn't test for the longitudinal effect of contacts (point G) or endogenous social capital models (point L) etc. I accept the possibility that critics might "quibble" with the findings. After all, might one not "quibble" with the estimation and results of many of the published positive findings in this literature? I firmly believe that all the results in the paper would hold up with extensive quibbling, that is why I will make all the data and command files public. The main point is to put out a dissenting view on this topic along with a unifying discussion attempting to explain the non-findings.

Perhaps an ideal case would be, as suggested by the reviewer, separate papers for each of the nodes in Figure 1, and then a single paper tying all the results together. My opinion is that it is difficult to get null results published (regardless of the scientific merit in doing so) and that the papers based upon the individual nodes of Figure 1 could not stand alone and would have to be combined together, as in this paper, under the rubric of some overarching discussion.

B8. The reviewer notes that the single-firm studies do not make claims to generalizability.

That is correct. I do not doubt the results from the single firm studies reviewed on pages 4-5 of the paper. These are excellent papers and they are, I believe, a major step forward. That is exactly why they make the null results in the existing literature puzzling. As suggested in the answer to question B6 (second paragraph), I think it is possible that better-connected workers obtain work more rapidly and receive higher wages, but that current measures of well-connected networks are not picking this up.

B9. The reviewer suggests that another way to reconcile the findings from the single firm studies with supply side data is that there could be competing mechanisms that cancel out, such as employers who avoid referrals.

That is certainly possible. I guess that a way to test it would be a multi-firm study where several firms are randomly selected.

B10. The reviewer argues that we would be better off if we had measures of job offers, and that social capital is a proxy for the quality of the distribution of the offers.

The reviewer is on target with this point--based on Montgomery's search model approach, what we would like to have is the frequency and distribution of the offers via formal and informal methods. However, reality is a bit more complicated. The data actually exists. The NLSY data has information on the number of job offers received and the wages of offers that were not accepted. The interesting finding is that very few offers are actually rejected (something like 10-15% if I remember correctly). One way to interpret this is that workers search sequentially from high wage (or highly desirable) jobs to lower wage jobs. One needn't have perfect knowledge about wage offers to see how this might be true; an employed academic on the job market doesn't apply randomly to all jobs but to several jobs which are likely to be "acceptable" in terms of wages and working conditions. So, the story would be as follows: workers use multiple methods to search for job leads, workers then rank the job openings from high to low based on expected wages and apply sequentially working down the list, accepting the first offer they receive. We could call this model the rank order sequential search model. The economists' sequential search

model should be updated to take the low rate of rejecting offers into account. However, for my purposes (i.e., the proposed test of causality) it doesn't make any difference which of the three models you choose (sequential search, extensive search, or rank order sequential search). If better-connected workers are more likely to hear about jobs (or more likely to hear about high-wage jobs) than poorly-connected workers then they will be more likely to obtain their jobs through contacts. [I could work this out for a rank-order model, but it should be evident that the proof would be similar to the extensive and sequential models in Appendices A and B].

In short, we would want information about all jobs they hear about, not just job offers. I could add this complexity into the paper, but it seems deserving a more complete treatment in a separate paper.

B11. I added the Korpi citation.

B12. The reviewer points out that the correct specification of the test of causality is important.

I agree. I include the test because I am concerned about the correct specification of labor market social capital models. The specification for the models of the probability of using contacts is the same as models of the effect of social capital on wages. If a researcher believes that he/she has the specification right to estimate the “effect” of network social capital on wages, then the same specification holds for the effect of network social capital on the probability of using contacts. I believe there should be a high bar for inferring causality from the coefficient on social capital variables, and I think the burden of proof of proper specification should lie with the researcher who wants to show positive effects.

Nonetheless, the basic assertion of Proposition 1 that better-connected workers should be more likely to use contacts is fairly straightforward empirically, and, I would argue, robust to different specifications of the search model. Although the prediction is derived from search models that make use parameters unobserved by the researcher (the distribution and arrival rate of wage offers, for example), testing whether a social capital variable is consistent with Proposition 1 only requires the observed variables (wages, use of contacts, measures of network social capital).

B13. Layoffs vs. plant closings & control variables in Table 3.

Addressed in B6 above.

B14. The reviewer argues that the discussion of exogenous social capital models (point I in Figure 1) is too brief to be convincing.

I think the evidence is pretty clear, but I present further evidence below that should make it even clearer. See the response to question C4 below.

B15. The reviewer is concerned that there could be race/ethnic interactions in the use of job contacts.

In Table B3 of this memo, I retest model 3 of Table 7 separately by race/ethnic group for black, white, Mexican, and Puerto Rican workers. As in Table 7, none of the social network measures have a significant effect on the probability of using contacts in Table B3. My results differ from those of Reingold (1999) for whites and Puerto Ricans, although the models are slightly different. The point is moot, however, because none of these variables in Reingold (1999),

mean education of friends and % employed, has an effect on wages in my models. The reviewer is correct to note that a detailed follow-up using the UPFLS and MCSUI data could be the subject of a full paper.

B16. The reviewer suggests that the best way to reconcile the single firm and supply side studies would come from data on job offers.

I agree, with the caveat that we would want information on all job leads, not just offers (see the response to B10 above).

Reviewer C

C1. The reviewer notes that there are major differences in the firm based research based on how the data is collected.

This is correct. The single-firm studies reviewed on pages 4-5 are unique because they are studies of all applicants for particular positions. In this way they avoid the problems the reviewer discusses in this paragraph. However, as noted by reviewer B, they do not make a claim for being representative of the labor market as a whole. With more space, I could have elaborated on the differences with other types of firm based studies. However, given the space constraints imposed on this paper, I don't think my discussion of the single firm studies detracts from the subsequent analysis in the paper.

C2. The reviewer argues that the hypothesis that workers with more social capital will be more likely to use contacts to obtain work seems to contradict prior results that informal ties are not used to find high-wage, high-prestige jobs.

I find this question a bit puzzling because the prior results (I assume that the reviewer is referring to the null results in the literature on the direct effect of contacts and wages on prestige, i.e., Model 1 of Table 2) do not indicate that informal ties are not used to good jobs. The prior results merely indicate that contacts are not more likely to result in high-wage jobs than not using contacts. I.e., the coefficient on contacts in Model 1 of Table 2 is -0.009. Indeed, Granovetter's seminal work on job contacts was a study of white-collar workers in Boston. Many good jobs are found through contacts.

Even if it is true that low-wage jobs are more likely to be filled using contacts, which might bias an otherwise positive effect of contacts down towards zero, the models in Table 7 account for this by controlling for the % of jobs in the respondent's 3-digit occupation that were filled using contacts (see appendix C on the NLSY data for information on the construction of this variable). Hence, Table 7 says, net of occupation differences in the probability of using contacts, do workers with better connections use them more often?

C3. The reviewer argues that "once it is accepted that informal ties are not used to gain the most favorable jobs in the labor market" then if there is some relationship between social capital and labor market outcomes "there is no logical way that those with more social capital would be more likely to use social ties to find jobs."

Not true. First, as indicated by the response to C2, I don't think it is true that informal ties are not used to obtain good jobs. However, even if contacts were less likely to result in the most favorable jobs, as the reviewer claims, there would still be a relationship between good

connections and using contacts to obtain work. Before I begin, let me note that key point #2 above differentiates between network social capital and other forms of social capital that do not pertain to information and influence from job contacts.

A simple example illustrates that it is perfectly logical that it is perfectly logical that better-connected workers would be more likely than poorly connected workers to use contacts even if, on average, contacts led to lower paying jobs:

1) Pretend that there are two types of jobs, those paying \$1 an hour and those paying \$2 an hour. Let's say that, for job offers coming from formal methods of job search, there is a 50% chance of getting each kind of job. However, for offers coming via contacts, there is a 75% chance of getting a \$1 job and a 25% chance of getting a \$2 job.

Table C1

Type of job	Search method	
	Formal search	Contacts
\$1 per hour	50%	75%
\$2 per hour	50%	25%
	100%	100%

2) There are two workers, A, who is well-connected and B who is poorly connected. During a job search A receives one job offer via contacts, and B has a 50% chance of receiving a job offer via contacts. Both workers receive one job offer via formal methods.

Here is the matrix of possibilities when A and B receive two offers, one via formal methods and one via contacts:

Table C2

Formal search	Contacts		Row totals
	\$1/hr	\$2/hr	
\$1 per hour	37.5%	12.5%	50%
\$2 per hour	37.5%	12.5%	50%
Column totals	75%	25%	100%

3) If you choose the highest offer you receive, and ties are decided randomly, then the average wage from receiving an offer via formal search and contacts in Table C2 would be \$1.625, and the probability of using contacts would be .375. In Table C2, the average wage of jobs found through contacts is \$1.5 $[=(.5*.375*\$1+.5*.125*\$2+.125*\$2)/.375]$, and the average wage of jobs found via formal search is \$1.7 $[=(.5*.375*\$1+.5*.125*\$2+.375*\$2)/.625]$.

In contrast, if you only get an offer via formal search, your average wage is \$1.5 and your probability of using contacts is 0. Because B is poorly connected and gets an offer via contacts only 50% of the time, his/her wages would be $(1.5 + 1.625)/2 = 1.563$, and his/her chance of using contacts .188.

Overall, the average wage of jobs found through contacts is \$1.5 and the average wage of jobs found through formal search is \$1.65 $(=.75*1.7+.25*1.5)$

4) To summarize:

Table C3

Worker	Average wage	Probability of using contacts	
A (well connected)	1.625	.375	
B (poorly connected)	1.563	.188	

Average wage of job offers found through contacts = \$1.25

Average wage of job offers found through formal search = \$1.5

Average wage of all jobs found through contacts = \$1.5

Average wage of all jobs found through formal search = \$1.65

Although the average wage of job offers and accepted jobs from contacts is lower than those from formal search, the better connected worker, A, has higher wages and a higher probability of obtaining work via contacts than the poorly connected worker, B. This proves that it is logically possible to have contacts, on average, lead to lower paying jobs while stilling having a positive effect of “social capital” on wages and the probability of using contacts to obtain work. The key point here, of course, is that more information, more job leads, more job offers is always a good thing. Everything else being equal, the better-connected worker will have more information etc. about jobs than the poorly connected worker.

C4. The reviewer argues that there is a methodological problem with the analysis of point I of Figure 1 (Table 5). He/she argues that in order to test whether contacts with higher prestige people lead to better job offers, I should test the effects of having contacts whose occupational prestige is higher than the respondent looking for the job using a categorical variable rather than use a continuous variable.

I have re-estimated the models in Table 5 to take this criticism into account. However, I would like to make two points before I discuss this additional analysis. First, my parameterization of the models in Table 5 follows the exact specification of Marsden and Hurlbert (1988) and closely follows the work of Lin and colleagues. The reviewer’s criticism is more appropriately directed at the existing literature on this subject, which I am also critiquing. Second, the reviewer seems to miss my methodological critique of this literature: The large proportion of same-occupation ties—i.e., contacts who are in the same occupation as the respondent—will induce a positive correlation between the respondent’s prestige and the contact’s prestige even if there is no causal effect of contact’s prestige on respondent’s prestige.

Nevertheless, the reviewer has made a good suggestion by noting that a good way to test whether the contact’s prestige is related to the respondent’s job outcome is to make a categorical variable for higher and lower status contacts. Table C4 breaks the contacts into 3 categories depending on whether the contact’s status was higher, equal, or less than the prestige of the respondent’s previous job (i.e., the job prior to the one the contact helped him/her get). In 35 cases the prestige was equal, in 142 cases the contact’s prestige was higher (an average of 13.5 points higher) and in 42 cases it was lower (10.75 points lower).

Next, Table C5 shows the average prestige of the respondent’s current job (the job found via the contact) according to whether the contact had a equal, higher, or lower prestige than the respondent. It turns out that equal prestige contacts resulted in higher prestige jobs than contacts with higher or lower prestige. Descriptively, at least, this does not support Lin’s theory. Granted, as discussed above, this data is old, but so is Lin’s Albany data. What happens when we control for other variables, such as the prestige of the previous job?

Table C6 shows that after we control for the prestige of the respondent’s prior job, the negative effect of having higher status contacts disappears—but note that the effect is essentially zero, not positive, as Lin’s theory would argue. According to Models C6.1, C6.2, and C6.3, there is no benefit from having higher status contacts.

I have added Model C6.2 to Table 5 as Model 4, along with a footnote in the text on page 11 regarding the results from Models C6.1 and C6.3.

C5. The reviewer argues that the labor market is segmented into jobs where informal social ties will lead to employment and a segment that is not, and that conventional social capital measures are weighted in the direction of the latter part of the labor market.

This goes back to the earlier discussion of network social capital. I would argue that if we think the information and influence that contact networks provide matters, then it doesn’t make sense to say that you can’t find good jobs via contacts. More information and influence is always an asset. Again, however, as described above I make no claims about being able to adjudicate the role of non-network social capital.

As for the claim that the labor market is segmented into a part where contacts matter and another part where contacts do not matter, I don’t think that makes sense. Table C7 shows the data from the 11,000 odd cases of the NLSY data from 1994-1998 NLSY data on the use of contacts in census 3-digit occupations (1970 codes), which is discussed in Appendix C of the paper. The data have been aggregated up to the categories provided by the census. It is true, as noted in the text, that blue-collar jobs such as craftsman, operatives, and laborers tend to use contacts more frequently than professional, managerial, and technical workers. But the difference is not so great to suggest that contacts are not used in white-collar occupations.

Table C7: The use of contacts by occupation category, 1994-1998 NLSY data (see Appendix C)

occ_cat	cases	contacts	pct
professional & technical	1870	251	.1342246
managers & administrative	1125	209	.1857778
sales	510	89	.1745098
clerical and unskilled workers	2128	366	.1719925
craftsman and kindred workers	1258	322	.2559618
operatives	1155	283	.2450216
transport equipment operatives	562	152	.2704626
laborers	874	230	.2631579
agricultural	73	28	.3835616
service workers	1981	434	.2190813
private household workers	50	21	.42

Again, note that the analysis in Table 7 uses this data to control for the occupation specific propensity to use contacts to find work. Even if the labor market were divided into segments that did and did not use contacts, controlling for the occupation level use of contacts would account for this.

C6. The reviewer argues that what is needed are more descriptive analyses concerning who uses informal contacts, what types of jobs are found with contacts, and whether and to what extent those who have social capital actually use it to find jobs.

I agree that good descriptive analysis can be very informative. However, if obtaining work via contacts is an endogenous variable—I.e., if it depends on the relative job opportunities obtained through multiple methods of job search—then one must think seriously about what sort of descriptive analysis would be useful. I believe that a data set which combined the NLSY's information on job search (methods used, offers received, time spend searching etc.) with social network information (measures of the extensiveness of job contact networks) might help us depict the relationship between network social capital and the use of contacts to find work.

Reviewer D

D1. The reviewer argues that the paper is overly long and poorly structured, and suggests that the Figure 1 flowchart be moved to the beginning of the paper.

As noted above, I cut 7 of the first 21 pages and reorganized the review of the literature and theoretical models. This makes the paper much shorter and puts the Figure 1 flowchart on page 15 of the paper. I agree with the reviewer that Figure 1 is where things start to get going in the paper. I hope the new structure is acceptable.

D2. The reviewer notes that readers unfamiliar with formal logic may find the language in Proposition 1 difficult, and proposes an alternative wording.

I have no problems with that alternative wording, although it is not as concise as the way it is right now. I will leave this decision up to the editors; either is fine with me.

D3. The reviewer notes that even if the individual accepts the first offer, the probability of using contacts would vary with social capital.

This is a very good point. This narrows the conditions under which the use of contacts is exogenous to social capital because the probability of receiving an offer must not be affected by network social capital, only the wages of offers (see the discussion on page 14).

D4. The reviewer notes that social capital measures should include the number of friends rather than then the truncated networks of the UPFLS and MCSUI data.

I agree. The UPFLS and MCSUI data are the best we have, but I think the next step in this line of research will be data on the extensiveness of job networks. This will be slightly different than questions that ask how many friends and acquaintances you have, because what we will want to get at are the number of occupational contacts one has. I.e., how many other janitors does a janitor know etc.

D5. The reviewer asks about the effects of search intensity and wonders whether workers with good or bad networks would choose higher search intensity via contacts.

This is a good question. Here is the way I would think about it. Let the level of network social capital be the # of friends you have, and the intensity of search be how often you ask them about jobs. In this case, it might be true that someone with few contacts would benefit more than someone with many contacts by checking in with them frequently. However, even if the

marginal returns to search intensity were greater, the returns, one would expect, would be declining. I.e., checking in with your contacts every day will yield little additional benefit compared to checking in once a week. Thus, workers with more contacts will still have higher network social capital than workers with fewer contacts. Thus, the reviewer is right that it could go either way, but it will not overturn the logic of Proposition 1b.

Alternatively, one might imagine an individual deliberately attempting to expand his/her contact network (i.e., MBA students). The marginal returns for accumulating contacts would be higher for those with fewer contacts to begin with. However, if network social capital were measured correctly, then the increase in contact size or quality would be directly accounted for in the analysis.

D6. The reviewer suggests that a deductive model (“partial-partial” equilibrium) could be derived to help reconcile the apparent importance of referrals in employer-side studies with the non-findings on the workers’ side, similar to the equilibrium search models in economics.

This is a very good point. While the standard sequential search model assumes an exogenous distribution of wages, equilibrium search models in the economics literature (i.e. van den Berg and Ridder *Econometrica* 1998) attempt to treat the wage distribution as an endogenous variable. I.e., if you have both sides of the labor market interacting simultaneously in the same model, would you still expect to see a distribution of wages? A similar question could be posed of the use of employee referrals by employers and workers: if both workers and employers are simultaneously choosing whether to use formal methods or referrals to hire/look or work, under what assumptions will contacts be used to find work? Fortunately (for me), this model has already been done (Montgomery 1991. “Social Networks and Labor-Market Outcomes: Toward an Economic Analysis” *American Economic Review* 81:5). Montgomery shows that, in the presence of social homophily, firms will be more likely to hire friends of productive workers (i.e. employee referrals are more likely to get the job) and that workers who are well-connected will do better than those who are not well-connected.

However, I think that my case is a bit different. Because we are beginning with the finding/assumption that employee referrals seem to get hired at higher rates than non-relatives (the results of the single firm studies), what remains to be shown is that if this is true then it should show up on the worker side of the equation. In other words, all we need is a partial equilibrium model rather than a partial-partial equilibrium model.

If contacts do matter on the employer side of the labor market, then the response to Question 1 below on page 17 argues that the current models of the paper (the sequential and extensive search models) show that it should show up on in worker data.

D7. The reviewer says that he/she is confused by my assertion that Montgomery’s model provides a basis for interpreting the discrepant findings between the firm and worker data, because he/she claims I have already rejected Montgomery’s model.

I have not rejected Montgomery’s multiple-method model. Indeed, I think it is the only way to reconcile the various findings discussed in the paper. The indirect test of causality (Proposition 1) checks the validity of particular network social capital variables under the framework of Montgomery’s model. Proposition 1 doesn’t make sense without some sort of multiple method search model. I think that Montgomery’s model is the right way to conceptualize job search;

what we need to do next is think about what sort of network data will measure network social capital rather than unobserved ability reflected through social homophily.

D8. In the final paragraph, the reviewer says that he/she agrees with the discussion in the paper that it is difficult to accept that social networks do not matter in the job-search process and wonders if we have the questions right.

I agree with this statement. I think social networks do matter in the labor market and that the reason we haven't proven it is because we are not getting the questions right (or the answers to questions posed by the non-random nature of social ties). It is in the context of eventually getting the questions right that I see the purpose of this paper. I know that null results and non-findings are not popular, but I think they have scientific merit when they help to redress the bias towards publishing positive results. Given the importance of the topic to sociological models of the labor market, hopefully this paper will provide a good starting point for future research to show more conclusively that contacts do matter. As argued in the paper, the existing evidence does not tell us how much contacts and contact networks matter, the prevalent use of contacts to find work notwithstanding.

Reviewer E.

E1. The reviewer suggests that the paper is not as sharp as it could be and that I need to get to my test of causality and Figure 1 in a hurry.

I went through the paper carefully sentence by sentence to make it sharper and more concise. The discussion of the test of causality now begins on page 8 rather than on page 15 in the previous version. As the reviewer suggested, I have removed portions of the text that explain in detail why the non-random nature of social ties poses an estimation problem, and have instead directed the reader to the existing literature on the subject. I think that the current version of the paper is the right length (31.5 pages of text) given the amount of ground covered. However, I will be happy to cut or shrink the paper further in response to specific instructions about what should be changed.

Question 1: What are the implications of the single-firm studies mentioned in the paper? If firms are more likely to hire workers with contacts inside the firm, should this effect show up on labor market surveys of workers?

Yes. If contacts inside the firm can provide information or influence that leads to job offers, then workers who are better connected (i.e., who have more contacts inside firms) will tend to get more job offers than other workers. If you tend to get more job offers, this increases your wages and/or decreases the time needed to find work.

In terms of the sequential and extensive search models discussed in the paper, this means that that probability of receiving an offer via contacts, P_{SC} , is higher for better-connected workers.

Appendices A and B prove that increasing P_{SC} leads to higher wages. For the sequential search model, increasing the arrival rate of offers also leads to shorter periods of unemployment duration (see Devine and Keifer 1991). **As far as the sequential and extensive search models go, the basic proof, then, is already in the paper:** if some workers have a higher probability of receiving job offers via contacts (because they have more, or better, job contacts) then they will have higher wages and/or they will find work more quickly.

As argued in the paper, an alternative explanation, consistent with the findings of the single-firm studies, would be that workers are equally well connected. In this case, the hiring rate of employee referrals may higher, but each worker would have an equal chance of being an employee referral. Therefore, workers themselves would not have different rates of obtaining job offers through contacts, P_{SC} .

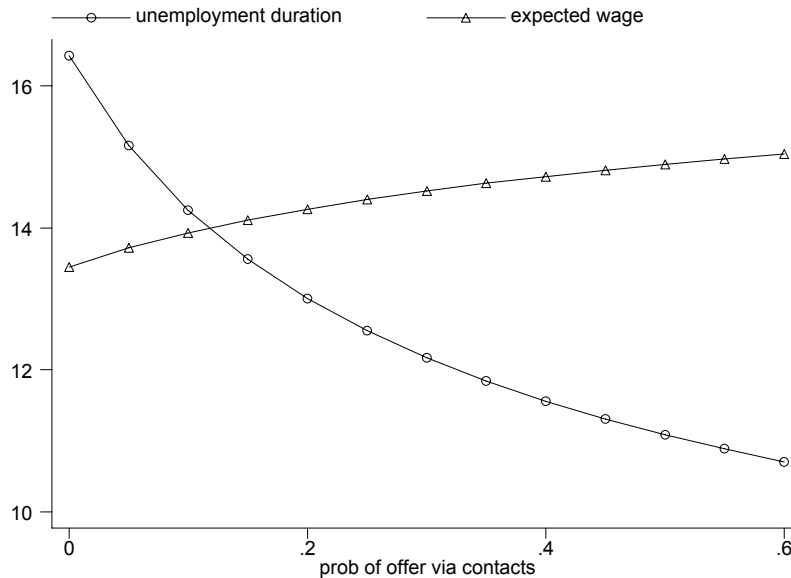
Some examples may help.

1) Sequential search.

Graph 1 (on page 18) uses numerical calculations from Mathcad to show what happens to the expected wage and average unemployment duration (in periods of search) when the probability of receiving an offer each period via contacts increases, holding everything else constant. In this example, the formal and informal offer distributions are normal variables with means of 10 and standard deviations of 1. The probability of receiving an offer via formal methods is set at .2 per period, and the future discount rate is .99 (b in Equation A1 in Appendix A). In Graph 1, the probability of receiving an offer via informal methods (p_c) varies from 0 to .6 per period in increments of .05. Graph 1 shows that the expected wage increases and unemployment duration decreases as p_c increases.

What is true in this specific example holds true in general. The predictions of the sequential search model are unambiguous. If p_c increases, expected wages increase and unemployment duration increases. The upshot of this result is that if contacts within the firm matter (as suggested by the single firm studies) then better connected workers should have higher values p_c of than poorly connected workers. As indicated by Graph 1, this will affect average wages and unemployment duration.

Graph 1



2) Extensive search.

A and B both search for work, but B has more job contacts than A. A has 10 job contacts while B has 20. For each worker, a glance at the newspaper reveals 10 job openings. In addition, at any one time 50% of A and B's friends know about job openings in their firm. Let's say that, on average, 10% of applications from newspaper ads result in job offers, while 20% of applications via employee referrals result in offers. Therefore, on a representative job search, both A and B would average 1 job offer from newspaper ads, A would average 1 offer via referrals (10 contacts * .5 * .2) while B would average 2 via referrals (20 * .5 * .2).

If we assume that wages are distributed uniformly from 0 to 1 (a convenient—but not necessary—assumption, but the distribution of the highest wage is much more complicated with other distributions), then the expected wage of the highest offer is $\frac{n}{n+1}$ where n is the number of job offers received. Hence, in this representative search, A obtained 2 offers and would have an expected highest wage of .67, while B obtained 3 offers and would expect a wage of .75. In the extensive search model, workers are trying to maximize wages, and because they are choosing the highest offer from all the offers they receive, having more offers is always better than having fewer.

Notice with this example that if both workers are equally well connected (10 job contacts each), then they would have the same expected wage despite the fact that the probability of getting a job offer from a job opening via contacts was higher than an opening found through the newspaper (.2 vs. .1).

3) "Take the first offer" model. It could be argued that workers attempt to get work quickly rather than maximize wages, and hence the extensive search model is misleading. However, even if workers try to minimize unemployment duration by taking the first job offer they receive, the benefits of being well connected will still be apparent.

For example, imagine workers A and B again. Let's assume that there is one new job opening each day in the newspaper, and that there is a .05 chance that this results in a job offer. In contrast, each day there is a .1 chance that each friend will hear about an opening in his or her firm and a .2 chance that this opening will result in a job offer. I.e., there is a .02 (= .1 * .2) chance per day that per friend of receiving a job offer. Assume that A has 10 friends and B has 20 friends.

What is the daily chance that A and B will obtain a job offer?

The daily chance of getting a job offer is: $P = .05 + [1 - (.98)^N]$ where N is the number of friends. This is .233 for A and .382 for B. If A and B take the first offer they receive, the expected duration of is 1/P or 4.3 days for A and 2.62 days for B.

The basic point of these three examples is that if contacts matter and some workers have more job contacts than others, then better-connected workers should either have higher wages and/or shorter periods of unemployment than poorly connected workers.

In the sequential search model the worker tries to maximize income, rather than maximize wages or minimize unemployment. The reservation wage balances the expected income over the life of the job obtained by taking the current job offer versus rejecting it in hopes of getting a better job offer, but incurring another period of unemployment without income. In this sense, the sequential search model falls in between the extensive and the "take the first offer" models, predicting an effect on both wages and unemployment duration.

The sequential and extensive search models indicate that the results from the single-firm and worker data should be consistent with each other. As mentioned in other parts of the paper and memo, here are three reasons why they might not be consistent.

- 1) All workers are equally well-connected (see page 5 of the paper).
- 2) What holds true in some firms may not hold true for all firms overall. Firms that keep records on whether or not an applicant employee referral may be a selective sample, and it is possible that the employee referrals do not affect hiring probabilities much overall.
- 3) We have poor measures of network social capital (see the response to B6 in the memo).

My intuition is that the answer is #3.

Note that even if employee referrals have the same hiring rate as non-referrals, network social capital models would still predict that having more/better contacts is beneficial. To see this, simply set the hiring rate for referrals in example #2 above to .1, the same as non-referrals. Better-connected workers always benefit by having more information about jobs than poorly-connected workers, everything else being equal.

Note also my citation of Montgomery's (1991) model of the use of contacts by firms and workers in the response to D6 above.

Question 2: What if workers turn to contacts when they get desperate to find work?

It is possible that when workers get desperate for work they might turn to their contacts to obtain work quickly (such as when unemployment benefits run out). This might result in lower wages, but it would be a beneficial aspect of having good contacts. This could bias the coefficient on contacts downward in wage regressions. Is there any evidence of this?

The 1986 wave of the NLSY has a module on job search methods used to look for work during each month of an individual's job search. This data can be used to see whether workers increasingly turn to friends and relatives to look for work as unemployment duration increases.

Each graph in Graph 1 shows the proportion of respondents who searched for work with newspapers, friends and relatives, and direct contact, for each month of job search. The graphs are arranged by the overall length of the unemployment spell (i.e., the first graph is for those who had a total unemployment spell of 1 month, the second is for those with a 2-month spell of unemployment etc.). The number of cases for each graph is in the upper left-hand corner. The graphs show no evidence that the use of contacts to search for work increases with the duration of unemployment. For the 75 respondents who searched for work for 6 months, the proportion who used friends and relatives to search for work by month is similar to the proportion who used newspapers and direct contact.

At least with respect to unemployment duration then, there is no evidence that the use of contacts to search for work goes up the longer one has been unemployed.

Table A1: Use of informal search methods to look for work (friends, relatives, or acquaintances), MCSUI data (see appendix C), same sample as Table 8.

Searched for work using friends, relatives, or acquaintances	Percent
No	23.36
Yes	76.64
(N=1,434)	

Table A2: Total number of other search methods used to look for work among those workers who searched using contacts. MCSUI data.

# of methods used other than friends, relatives, and acquaintances	Percent	Cumulative Percent
0	9.61	9.61
1	16.64	26.25
2	25.18	51.43
3	18.87	70.30
4	17.94	88.24
5	8.33	96.57
6	2.24	98.81
7	1.03	99.84
8	0.16	100.00
(N=1,099)		

Table A3: Search method used by workers who did not search using contacts. MCSUI data.

newspaper	.626506
union	.0122699
state employment agency	.1595092
school	.1257669
help-wanted signs	.231003
temp agency	.1507692
private emp. service	.095679
walk-in	.4847561
send resume or call	.3914373

(N=329)

Table A4: Use of multiple search methods, MCSUI data.

	(1)	(2)	(3)
Model	OLS	OLS	Logit
Sample	All cases from table 8 who searched w/ friends	All cases from Table 8	All cases from table 8 who searched w/ friends
Dependent variable	# of methods other than friends	# of methods other than friends	Searched using newspaper ads
Female	-0.176 (0.100)	-0.183 (0.082)*	0.251 (0.140)
Race (omitted category: White)			
Black	0.412 (0.154)**	0.231 (0.124)	0.509 (0.226)*
Hispanic	-0.477 (0.158)**	-0.337 (0.126)**	0.043 (0.218)
Asian	-0.002 (0.387)	-0.283 (0.253)	0.159 (0.541)
Other	0.270 (2.941)	0.042 (2.498)	-0.244 (4.150)
Education	0.063 (0.023)**	0.073 (0.019)**	0.074 (0.032)*
City (omitted category: Atlanta)			
Los Angeles	0.439 (0.155)**	0.470 (0.124)**	0.330 (0.208)
Boston	0.585 (0.162)**	0.493 (0.128)**	0.719 (0.222)**
Social Network Measures: (mean)			
education	-0.075 (0.029)**	-0.080 (0.023)**	-0.087 (0.041)*
(mean) steady job	-0.024 (0.181)	-0.009 (0.142)	0.227 (0.250)
(mean) welfare	0.030 (0.342)	-0.099 (0.254)	0.386 (0.502)
% in 3-digit occupation using contacts	0.581 (0.596)	0.634 (0.514)	-0.152 (0.831)
Constant	2.380 (0.438)**	2.123 (0.368)**	0.185 (0.610)
Observations	1026	1434	1025
R-squared	0.05	0.04	

Standard errors in parentheses

* significant at 5%; ** significant at 1%

Table B1: Alternative Models for Table 4 (NLSY data),

Model Variable	(1) log wage	(2) log wage	(3) log wage
Contact: Used Friend or Relative	0.024 (0.016)	0.011 (0.015)	0.005 (0.015)
Search ¹ :			
Unemployed Search	-0.075 (0.013)**		
Employed non-Search	0.042 (0.017)*		
Unemployed non-Search	-0.032 (0.022)		
Tenure	0.031 (0.006)**	0.031 (0.006)**	0.009 (0.005)
Tenure ²	-0.001 (0.000)**	-0.001 (0.000)**	-0.000 (0.000)
Experience	0.039 (0.035)	0.065 (0.035)	0.199 (0.033)**
Experience ²	-0.000 (0.001)	-0.001 (0.001)	-0.002 (0.001)
Year (1996)	0.101 (0.015)**	0.097 (0.015)**	
Year (1998)	0.178 (0.015)**	0.176 (0.015)**	
Constant	1.729 (0.248)**	1.479 (0.247)**	0.234 (0.211)
Observations (N)	7409 3281	7409 3281	7409 3281
R-squared	0.09	0.08	0.05

Notes:

* significant at 5% level; ** significant at 1% level

Table B2: Alternative models for Table 3, Log-Time of Unemployment Duration, NLSY data

Model	(B2.1)	(B2.2)	(B2.3)	(B2.4)	(B2.5)
Sample	All layoffs + plant closings	(1) - missing data	(1) - missing data	plant closings	All layoffs + plant closings
Dep. variable	Unemployment duration	Unemployment duration	Unemployment duration	Unemployment duration	Unemployment duration
Used contact	0.207 (0.099)*	0.228 (0.102)*	0.221 (0.102)*	0.243 (0.208)	0.208 (0.100)*
% in 3-digit occupation using contacts	-0.142 (0.375)	-0.169 (0.366)	-0.119 (0.362)	1.211 (0.920)	-0.111 (0.377)
(missing)	0.030 (0.124)			-1.188 (0.273)**	0.011 (0.126)
Female	0.047 (0.080)	0.123 (0.083)	0.133 (0.081)	-0.260 (0.172)	0.038 (0.080)
Race: Black	0.141 (0.162)	0.102 (0.164)	0.112 (0.163)	0.198 (0.392)	0.146 (0.162)
White	-0.046 (0.142)	-0.086 (0.144)	-0.092 (0.144)	-0.152 (0.338)	-0.039 (0.142)
Education	-0.046 (0.019)*	-0.071 (0.020)**	-0.073 (0.020)**	0.011 (0.047)	-0.046 (0.019)*
Experience	-0.000 (0.001)	0.000 (0.001)		0.002 (0.002)	-0.000 (0.001)
Experience^2	0.000 (0.000)	-0.000 (0.000)		-0.000 (0.000)	0.000 (0.000)
Plant closing (excluded category: layoff)					-0.103 (0.093)
Constant	3.781 (0.366)**	3.973 (0.374)**	3.957 (0.295)**	2.017 (1.001)*	3.782 (0.367)**
Observations	733	665	665	151	733

Standard errors in parentheses

* significant at 5%; ** significant at 1%

Table B3: Alternative Models for Table 7, the probability of using contacts, UPFLS data, by race.

Model	(B3.1)	(B3.2)	(B3.3)	(B3.4)
Sample	Blacks	Non-Hispanic Whites	Mexicans	Puerto Ricans
% Previous use of contacts	0.489 (0.262)	1.100 (0.462)*	1.231 (0.354)**	1.528 (0.425)**
% Friends w/ same job	-0.839 (0.344)*	-0.850 (0.545)	1.475 (0.656)*	0.096 (0.686)
% Relatives w/ same job	0.732 (0.528)	-0.501 (0.773)	-0.679 (0.672)	-1.181 (0.853)
Female	-0.534 (0.201)**	0.263 (0.333)	0.277 (0.283)	-0.105 (0.357)
Education	-0.152 (0.060)*	0.133 (0.088)	-0.038 (0.038)	-0.047 (0.067)
Social Network Measures: (mean)				
unemployment	-0.025 (0.442)	-0.380 (0.832)	-0.378 (0.640)	-0.912 (0.836)
(mean) education	-0.047 (0.069)	-0.116 (0.099)	-0.048 (0.047)	-0.036 (0.080)
(mean) welfare	0.467 (0.509)	-1.265 (1.269)	-0.949 (1.582)	0.239 (1.185)
% in 3-digit occupation using contacts	2.601 (1.314)*	1.011 (2.047)	-0.803 (1.723)	7.373 (2.607)**
Dummy variables for missing data ⁺⁺	Yes	Yes	Yes	Yes
Constant	1.534 (1.031)	-1.284 (1.410)	0.534 (0.702)	-1.132 (1.176)
Observations	546	207	313	199

Standard errors in parentheses

* significant at 5%; ** significant at 1%

Table C4: Contact's prestige relative to the prestige of the respondent's previous job, DAS data

Prestige difference, (Respondent minus Contact)	Mean difference	Number of cases
same prestige	0	35
contact higher	-13.5	142
resp. higher	10.8	42

Table C5: DAS data, descriptive effect of high/low prestige contacts on respondent's occupational prestige

Prestige difference, (Respondent's previous job minus contact's prestige)	Respondent's occupational prestige, new job
same prestige	47.0
contact higher	39.8
respondent higher	38.8
Total	40.7

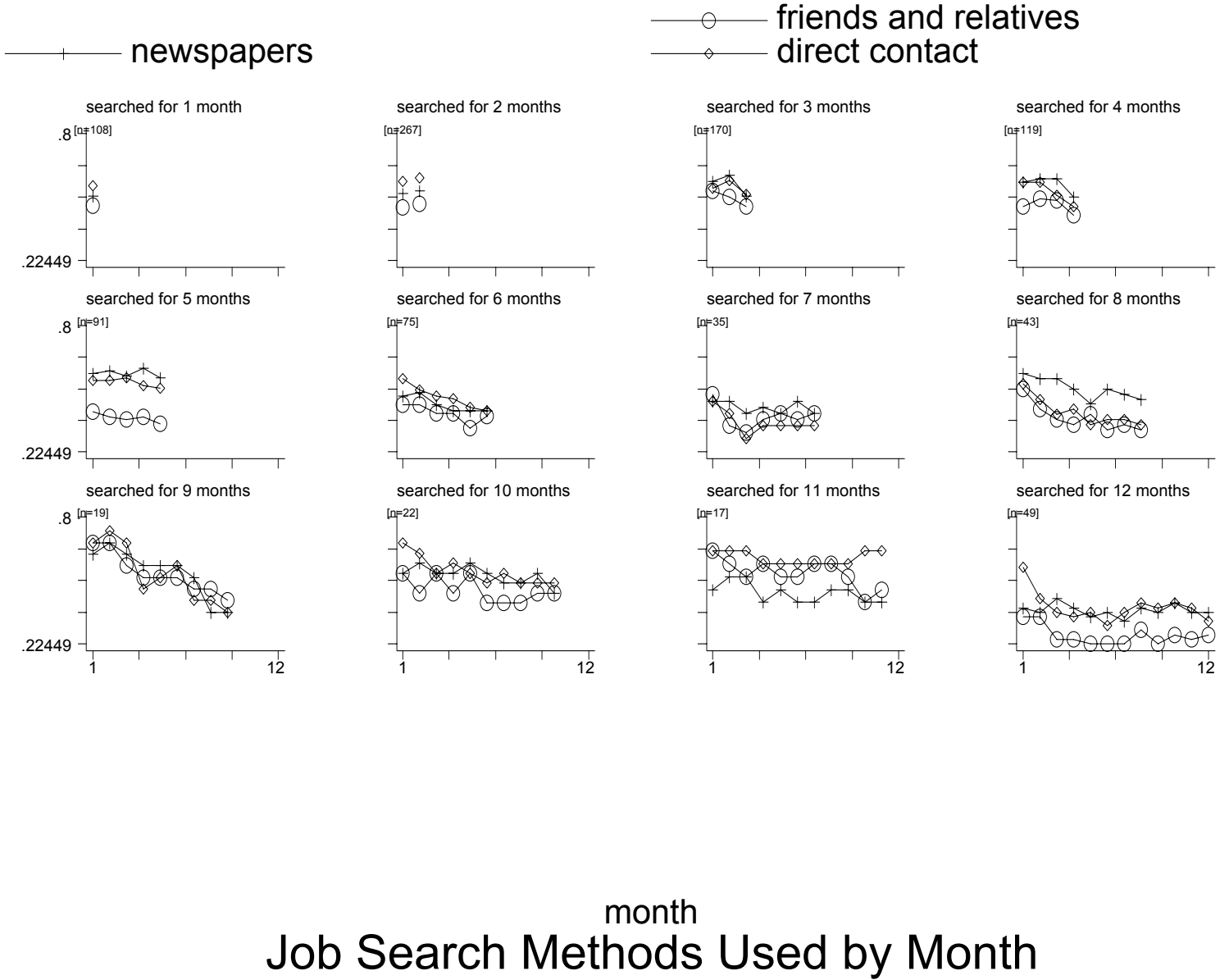
Table C6: Alternative Models to Table 3. DAS data, "effect" of high/low contacts on occupational prestige.

Model	(C6.1)	(C6.2)	(C6.3)
Dependent variable	respondent's occ prestige	respondent's occ prestige	respondent's occ prestige
R's previous job prestige – contact's job prestige (excluded category: equal)			
Contact higher prestige	-7.202 (2.318)**	-0.297 (2.084)	-1.168 (1.996)
Contact lower prestige	-8.180 (2.811)**	-7.910 (2.367)**	-7.078 (2.277)**
Prior Job Prestige		0.556 (0.059)**	0.415 (0.063)**
Father's Prestige			0.054 (0.061)
Education			1.282 (0.277)**
Tie Strength (1 = strong, 0 = weak)			2.204 (2.770)
Contact connected to firm			-0.952 (1.508)
Constant	46.966 (2.076)**	21.734 (3.187)**	8.929 (4.752)
Observations	219	219	219
R-squared	0.05	0.33	0.40

Standard errors in parentheses

* significant at 5%; ** significant at 1%

Graph 2



Note: Each graph shows the use of different methods of job search by month of job search for workers with a specific number of months of unemployment duration.

Text of social network questions. The first insert below is the text of the social network questions from the UPFLS (section R). The second insert is the text of the social network questions for the MCSUI data (section H). The MCSUI questions repeat for each of the 3 queried friends. These questions could be included as part of Appendix C of the paper, but because the data and codebooks are publicly available through ICPSR perhaps a better solution would be to post them to my web page along with the data and command files.