Memo to the reviewers #1, 9-20-2002. (Refers to an older version of the paper)

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

First, I would like to thank the reviewers for their comments. I believe that the revised paper is much improved because of the careful criticism of the reviewers.

I want to highlight the main changes in the current version:
1) Focus on Social Capital: The majority of the comments of the reviewers related to the social resources/social capital perspective on the role of contacts in the labor market. I have reframed the paper to focus on the social capital evidence. This is also reflected in the change in the title of the paper.

2) Test of Causality: Several reviewers noted that although the non-random nature of social network data makes the social capital results problematic, it might be unwarranted to dismiss the social capital results based solely on conjecture. I agree. A central part of this version of the paper is devoted to the question of the "causal" effect of social capital variables. I argue that information on the use of contacts can be used as a test (or a "check") of the causal effect of social capital variables on labor market outcomes (see Proposition 1 in the paper). I believe that this additional information on whether contacts where actually used to find work gives us leverage on whether "better connected" individuals actually benefit from their networks or whether the network measures of social capital are simply reflecting unobserved individual characteristics that affect the process of friendship formation. As detailed in the paper, I derive this test of causality from the sequential and extensive models of search.

3) Extensive search model in addition to the sequential search model: Reviewer B raised concerns about the behavioral assumptions of the economic model of sequential search (i.e., reservation wages, known wage distributions etc.). In this version of the paper, I also include a discussion of the extensive search model, which makes far fewer behavioral assumptions. The extensive search model assumes that workers send off applications, wait for the offers to come in, and then choose the best offer. I show that the proposed test of causality (point #2 above) holds for both the sequential and extensive search models. I believe that the proposed test of causality is not an artifact of the assumptions of the economic search model and that Proposition 1 (pages 15-21 in the paper) will hold in general for any model of search where job seekers seek to maximize income or wages and choose among multiple methods of search (i.e., using contacts and using formal methods).

4) Extended discussion of why nonrandom network data is problematic: I have attempted to clarify the argument for why nonrandom network data makes empirical estimation of social capital models difficult. As several reviewers point out, social homophily itself may be the basis for social ties and group (i.e., race, class, and gender) variation in access to good connections. As discussed in the text on pages 10-15, this does not negate concerns about the overestimation of social capital effects.

5) Flow-chart of alternative theories: In order to make the theoretical and empirical discussion clear and concise, I have organized the results section as logically as possible and summarized the narrative into the flow chart in Figure 1.

6) Data on unemployment duration: I have added data from the 1994-1998 waves of the National Longitudinal Study of Youth 79 (NLSY) on length of time it took unemployment workers to find jobs after losing their jobs through either layoffs or plant closings (Table 3, Model 2). This data is important if we want to test whether the purported benefits of contacts in single-firm studies are

true in general. If contacts are as beneficial for all workers as they appear to be in the single-firm studies (i.e., Fernandez and Weinberg 1997), then we might expect that workers who use contacts find work more quickly. If layoffs or plant closings represents an exogenous shock pushing workers into unemployment, then this will mitigate the problem of the selection of poorly connected workers into unemployment that might otherwise bias estimates of the effect of using job contacts on unemployment duration.

7) <u>Information on same-occupation ties from the Urban Poverty and Family Life Study (UPFLS):</u>
The results from the DAS data (Table 5) indicate that higher status contacts are not "better" than lower status contacts. However, the DAS data reveal that many jobs are found through same-occupation ties. It is possible that same-occupation ties are an advantage (a point made by reviewer D). In this revised paper, I have added data from the 1987 UPFLS (see appendix C for a full description of the data). The advantage of this data is we know the occupation (coded according to a 20-category variable) of friends and relatives. Therefore, we can test whether having friends in the same occupation represents an advantage.

In the following, I respond to each reviewer's comments point by point.

<u>Reviewer A</u>

<u>Main Points</u>
A1) The reviewer argues that because the paper cites almost no studies of significant gains from contacts, arguing that there is no evidence that contacts are beneficial does not represent a significant contribution to the literature.

Response: I took this comment very seriously. As noted above, in this version of the paper I emphasize the literature on social capital and labor market outcomes, where there is ample existing evidence of significant findings between the characteristics of the network or the contact person and the respondent's labor market outcomes.

A2) The reviewer argues that although the paper points out that there are critical problems in previous studies of the effect of social capital on labor market outcomes it is unable to resolve the problem, concluding that data limitations prevent me from discriminating between hypotheses.

Response: This is a good point. Although the previous version of the paper contended that nonrandom social ties made it difficult to determine whether the results of social capital models were real or spurious, I all I could do was wave my hands at it. In the current version of the paper, answering this question—is it real or spurious—is the principal focus of the paper. As noted above, I have developed a test of the causality of labor market social capital measures.

Second, the reviewer is correct to point out that I argued that the lack of data on same-occupation ties prevented me from making strong conclusions in the previous version. However, with the inclusion of the UPFLS this is not the case in the current version. In the previous version, I found that if you exclude same-occupation social ties in the DAS data, higher status contacts are not more beneficial than low status contacts. However, it was apparent that a significant number of job contacts were same-occupation ties, suggesting that this might be an important factor. The UPFLS has data on the occupation of social network members, so we can actually test whether having more same-occupation social ties is truly an advantage in the labor market. Tables 6 and 7 show that although same-occupation social ties are positively associated with wages, they have a negative effect on the use of contacts, indicating that the effect is not causal. [If you reject the

2

test of causality by arguing that the use of contacts is exogenous to social capital, then the lack of significance of the interaction also indicates a lack of causality, see point O in the text].

A3) The reviewer suggests that the paper would be better suited for ASR if it had more conclusive findings.

In this version of the paper I believe I do have "conclusive" evidence that the widely accepted social capital effects on labor market outcomes are misleading. At the same time, one should note that you cannot prove null findings in the same way that you can prove positive findings. The door is always open for a new survey with better network data to show the positive effects of (properly measured) social capital. But my point in the paper is not that social capital doesn't matter—indeed in the conclusion I argue that it does, at least in theory—but rather that the current measures of labor market social capital do not provide us with any indication of how much it does matter. I believe the contribution of this paper is to move the literature forward by encouraging future research that takes seriously the problem of nonrandom social ties.

Other comments

A4) Thanks for the suggestion to look at the Brock and Durlauf article from the Handbook of Econometrics ("Interactions-Based Models"). The models proposed by Brock and Durlauf do not, I believe, represent a panacea for the issues raised by the correlation of unobserved ability across friends. They rely on nonlinearities in the estimation of discrete choice models to identify social context effects. Moreover, in terms of Manski's (1993) language on social effects, Brock and Durlauf are concerned with differentiating between endogenous and exogenous social interactions. As Moffit (2000:24) points out, the presence of correlated unobservables makes it impossible to confidently estimate the existence of any form of social interaction, endogenous or exogenous. However, the reference to Brock and Durlauf's work was very useful in helping me clarify the issue. I have included a recent article by Durlauf that critiques existing empirical estimates of social capital in my discussion of spurious effects (page 14).

A5) I have taken the footnote about the alternative explanation for why Fernandez and Weinberg's results seem incongruous with the supply-side results (i.e., all workers are equally well connected) and placed it directly in the text.

A6) The reviewer wants a clearer discussion of why Granovetter and Fernandez and Weinberg obtain results about contacts that appear to differ so much from what the author finds.

Response: As for Granovetter, all of the review articles mentioned in the text point out that the subsequent empirical literature has not confirmed the hypothesis that the use of contacts per se is associated with higher wages. This fits with my findings in Table 3 Model 1. The reason for this, as I argue in the paper, is given in Montgomery (1992): if the use of contacts is endogenous to the level of social capital, then we wouldn't expect a strong relationship between the use of contacts and wages (well connected individuals have high wages regardless of whether they use contacts or other methods).

       As for Fernandez and Weinberg, I do not dispute their finding that employee referrals— in the firm they studied—resulted in higher employment probabilities. As noted in the previous comment, this may not show up in studies of workers if workers tend to be equally well connected. Page 36 in the discussion section addresses the single-firm studies in light of the findings in this paper. In addition, as pointed out on page 7, Montgomery's logic works here also. For any particular job, referrals may result in higher hiring probabilities. Workers who are better connected will only accept jobs from formal methods (i.e. direct application, responding to

newspaper advertisements) if they pay higher wages than informal job offers (if they are maximizing wages) or they get the job offer before they get a job offer from contacts/referrals (if they are trying to minimize the amount of time spend unemployed). As a result, in order to observe the overall effect of using contacts in worker data, we need to look and see the relationship between being "well-connected" and labor market outcomes. Hence the logic behind using social capital models.

A7) The reviewer asks whether limiting friendship networks to three people will lead to biases.

I expect limiting friendship networks to 3 people to lead to measurement error. This is limited network data. In general, measurement error will bias the effect down towards zero. However, it is important to note that even these crude measures of social networks have an effect on wages— but no concomitant effect on the probability of using contacts. Hence, I argue that the effect— even though it is undoubtedly diminished by measurement error—is not causal. Better network data would certainly ameliorate the situation. Again, though, the point of the paper is that these abstract measures of social capital such as the average education or status of network members are leading us in the wrong direction. The network data that gets it right will have to take seriously both the nonrandom nature of social ties and the actual means by which job information is transmitted in the labor market.

Reviewer B

Main Points

B1) The reviewer asserts that "the author criticizes a substantial body of literature verifying the contributions of social resources…Several demonstrations of the empirical indeterminism are offered. While…none of these demonstrations are conclusive, it is worthwhile to examine them here because they also demonstrate the fundamental orientations of the author's approach to this critique."

In this comment and many of the reviewer's subsequent comments his/her tone suggests that I am unfairly singling out the social resources/social capital literature as a target for methodological criticism. I would like to respectfully object to this sentiment. What I am doing in this paper is applying the well-established concern over correlated unobserved variables to the labor market social capital literature. The potential bias due to unobserved ability on empirical estimates of human capital, and quasi-experimental attempts to get around it, is well documented (see Card 1999, cited in the paper, for an extensive review). Methodological critiques of the peer effects and neighborhood effects literature, which is conceptually very similar to Lin's social resources theory, already exist (see Manski 1993 and Moffitt 2000 for reviews).

The substantial body of literature that the reviewer refers to does indeed show that there is a positive correlation between measures of social resources or social capital (see pages 8-10 of the text for a review). This is indeed a contribution to our sociological knowledge. However, this literature does not in any way demonstrate that this correlation is causal.

B2) The reviewer points out that the discussion of bias in Equations 1-4 (formerly in Appendix A, now on pages 12-13) makes the "convenient assumption that all network effects are measured," and wonders what would happen if the unobserved measure of social networks was also entered into the equation.

Response: This is a perfectly legitimate question, and I apologize for not being clearer about this in the previous version. Without doubt there is a lot of measurement error in the social capital measures, and one should take this seriously.

In the current version of the paper, I point out in footnote 6 that it makes no substantive difference to the conclusions drawn from Equations 1-4 whether we include a term for unobserved social capital (social networks) or not. The reason is that unobserved ability is already sufficient to bias the effects of both "observed human capital" and "observed social capital". My omission of unobserved social capital was not a sleight of hand intended to make human capital measures seem superior to social capital measures.

Indeed, one could rewrite Equations 1-4 changing "unobserved human capital" to "unobserved social capital" and it would not change the mathematical logic in the least. The observed measures of human capital and social capital will be biased if they are correlated with the measure of unobserved social capital and unobserved social capital has an independent effect on outcomes. In other words, the source of the correlated unobservable is, mathematically at least, immaterial. However, Equations 1-4 are just a mathematical exercise unless there are good theoretical reasons why the unobserved variable might be correlated with the observed variables. The reason to keep Equations 1-4 as they are is that is a developed literature on the possible relationship between ability and how much education one obtains (leading to the suspicion that the effect of education is overestimated).   Similarly, the existing sociological literature on homophily suggests that friendship is not random, but may be associated with individual characteristics—some of which may be difficult to measure factors that also affect labor market productivity. So, without including additional terms in Equations 1-4 there is already sufficient theoretical justification for thinking that <u>both</u> observed human capital and observed social capital measures are biased. If the reviewer wants, I can explicitly incorporate unobserved social capital in these equations, but it will not change the interpretation.

As noted on page 12, concern over the bias induced by the relationship between ability and education has resulted in a large literature in economics that attempts to get around this problem—with varied degrees of success. As depicted by Equations 1-4, the economists' concern over biased estimates of human capital essentially mirrors the situation regarding biased estimates of labor market social capital. Furthermore, on pages 13-14 I cite a number of papers in the peer-effects literature demonstrating that concern over the bias of peer-effects due to the nonrandom nature of friendship ties. Because the case of peer-effects on behavior and attitudes parallels the case of social capital effects on labor market outcomes this should be adequate to establish that the concern addressed in Equations 1-4 is a valid one.

B3) The reviewer points out that the paper focuses on income or wages as the dependent variable, and argues that attention should also be paid to job prestige, status, and authority-supervision as the outcomes.

Response: The reviewer argues that job prestige, status, and authority-supervision are important because they "consistently and substantially affect economic returns." If this is the case, then it stands to reason that if contacts are beneficial because they affect job status, then this effect should also show up in higher wages. Hence, using wages as an outcome should be a sufficient test.

However, to give this question the benefit of the doubt, I have rerun Tables 2, 4, and 6 using occupational prestige as the dependent variable. In Table 2P it is clear that there is no evidence that using contacts is associated with an increase in occupational prestige. Indeed, in Model 3,

5

the coefficient is negative (-1.197, p<.01). A similar result is obtained when we replicate the fixed-effects results in Table 4P. Finally, in Table 6P we find that the effects of social capital measures on occupational prestige are essentially the same as the effects on wages. Hence, I do not believe that using wages as opposed to occupational prestige has in any way distorted the results in this paper.

B4) In the reviewer's point #3, he/she argues points out that I dismiss studies of the effect of social resources based on selectivity bias, faulty measurement, etc., but notes that these sorts of problems are endemic in all sorts of sociological studies.

Response: The reviewer is indeed correct to note that these same sorts of problems are evident in all empirical research, and I strongly believe that this should not prevent us from going ahead and conducting empirical research—I am not suggesting that we engage in some sort of epistemological paralysis because of irresolvable methodological difficulties. However, I am suggesting that we be cautious, particularly because there are good reasons to believe that social network data is not random. Moreover, I believe that the benefit of the current version of the paper is it attempts to go beyond pointing out the problem to actually testing whether or not it is an issue empirically. In other words, the proposed test of causality makes use of the assumption that better connected individuals should be more likely to use contacts--everything else equal--to infer whether or not purported social capital variables are actually exerting a causal effect on labor market opportunities. Finally, I acknowledge the possibility that the reviewer may be disinclined to like my test of causality—despite the fact that it is grounded in Lin's notion of the instrumental nature of job contacts—but that is not fatal to the purposes of the paper as test alternative perspectives on the causal role of social capital and contacts (see points B-I and O in Figure 1).

B5) The reviewer argues, again in his/her point #3, that "it is hard to understand the author's equating the three-digit occupations with a lack of influence. Homophily is an important sociological, not economic, principle and it is seen by many researchers in this area as an important factor in social influence and information flow, not explainable by economic processes."

Response: I think there are two questions here. The first question concerns my analysis of the DAS data (Table 5 in the current version) and the second question has to do with the effect of social homophily on labor market outcomes.

With respect to the DAS data, I believe that the reviewer's comment must stem from the fact that I was not clear enough in my discussion of Lin's hypothesis about the effect of contact's status on respondent's status. I have clarified this discussion in the current version of the paper. Lin argues that reaching up the social hierarchy to a contact of high status is beneficial. If this is true, the benefit of high status contacts over low status contacts should hold regardless as to whether we include or exclude contacts who are in the same 3-digit occupation as the respondent. What Table 6 shows, however, is that <u>if we exclude the contacts who are in the exact same occupation as the respondent, there is no evidence that higher status contacts are "better" than low status contacts</u>. In other words, a janitor who uses a Supreme Court judge as a contact doesn't do better than a janitor who uses a barber as a contact.

Now, the interesting finding from the DAS data is that a higher proportion of contacts were in the same 3-digit occupation. So, based on the high propensity alone, same occupation contacts might be beneficial. However, the correlation between contact's status and respondent's status for same occupation contacts cannot tell us whether same-occupation contacts are important, because the

6

correlation is 1 by construction. As a result, I use the UPFLS data, where we have a 20-category variable of friends' and relatives' occupations. Is it true that having more friends and relatives in the same occupation is beneficial? That is what Tables 6 and 7 try to determine. We cannot test that with the DAS data. As a result, with the DAS data, I am not arguing that same-occupation ties are not important, merely that higher status contacts are not. The DAS data shows that higher status contacts are not better than lower status contacts.

With respect to the broader question about homophily I agree that homophily is an important sociological principle and that it structures the way in which job information and influence flow to broader race, class, and gender groups in society. Social class homophily, for example, suggests the rich will have access to higher levels of social capital, which will reproduce inequality above and beyond anything that might be called individual ability or merit. However, the basic point in this paper is that the presence of unobserved individual factors that affect both labor market outcomes and the process of friendship formation will lead to an overestimation of social capital effects. Both of these concepts—homophily indicating group differences in access to social capital and suggesting bias in the estimation of social capital effects—can coexist without contradiction. The presence of social homophily--for example, the tendency of rich people to have rich friends--is prima facie evidence for the existence of social capital effects (i.e. your friends' characteristics affect your outcomes). But if there are unobserved individual characteristics that affect friendship formation and affect income/wealth, then the social capital affects will be biased--regardless as to whether one wishes to depict the process of friendship formation in terms of sociological or "economic" models.

B6) I want to thank the reviewer for pointing out the importance of the article by Lai, Lin, and Leung (1998). This article and the recent book by Nan Lin (Social Capital: A Theory of Social Structure and Action, 2001) provide an extensive discussion the causal mechanisms underlying the role of social capital in the labor market and were very useful in the current version of the paper. On page 16 I note that Lin (2001) and Lai, Lin, and Leung (1998) anticipate my hypothesis that better-connected individuals should be more likely to use contacts. Indeed, Lin (2001:21) argues that "the general expectation is that the better the accessible embedded resources, the more embedded resources can and will be mobilized in purposive actions by an individual." In addition, Lai et.al. (1998) test whether their measures of social capital are associated with higher probabilities of using contacts (which I discuss on page 33), and mention the important point that Boxman does find a relationship between occupational ties among Dutch managers and the use of contacts to find work (also discussed on page 33).

B7) The reviewer suggests that I demand "precise, step-by-step demonstration of the causal processes in empirical studies." Instead, the reviewer argues that "While this may be an ideal goal, epistemological methodology and logic tells us that such demonstrations are almost never done. Can economic models, offered by the author, do that?"

Response: I think the basic problem with the existing literature on labor market social capital is that it goes to quickly from sociological intuition that contacts and social capital matter to empirical models that cannot adjudicate between alternative theories. The intuition may be correct, but the lack of clearly articulated models tends obscures the fact that the empirical models cannot distinguish between a causal effect of social capital or an effect due to the nonrandom way in which friends are acquired.

As mentioned above, the existing empirical literature demonstrates that there is a positive correlation between labor market social capital and job outcomes. This is the first step in the

process of maturation of any empirical literature. The next step is showing that the correlation actually represents a causal effect.

B8) The reviewer argues that the economic model of job search requires extraordinary assumptions (unemployed search, sequential decision making, reservation wages, information about wage distributions) and wonders whether I can demonstrate the validity of these assumptions.

Response: I agree with the reviewer that these are behavioral assumptions. As a result, I have also included a discussion of the extensive search model in the current version of the paper. As far as the test of causality depicted in Proposition 1 goes, the key assumptions are that individuals seek to maximize wages or income and they have multiple methods to search from. I do not need to prove the empirical validity of every feature of the sequential search model because they simply do not affect the validity of my basic proposition.

To take a few of the assumptions that the reviewer mentions in the finally paragraph of page 2:
i) The sequential search model can be easily extended to include employed workers as well as unemployed workers. Indeed, if job offers from contacts come as the result of incidental social interaction (as argued by Granovetter), then well-connected workers will obtain job offers without consciously searching for work. I have worked out an extension to Montgomery's search model that includes job offers arriving from incidental social interaction for both employed and unemployed workers. I will be happy to share it with the reviewer if he/she is interested.
ii) each offer must be accepted or rejected sequentially: This is a basic assumption of the sequential search model. In this revision, I also discuss the extensive search model, which does not make this assumption. As far as Proposition 1 goes, the results are the same: better connected workers should be more likely to use contacts to find work.
iii) reservation wages: I do not believe that workers precisely calculate "reservation wages" based on Equation A4 in appendix A. However, I do believe that workers have some sense of expectations that are based on their skill levels and available opportunities that reflect the basic principle of reservation wages, which is that one doesn't accept offers that are less than what you could reasonably expect to make provided you search a little longer.
iv) the normal distribution of job offers the reviewer refers to in this paragraph is arbitrary. Any distribution would do.

The reviewer misses the point that Montgomery's search model supports the use of social capital models. In general, the reviewer seems to be operating under the assumption that the (economic) sequential search model and sociological theory are somehow antithetical or incompatible. On the contrary, I think Montgomery's (1992) use of search theory actually supports sociological theory by arguing that it is premature to conclude that contacts do not affect labor market outcomes based upon the lack of a relationship between using contacts and wages or occupational prestige. Indeed, one of Montgomery's conclusions was that the sequential search model suggests that we need to turn to models of network composition and structure—i.e. social capital models (see the discussion on pates 7-8).

B9) In the same paragraph as the previous point, the reviewer argues "even in this model the reservation wage is assumed to be related to connections to others".

Response: Yes—the reservation is affected by connections to others. That is precisely the point. Compare two workers, one who is well connected and one who is not, but who are otherwise

identical. The well-connected workers will have a higher reservation wages because he/she expects to get good jobs via connects. The result of raising his/her expectations is that both formal and informal job offers that are accepted will be better than the reservation wage.

B10) In the his/her final paragraph, the reviewer says that I dismiss the demand side evidence (i.e., Fernandez and Weinberg) to quickly, and that there is no need for the demand side effects to show up on the supply side.

Response: I believe that the results of Fernandez and Weinberg (1997), Petersen (2000), and Fernandez and Camilla (2000) demonstrate that, in the firms they studied, contacts inside the firm led to higher rates of job offers. I also agree with the reviewer that this may not show up in supply side studies of workers. There at least four reasons that come to mind:
1) The effect is only apparent in a few firms—such as firms that actually keep a record of employee referrals—and is washed out overall.
2) Workers tend to be equally likely to have an inside contact (see page 7), so that over several jobs the effects of "getting lucky" by an inside contact tend to balance out. This would increase the unexplained component of the cross-sectional variation in income and wages, but it would not increase permanent income inequality. I.e., it would not be associated with long-term inequality.
3) The single-firm studies investigate the effect of employee referrals, which are a subset of all job contacts. It is possible that the there is a benefit to employee referrals but not job contacts in general. However, footnote 3 on page 6 indicates that around ½ of all job contacts are actually referrals. Therefore, if the beneficial effects of referrals are as large as suggested by the single firm studies, the benefit should still show up in demand side studies.
4) Well-connected workers simply raise their reservation wage, so that difference in wages or duration of unemployment among accepted jobs found via informal or formal methods are small, despite differences in the average wage or probability of receiving a job offer between each method. (See point 6 from Reviewer A).

In addition to these three possibilities, one can probably think of other reasons why we might not expect symmetry between the demand side and the supply side results. However, it is important which one of these reasons we select. For example, #2 and #4 suggest very different conclusions about whether social capital results in differences in income inequality. If we think that the results from the single-firm studies generalize to the labor market as a whole, then I think it is important to explain why these results don't show up in studies of workers.

Reviewer C

Main Points

C1) The reviewer points out that the paper meanders too much and that too much space is devoted to discussing the null results, and suggests that the focus of the paper should be on the empirical analysis of the MCSUI data.

Response: In the current version of the paper, I have compressed the discussion of null results from the direct-effect models in Table 3 to a single page, and I have reduced my literature review of these studies to a bare minimum, relying on the excellent reviews that have recently been published. In addition, Figure 1 attempts to organize the results section as logically and concisely as possible.

As noted above in response to reviewer A, the focus of the current paper is on the social capital results. In addition to the social capital models estimated with the MCSUI data in the previous version, in this version I also include the UPFLS data, which has information on same-occupation social ties.

C2) The reviewer points out that there should be more congruence between Montgomery's search theory model and the empirical analysis on the MCSUI data.

Response: This point is well taken. Part of the problem in the previous version of the paper was that I discussed differences in the mean of the wage distribution (μ_c and μ_f) but not differences in the offer arrival rate (P_C and P_F). However, as the reviewer points out, they are both important. In the new version of the paper I show that, under the framework of either the sequential or extensive search models, increasing either μ_c or P_C results in an increase in expected wages and an increase in the probability of using contacts to find work. Therefore, both μ_c and P_C represent the contributions of labor market social capital.

More precisely, in Equations 6 and 8 I define two social capital variables, P_{SC} and μ_{SC}. P_{SC} increases the arrival rate of job offers from informal methods but not from formal methods, and μ_{SC} increases the average wage of job offers from informal methods but not formal methods. Proposition 1 shows that both of these variables increase wages and increase the probability of using contacts.

I prefer to think of the effect of social capital as the combination of P_{SC} and μ_{SC} rather than attempt to estimate models that separate the effects. There are two reasons for this. The first is for the sake of simplicity. Because Proposition 1 predicts positive effects on expected wages and the probability of using contacts (Pr(c)) for both P_{SC} and μ_{SC}, we can estimate reduced form models that estimate the overall effect of social capital variables on wages and Pr(c) without decomposing any observed effect into components due to P_{SC} and μ_{SC}. Secondly, I view both of the job search models used in this paper as heuristic models to guide our intuition rather than exact depictions of the process of job search. Without more complete data on the process of job search itself (i.e., wages of job offers and information on available opportunities and the wage structure) any attempt to decompose the effect of P_{SC} and μ_{SC} will depend heavily on which model of job search you use. For example, a sequential search model where individuals search high-wage jobs first would be consistent with Proposition 1 but might give us different estimates of the role of P_{SC} and μ_{SC} in social capital effects even with the same data. For the moment, anyway, I prefer to think of P_{SC} and μ_{SC} as representing the same concept.

On a related note, the log-likelihood equation that the reviewer sets up in the second paragraph is indeed correct. However, to truly estimate the sequential search model, the likelihood equation would be even more complicated, as accepted offers are random draws from the formal and informal offer distributions truncated from below at the reservation wage. Thus, the reservation wage, which is itself a function of all of the parameters in the model, would appear in both of the

bracketed terms in this equation. To get an idea of the relationship between P_{SC} and μ_{SC} and the reservation wage, inspect $\dfrac{dw^R}{d\mu_{SC}}$ in Equation A9 and $\dfrac{dw^R}{dP_{SC}}$ in Equation A10.

The focus of the current paper is on making the case for the test of causality derived from the simple search theories in Proposition 1. For this purpose, reduced form empirical models are sufficient. If this approach is found acceptable, I think there are reasons to be interested in estimating structural models that would attempt to estimate a form of the log-likelihood model the reviewer proposes. For instance, in the conclusion I argue that even if a social capital variable had an effect on both wages and the probability of using contacts, it would not follow that all of the effect on wages was causal. A structural model that estimated the parameters in Equations 5-8 could help us make inferences about the proportion of the effect on wages that was "causal" based upon the magnitude of the effect on the probability of using contacts. This is beyond the scope of the current paper and is the subject of future research. The benefit of the current approach is that it makes relatively few assumptions in the statistical analysis and in the underlying behavioral model.

C3) The reviewer notes that when considering the factors that might influence the probability of receiving offers through informal and formal means, it is difficult not to consider the job or occupation that one is seeking employment.

Response: This is a good point. I think that the proportion of jobs that are found via contacts is likely to vary across occupations. As mentioned in the paper, there is empirical evidence that blue collar occupations and lower-wage jobs are more likely to be filled by contacts or employee referrals. As a result, in this version of the paper, I include a variable measuring the proportion of 3-digit census occupations that were found through contacts. As mentioned in Appendix C, this information is from the 1994-1998 waves of the NLSY, which asked respondents whether they used contacts for each job they held over this period. As a result, I am estimating models of the probability of using contacts controlling for the average propensity of using contacts in each 3-digit occupation.

The reviewer is correct to point out that other factors affect the probability of receiving job offers, such as skill levels, gender, race etc. The reduced form models in Tables 6-8 also include observed individual characteristics. In other words, the effect of observed human capital and gender on wages and the probability of using a contact are controlled for.

This paper specifically addresses the role of contacts and social capital in the job matching process. Due to space constraints, this paper has not gone into an extended discussion on other facets of the job matching literature. To the degree that difference in the use of contacts due to factors such as labor market segmentation are expressed at the occupation level, the NLSY variable measuring the occupation-specific use of contacts should control for them.

Reviewer D

Main Points

D1) The reviewer says that the most important point that the paper raises is the issue of the non-random acquisition of friendship ties. The reviewer argues that while he/she is in favor of

"maintaining high bars to clear with respect do evidence, does it make sense to make the bar so much higher for structural interpretations than individualist interpretations?"

Response: This point is well taken, and I took it very seriously. The most significant revision to the paper is the attempt to test whether the social capital effects are causal or spurious. (See similar remarks in point #2 above and in response to Reviewer B's comments in Point B4.) What differentiates studies of labor market social capital from more general social capital models is that information on whether or not contacts were actually used to find work gives us an additional piece of information.

D2) The reviewer sees that main contribution of the paper in the fixed effects models with the NLSY data.

Response: I think the fixed-effects results in Table 4 are important. If you believe that the use of contacts are exogenous to social capital, then the fixed effects models tell you whether workers who used contacts did better than when they did not use contacts. However, I believe the results are qualified by the logic of the sequential and extensive search models. The sequential search model predicts that all accepted offers are at least as good as the reservation wage, whether or not they are from formal or informal methods. Similarly, the extensive model assumes that the accepted offer is the best offer received, after waiting for all the offers to come in. In this case the difference in wages between formal and informal methods in accepted jobs will not accurately reflect the wage difference in job offers. Hence, on page 36 I note that "According to the job search model, the results in Table 4 are what we would expect: one of the things we are "differencing out" in Equation 2 as a fixed effect is heterogeneity in the reservation wage across workers."

D3) The author argues that "the fact that job contacts may come from others like oneself (homophily) does not mean that job contacts do not matter. It just means that…homophily might be important as a basis of job contacts."

Response: I agree with this statement. Homophily does not mean that job contacts do not matter. In and of itself, social homophily just indicates that friends tend to resemble each other on observed characteristics. By itself, this does not bias social capital coefficients. As depicted in the "Social Homophily and Causality" section on pages 10-15, the problem comes if social homophily also operates on characteristics that are unobserved to the researcher and also affect wages. In other words, if "motivation" or "entrepreneurial ability" are characteristics that friends sort on but are not observed on surveys, and these characteristics also affect wages, then social capital effects will be upwardly biased.

See similar comments in point B5 above. Social homophily as a basis of job contacts and bias due to homophily on unobserved individual human capital characteristics can both exist without contradiction. An observed correlation in friends' income after controlling for observed individual characteristics may reflect both a causal effect of social capital and a spurious effect due to homophily on unobserved individual characteristics. I hope my revised manuscript and this memo makes this point clearly.

D4) A similar point to D3 is brought up with respect to the papers by Liu and Duff and Fernandez et. al.. The reviewer suggests that I argue that homophily is an artifact. By "social homophily", I mean the empirical fact that friends tend to resemble each other on observed characteristics. I take this as fact, not artifact. In Table 2 of Fernandez, Castilla, and Moore ("Social Capital at Work: Networks and Employment at a Phone Center" AJS 2000) the authors find that a positive

referrer-referral correlation on several characteristics. I don't find this surprising at all. However, as pointed out in D3, social homophily and a problem of bias due to correlated unobservables can exist at the same time. Indeed, it is the large amount of empirical evidence such as Fernandez et. al. pointing to the tendency towards homophily on observed characteristics (reviewed in McPherson et. al. 2001) that makes one suspect that homophily is likely to occur on unobserved productivity-related characteristics also thereby biasing social capital coefficients.

D5) The author argues that the results from the DAS data (Table 5 in the current version) do not mean that contact effects are just an artifact.

Response: Again, I agree with what the reviewer says. The DAS results show that having higher status contacts are not more beneficial than lower status contacts. Once we remove the substantial proportion of same-occupation ties, the friends occupational status has no effect on respondent's status. However, as the reviewer points out, this does not mean that contacts do not matter, merely that the status of the contact person does not matter. I hope of have done a better job of making this clear in the revised version. Indeed, as I point out on page 30, the prevalence of same occupation contacts suggests same occupation contacts may be an important source of social capital. I think that it is important to point out that the large positive correlation between contact's status and respondent's status (as depicted by the coefficient of .254 in Model 2 of Table 4 which includes all cases) is probably due to the large proportion of same-occupation ties (the coefficient on contact's status is .035 when the 65 same occupation cases are excluded). But I totally agree with the reviewer that by itself this does not indicate that contacts do not matter.

Other points

D6) I have deleted the original appendix on reservation wages and the indeterminacy of social contact effects. The new appendices derive Proposition 1 for the sequential and extensive search models.

D7) I have completely rewritten the literature review in light of the several excellent review pieces that have recently been published. I have tried to make the literature review as concise as possible.

I now review the other single-firm employer-side studies that the reviewer mentioned. The reviewer notes that some employers are willing to pay current employees for contacts. This is true. A more significant question would be whether they are willing to pay individuals who got their job through contacts higher wages. The per hire dollar savings associated with referrals reported Fernandez, Castillo, and Moore (2000) are not large enough to suggest that employers would be willing to pay higher wages to referrals.

Finally, I thank the reviewer for pointing out the interaction between the use of contacts and tenure in Simon and Warner (1992). I have tried to replicate the interaction of use of contacts with tenure in the NLSY, MCSUI, and UPFLS data used in this paper, and do not find any evidence of a significant interaction effect.