



Social capital and homophily both matter for labor market outcomes – evidence from replication and extension



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ABSTRACT

Social resource theory has been challenged, as the effects of contact resources on job outcomes may be spurious given the presence of homophily. We review the Mouw–Lin debate and propose that occupational homophily moderates the role of contact resources in the labor market and that effects of resources depend on labor market institutions. We analyze data from the US, East Germany before 1989, and post-reform China, combining the first-difference method and Heckman selection procedure to deal with endogeneity. Empirical findings from different labor market contexts demonstrate that: (1) contact resources have a causal and positive role in job outcomes; (2) in a market economy, the role of resources is more salient if they provide within-occupation job-leads; (3) under state job-assignment systems occupational homophily does not pay off; and (4) job-search ties can take the form of heterophilous selection.

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1. Introduction

A large body of sociological literature has been accumulated regarding how social networks or social capital affect individual job outcomes in the labor market. Lin (2001) has classified relevant studies into three strands. The first strand, labeled “using contacts”, focuses on the effect of using social ties as compared to not using them. The second strand, labeled “accessible contacts”, addresses the role of the overall quantity or quality of resources embedded in one’s ego-centric networks. The third strand deals with the effects of “mobilized social capital” often measured by contact resources (i.e., resources of a specific contact who has offered substantial help in the job search). Central to the third strand of research is Lin’s (1999) social resource theory, which argues that the better, or the higher quantity of social capital that is used, the better job outcomes will be.

However, replicating and extending the 1970 Detroit Area Study (DAS) by Marsden and Hurlbert (1988), Mouw (2003) prominently challenged the social resource theory by showing that once homophily (e.g., the similarity between the occupations of the job seeker and the contact) is considered, the role of contact prestige in

status attainment becomes insignificant.¹ Since previous empirical studies supporting Lin’s theory had not considered the presence of homophily, the estimated role of contact resources is very likely to be spurious and due to selection effects. To defend the social resource theory, Lin and Ao (2008) aptly pointed out that Mouw (2003) had erroneously coded the key variable of occupational homophily, or “same-occupation”. In particular, the comparison between the contact’s job and the respondent’s current job “cannot reflect social influence (e.g., a respondent approaching the contact for help in the labor market) that must precede the current position of the respondent” (Lin et al., 2013:26; emphasize by authors). Taking into account the “correctly” specified occupational homophily – i.e., to compare the respondent’s previous job and the contact’s job – recent studies have lent support to social resource theory (Lin and Ao, 2008; Son and Lin, 2012; Son, 2013; Lin et al., 2013; Bian et al., 2015).

Although the debate has lasted for more than a decade, two issues raised by Mouw (2003) still deserve further investigation. First, the similarity of one’s “current job” and the contact’s job may affect the role of social capital, since “inside” job-leads can be more productive than “outside” job-leads, especially in a modern labor market like the US, where job information matters for job searches. In other words, the role of social capital may be

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¹ Mouw (2003) not only examined the role of used social capital, but also addressed the effects of mobilized contacts.

moderated by occupational homophily and depend on particular labor market contexts. Second, the endogeneity problem in estimating the role of social capital should be addressed in a systematic way. Under the counterfactual framework, to defend Lin's (1999) social resource theory, one needs to use appropriate identification strategies (e.g., a fixed effects model, an instrumental variable, or propensity score matching, etc.), which have been advocated by Mouw (2006) in relation to carrying out causal analysis.

We first provide a critical review of both sides of the "Mouw–Lin debate". We replicate the 1970 DAS and reveal the potential significance of occupational homophily (i.e., Mouw's version of "same-occupation") by demonstrating how it influences the productivity of contacts prestige in urban USA. Next, we develop a theoretical model of the network effect and how it hinges on occupational homophily under different labor market institutions. To examine the empirical implications of this model, we draw on causal evidence from the former German Democratic Republic (GDR) and post-reform China by combining the first-difference method and the Heckman selection model. We aim to add to this line of research by: (1) testing Lin's (1999) social resource theory using the advanced models that Mouw (2006) has called for; and (2) providing insights into how institutional contexts shape the role of social capital by exploring whether and how the role of contact resources interacts with occupational homophily in different societies.

2. The Mouw–Lin debate: a critical review

It is theoretically plausible to expect that contacts matter in the job seeking process, because social resources are unevenly distributed in society. As a result, all other things equal, mobilized social resources pay off. Empirical evidence supporting the role of contact resources can be found in a host of studies (e.g., Lin et al., 1981a,b; Marsden and Hurlbert, 1988; De Graaf and Flap, 1988; Requena, 1991; Wegener, 1991; Bian and Ang, 1997; Bian, 1997; Volker and Flap, 1999, 2001; Smith, 2000; Lin, 2003; to name a few). Lin (2001) presents a cogent review of these studies, and argues that a consensus has been reached that high status contacts are in general beneficial to job outcomes.

2.1. Homophily as confounder: Mouw's challenge

Mouw (2003) was among the first to notice that social homophily (for an overall review of homophily see McPherson et al., 2001) might be a competing theory to the social resource perspective. Social capital may be wrongly interpreted while effects actually only mirror the tendency of similar people to become friends. Hence, the major concern is: the estimated effects of contact resources might be misleading as the association between social capital and the job outcome may not be causal but due to homophily; which is not accounted for in the regression model. In the section "Exogenous social capital model" Mouw (2003:882) tested this by replicating and extending the 1970 DAS by Marsden and Hurlbert (1988). Mouw (2003) suspected that the estimated role of contact prestige in previous analysis might be accounted for by selective social networks or friendship derived from homophily (p. 869). Therefore, Mouw constructed a variable that represented whether or not respondents had the same occupation as their contacts. Taking into account this occupational homophily, the replication reveals that the significant effect of contact prestige disappeared (Mouw, 2003:883, Models 2 and 3 in Table 5).

According to Mouw (2003), the significant role of contact prestige merely captures the fact that there are some job-seekers, who have the same occupations as their contacts. Consequently, the results from previous studies that had not considered the presence of occupational homophily must have overestimated the role of

used social capital. Mouw (2003) therefore stated that: "the evidence in favor of the social resources perspective is largely an artefact of the incidence of same-occupation information flows between contacts and job-seekers" (Mouw, 2003:883). This finding strongly challenged Lin's (1999) social resource theory, since the reported social capital effects only mirror the advantages brought about by occupational similarity, which is obviously one of the various dimensions of social homophily.

2.2. Questionable operationalization of homophily: Lin's refutation

The original data and Stata codes used in Mouw (2003) were published on his personal website, making it possible for other scholars to replicate the study. Lin and his colleagues put forward a strong refutation after they inspected Mouw's (2003) Stata codes (Lin and Ao, 2008; Lin et al., 2013). According to the syntax for coding key variables, the occupational homophily in Mouw (2003) was specified by comparing the contact's position with the respondent's "current" position. However, as Lin et al. (2013:25) argue, "the social capital theory clearly proposes that the 'reaching up' phenomenon refers to the process from ego's initial position to contact's" – implying that the comparison should be made with respondents previous position instead of the current one. The comparison between the contact's job and the respondent's current job "cannot reflect social influence (e.g., a respondent approaching the contact for help in the labor market) that must precede the current position of the respondent" (Lin et al., 2013:26).

Lin and his colleagues further pointed out that the correct method by which to construct the dummy variable of occupational homophily is to compare between the respondent's *previous* job and the contact's job. Using the "correctly" specified occupational homophily, Lin et al. (2013) replicated the 1970 DAS and found that the effects of contact prestige are still statistically significant, no matter whether similar occupations are excluded or not (see also Lin and Ao, 2008; Son, 2013; Son and Lin, 2012). Including three measurements for occupational similarity in the regression, Bian et al. (2015) reported that controlling for homophily does not mitigate the role of network resources. In general, this line of research stresses that the mistakenly specified social homophily variable led to misleading findings in Mouw (2003).

2.3. Revisiting the debate: occupational homophily as a moderator

Is this the end of the debate? On the one hand, we believe that – although the refutation by Lin and his colleagues is based on cross-sectional analyses – Mouw's (2003) challenges to the genuine role of contact resources have not seriously undermined social resource theory. On the other hand, we argue that it is premature to claim that Mouw's (2003:883) concern with the similarity between the contact's job and the respondent's current job is unsubstantiated. We argue that, although occupational homophily seems not to be a potential confounder, it may still have important implications. More in particular, we argue that having the same or a very similar occupation can determine the returns of social resources. In addition, we believe that the broader institutional context of a labor market influences the value of contacts since different institutional settings enforce different mechanisms of social capital. If this holds, the results from the models by Mouw (2003) require a different interpretation.

Occupational homophily – the similarity between the current job of a respondent and the job of a contact – can actually be regarded as a measure of the efficiency of social capital: job-leads, especially job-related information from "inside" the contact's own occupation is more accurate, efficient, and hence more useful

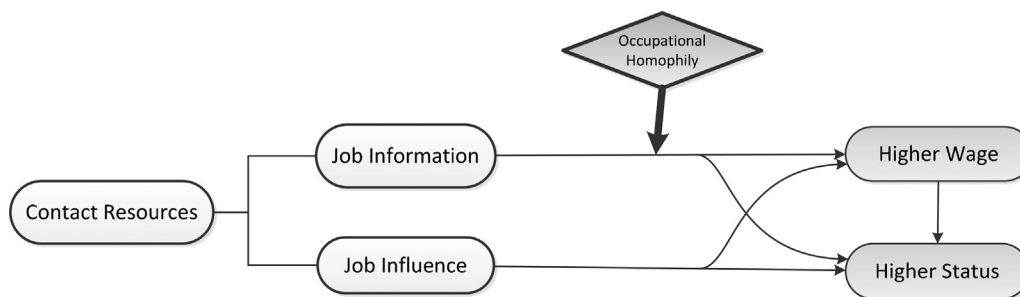


Fig. 1. A theoretical model of the labor market implications of contact resources and occupational homophily.

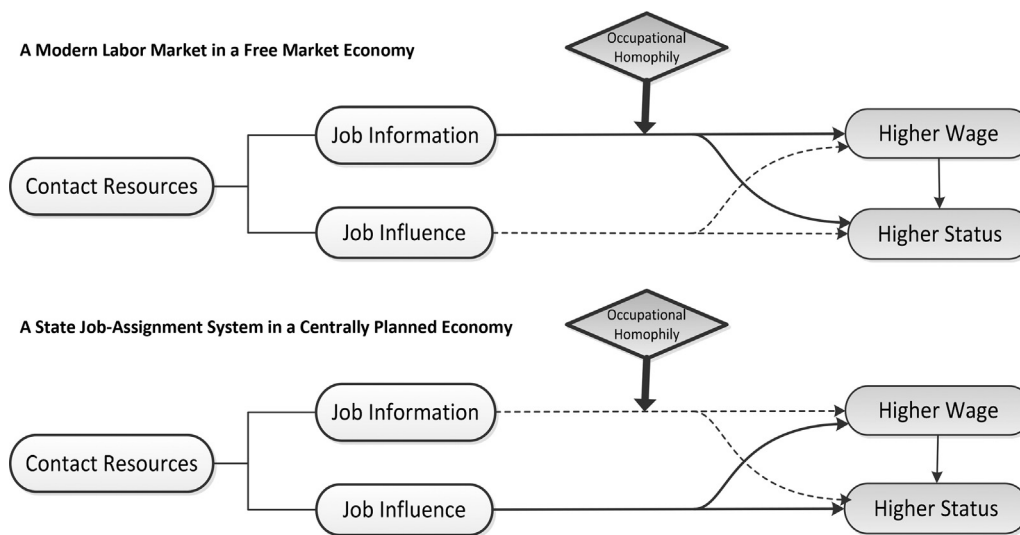


Fig. 2. Theoretical models in two different institutional contexts.

than those from “outside”. This is particularly the case when job information matters greatly, such as in a highly segmented labor market in a free market economy like the US (see the literature that distinguishes between information and influence as two types of network resources that facilitate job matching: Granovetter, 1973, 1974, 1995; Bian, 1997; Bian et al., 2015; for the labor market segmentation literature see Piore, 1970, 1975; Freedman, 1976; Thurow, 1975; Thurow and Lucas, 1972). For example, for a plumber who is seeking a new position, vacancy information from a lawyer with a much higher status is very likely to be less useful than that from a gardener with a similar status or from another plumber with the same occupation. However, in a centrally planned labor market with a state-driven assignment system of people to jobs not information but influence is the relevant mechanism of social capital. This idea is illustrated by Bian (1997). Using the state job-assignment system in pre-reform China as an example, Bian (1997:367) stated that “within this institution, personal networks are used to gain influence from job-assigning authorities rather than to gather employment information, because even when they have information, job-seekers cannot apply for jobs; jobs are secretly assigned by officials as favors to those who are directly or indirectly connected to them”. Hence, our argument is that contact resources actually differ according to the institutional context where social capital is created.

Our argument is summarized in Fig. 1, which depicts a model that extends the social resource theory by including the potential moderating role of occupational homophily. The left-hand side of the model indicates two kinds of contact resources, information and influences, whilst the right-hand side shows two major job outcomes, improved income and higher status. In our model,

homogeneity (same occupation) affects the productivity of information by offering in depth information, which brings about an improved negotiation ability, a shorter learning process, and a much better job-worker matching, etc. (see Bian et al., 2015 for a review).² That is, occupational homophily positively moderates the role of information. We do not expect that homophily also positively interacts with influence, because influence resources in same occupations are likely to be not far-reaching, not powerful, and therefore less productive for job seeking. In general, for job seeking, heterogeneous contacts embedded in larger social networks are more beneficial as regards mitigating socioeconomic inequality and making more diverse resources available in the focal network (Lin, 2008; Bian and Logan, 1996; Briggs, 1998, 2002; Wellman and Gulia, 1999; Domínguez and Watkins, 2003).

The model in Fig. 1 varies with institutional conditions. As mentioned above, we expect that in a labor market under a free market economy, information as a resource provided through social ties matters most to job outcomes. By contrast, in a centrally planned economy with a state job-assignment system, it is the job-related influence rather than information that pays off. We therefore depict two different models in Fig. 2, one for a modern labor market in

² Although not presented in the figure, previous studies have identified various mechanisms through which social capital exerts its role, including more job openings, higher negotiation ability for better salary, quicker adaption to the new environment, reduced search costs for the employers, direct assigning to better positions, and better job-worker matching (Granovetter, 1974, 1981; Seidel et al., 2000; Rosenbaum et al., 1999; Coverdill, 1998; Fernandez and Weinberg, 1997; Fernandez and Castilla, 2001; Bian and Logan, 1996; Borocz and Southworth, 1998; Yakubovich and Kozina, 2000; see Bian et al., 2015, for a cogent review).

Table 1
Replication of 1970 DAS data with different model specifications. Dependent variable: occupational prestige.

Independent variables	Mouw (2003:883) Table 5 Model 2	Mouw (2003:883) Table 5 Model 3	Model 1	Lin et al. (2013) Table 1	Model 2
Father's job prestige	.060(.061)	.024(.074)	.027(.056)	.059(.070)	.064(.060)
Education	1.136***(.278)	1.364 (.346)	1.004 (.258)	1.320 (.317)	1.113***(.276)
Prior job prestige	.266 [*] (.060)	.300 [*] (.074)	.233 [*] (.056)	.197 ^{**} (.067)	.216 ^{**} (.064)
Tie strength (1 = strong, 0 = weak)	3.135(2.714)	3.405(3.589)	2.370(2.513)	3.479(3.133)	3.017(2.710)
Contact connected to firm	-.217(1.504)	-.797(1.849)	-.781(1.395)	-.501(1.688)	-.394(1.495)
Contacts' job prestige (CP)	.254***(.063)	.035(.077)	.085(.066)	.244***(.069)	.251***(.066)
Same-occupation 1 (SO1)	-	Sample Excluded	3.650***(.1348)	-	-
SO1 # CP	-	-	.606***(.105)	-	-
Same-occupation 2 (SO2)	-	-	-	Sample Excluded	3.782***(.1727)
SO2# CP	-	-	-	-	.122(.127)
Constant	1.304(4.350)	7.712(5.934)	16.813 ^{**} (4.242)	1.322(5.151)	13.959 ^{**} (4.533)
Observations	219	154	219	176	219
R ²	.41	.29	.53	.34	.43

Notes: Standard errors are in parentheses; SO1 is constructed by comparing current job with contact's job, while SO2 is constructed by comparing previous job with contact's job.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

a free market economy and the other for a state job-assignments system in a planned economy. Below, we test the empirical implications of the theoretical models in Fig. 2 by analysing data sets from different societies.

3. Replication and extension

Mouw (2003) did not investigate whether and how occupational homophily influences the role of social capital but excluded respondents with the same-occupation as their contacts from the model; which can be regarded as a questionable specification. In fact, Mouw's (2003) method of excluding respondents amounted to fitting models onto persons who used low-efficiency resources to find jobs (from different-occupation contacts in Detroit). A more appropriate model specification is to include a dummy variable indicating whether occupations are the same or not and its interaction with the contact's prestige, which we present in Table 1.

We first replicated two models estimated by Mouw (2003:883) and obtained exactly the same coefficients as Mouw, see the second and third column of Table 1. Model 1 in the fourth column, nevertheless, is fitted on the full sample and further includes the same-occupation dummy constructed by Mouw (2003) and the interaction term between same-occupation and contact's job prestige. As Model 1 shows, whilst same-occupation (SO1) does pay off, contacts' prestige mainly matters among those who used a same-occupation contact, as indicated by the interaction between same-occupation and prestige of contact. Specifically, other things being equal, if contacts were both "insiders", a 10-point higher contact prestige will bring about around a 6.06 point higher job status ($10 \times .606$). Moreover, the role of contact resources is in fact considerable, which contradicts Mouw's (2003) findings based on excluded samples. Note that the effect of the contact's prestige when in the same occupation is in fact more than two times greater than the role of a respondent's prior job prestige (.233). If Mouw (2003) had used the correct model specification, he would not have arrived at the conclusion that contact does not matter.

Interestingly, Lin et al. (2013) also excluded respondents when replicating the 1970 DAS, using their version of the same-occupation variable (SO2) to determine which respondents to be kept. We therefore present the model including the same-occupation dummy constructed by Lin et al. (2013) in the two last columns of Table 1. As Model 2 shows, controlling for SO2 and the interaction term in fact magnifies rather than mitigates the role of contacts' prestige i.e., the coefficient of SO2 is slightly larger in model 2 than in the previous model. More importantly, it

should be noted however, that the R^2 of Model 2 (.43) is smaller than that of Model 1 (.53). This finding points to the significance of including Mouw's version of the occupational homophily dummy (SO1) in models, and calls for further investigation, in future studies, regarding whether and how the role of social capital hinges on occupational homophily.

3.1. Beyond the "same-occupation" tangle: model identification issues

Lin and colleagues' more recent studies are still cross-sectional and therefore do not provide a conclusive basis for causal inferences regarding the role of contact resources. Major sources of the endogeneity in this line of research are twofold. One is the non-random use of contacts to find a job, leading to studying non-representative samples. Sample selection arising from observed or unobserved attributes has been extensively reported in previous studies. For example, while individuals of a higher social status are found less likely to use contacts (Marsden, 2001), previous research has also shown that richer human capital predicts a higher probability of using contacts to find work (Lin and Ao, 2008). More importantly, selection from unobserved ability has also been confirmed in social capital studies. For example, Chen (2011) found that the positive selection process of rural-to-urban migration (rural residents with a higher earning ability are more likely to migrate to cities) leads to an underestimated role of social networks in income among migrants. Since contact-users always constitute a "truncated sample" (because there are always non-users), we must take into account the sample selection problem. As Marsden and Hurlbert (1988) stress, there are substantive reasons for thinking that unmeasured factors predisposing people to use contacts may be correlated with error terms in the job outcome equations.

The other problem is the one of omitted variables, which cannot be easily resolved. Although Lin and colleagues took into account the "correctly" coded same-occupation as a potential confounder (SO2), this dummy variable merely represents one of many possible types of homophily. What is more, social homophily by and in itself is merely one of various sources of an endogeneity bias when estimating a social capital model (see a review of endogeneity sources in social capital research in Mouw, 2006; Chen, 2012). For example, Bayer and Ross (2009) found that it is social heterophily rather than homophily that leads to a biased estimation of the role of neighborhood social capital in job outcomes. Furthermore, individual ability is always a confounder in this line of research because ability is hard to measure and most likely

to determine both the volume of contact resources and the job outcomes.

It should be noted that early studies in this line of research were not totally unaware of the potential endogeneity problem. A common method to deal with sample selection is fitting the Heckman selection model (see e.g., Marsden and Hurlbert, 1988; Wegener, 1991; Lin, 2003). However, as Wooldridge (2006:620) mentions, to practically distinguish the sample selection from a misspecified model, one should have at least one explanatory variable in the selection model that has no direct effect on job outcomes.³ As for the strategy used to deal with the potential omitted variable, an often-used method is to include the time-lagged dependent variable in the model. This method is reasonable, given the data limitations, but far from perfect, since the inclusion of job outcomes from a previous period introduces a number of additional unobserved factors into the model. In general, although studies in the parallel fields of social capital research (e.g., the treatment effects of using contacts or tie strength) have exploited advanced identification strategies to deal with the endogeneity problem (see, e.g., Mouw, 2002; Yakubovich, 2005; Gerber and Mayorova, 2010; Obukhova, 2012; Chen, 2012; McDonald, 2015), academic efforts to simultaneously address the selection and omitted variable problems in order to estimate the role of contact resources are rare.

Since the data from 1970 DAS are not sufficient for performing counterfactual analysis, we will exploit more rich data sets to examine the empirical implications of our theoretical model. The rest of the paper is structured as follows: after addressing our identification strategies, we first replicate and extend the study by Volker and Flap (1999) to look at the former GDR case, where the labor market is obviously quite different from that in the US. We then extend this strategy to analyze Chinese data, where the labor market can probably be seen as a hybrid one in comparison to the GDR and the US. Finally we interpret the findings and provide our conclusions.

4. Models and strategies

To estimate the effect of used social capital on job outcomes, we start with a standard ordinary least squares (OLS) model, which can be expressed by:

$$Y_i = \beta_0 + \beta_1 S_i + \beta_2 X_i + \varepsilon_i \quad (1)$$

where Y_i is the job outcome (e.g., job status or wage) of the individual i . S_i denotes the level of the used social capital, X_i denotes a vector of individual factors, ε_i is the random error. Estimating Eq. (1), however, is confronted by a major difficulty. Notice that the most important premises for obtaining an unbiased OLS estimate of β_i is that there is no sample selection and S_i should be exogenous. However, the twofold assumption is very unlikely to hold in the context of a social capital study.

First we allow for sample selection because we know people who use contacts to find jobs may share some unobserved attributes. Thus, we write the Heckit model given by Eq. (2), or, the selection model, and Eq. (3), or, the substantial model, as follows:

$$\lambda_i = \gamma_0 + \gamma_1 Z_i + \gamma_2 X_i + \mu_i \quad (2)$$

where $\lambda_i = 1$ denotes using contacts while $\lambda_i = 0$ is not using contacts. Importantly, Z_i is a vector of the factor that explains the choice of job-seeking method but should not be included in Eq. (3). In other words, Z_i is the exclusion restriction needed to practically distinguish the sample selection from a mis-specified model (Wooldridge, 2006).

$$Y_i = \beta_0 + \beta_1 S_i + \beta_2 X_i + \beta_3 \hat{\lambda}_i + \varepsilon_i \quad (3)$$

where $\hat{\lambda}_i$ is the inverse Mills ratio, a transformation of the predicted individual probability of using contacts to find a job from the following selection equation. We simultaneously estimate Eqs. (2) and (3) to obtain maximum likelihood estimates and calculate heteroscedasticity-robust standard errors. Note that including the inverse Mills ratio in Eq. (3) amounts to control for selection derived from unobserved variables.

However, there may be other sources of endogeneity, such as social homophily or ability, that cannot be corrected for using the Heckit model. To systematically overcome the endogeneity problem, we fit the first-difference model in which the Heckit method is also incorporated. In the Heckit-FD model, the constant effects of time-fixed unobserved factors are ruled out, whilst sample selection is allowed. That is, we compare the outcome that an individual obtains with a certain level of social capital versus the outcome that the same individual receives given another level of social capital, allowing for unrepresentative samples. Therefore, we write Heckit models for two separate cross-sectional equations of two different years, namely year t and year $(t-1)$, as:

$$Y_{it} = \beta_0 t + \beta_1 S_{it} + \beta_2 X_{it} + \beta_3 \hat{\lambda}_{it} + \alpha_{it} + \varepsilon_{it} \quad (4)$$

$$\lambda_{it} = \gamma_0 t + \gamma_1 Z_{it} + \gamma_2 X_{it} + \mu_{it} \quad (5)$$

$$Y_{i(t-1)} = \beta_0(t-1) + \beta_1 S_{i(t-1)} + \beta_2 X_{i(t-1)} + \beta_3 \hat{\lambda}_{i(t-1)} + \alpha_{i(t-1)} + \varepsilon_{i(t-1)} \quad (6)$$

$$\lambda_{i(t-1)} = \gamma_0(t-1) + \gamma_1 Z_{i(t-1)} + \gamma_2 X_{i(t-1)} + \mu_{i(t-1)} \quad (7)$$

Then we subtract Eq. (6) from Eq. (4) and obtain:

$$Y_{it} - Y_{i(t-1)} = (\beta_0 t - \beta_0(t-1)) + \beta_1 (S_{it} - S_{i(t-1)}) + \beta_2 (X_{it} - X_{i(t-1)}) + \beta_3 (\hat{\lambda}_{it} - \hat{\lambda}_{i(t-1)}) + (\alpha_{it} - \alpha_{i(t-1)}) + (\varepsilon_{it} - \varepsilon_{i(t-1)}) \quad (8)$$

Since we assume that individual unobserved variables are time-invariant, that is $\alpha_{it} - \alpha_{i(t-1)} = 0$, Eq. (8) ends up as:

$$\Delta Y_i = \beta_0 + \beta_1 \Delta S_i + \beta_2 \Delta X_i + \beta_3 \Delta \hat{\lambda}_i + \Delta \varepsilon_i \quad (9)$$

where “ Δ ” denotes the change from t to $(t-1)$. Eq. (9) is thus a first-difference Heckit model (Heckit-FD), predicting changes of outcomes as a function of changes of the independent variables.⁴ Since the Heckit-FD models estimate longitudinal models of multiple job searches for the same respondent, we only use individuals who used contacts to find a job more than once during the two time periods.

We choose variables to serve as the exclusion restriction, to help identify the Heckit or Heckit-FD model. In the GDR study, we use whether the respondent has siblings to serve as Z_i . Due to the family plan policy implemented, in the China study we use marital status as the Z_i . The justifications are twofold: having siblings or being

³ Although it not absolutely necessary to apply the Heckit method, without imposing such exclusion restrictions it will be extremely difficult to distinguish the sample selection from a misspecified function form of the substantive model. For example, in Lin (2003), the variables appearing in the selection model but not in the job outcome model are work experience and the rank of the father's work unit education. In Marsden and Hurlbert (1988), the exclusion restriction is the job experience. However, there may be good reason to suspect that these variables (e.g., work experience) should also appear in the substantive model. That is, the exclusion restrictions are qualitatively not good, so that there may still be estimation biases in these applications.

⁴ We corrected the standard errors since the asymmetric variance matrix of the Heckit estimator is complicated by its dependence on the first-step estimates. An alternative strategy to Eq. (9) is to directly incorporating the inverse Mills ratio into the differenced model so that the “heckman” command in Stata can automatically produce the correct standard errors. We also tried this strategy and got similar results and the same pattern of significance.

married leads to a larger pool of potential job helpers, and thus predicts a higher possibility of using informal channels to find jobs.⁵ Meanwhile, number of siblings or marital status in itself does not directly affect one's job outcomes. Although the validity of exclusion restrictions is always debatable, it is more convincing to use them compared to using the variable of experience, which was used in previous studies by Marsden and Hurlbert (1988) and Lin (2003).

4.1. The case of the GDR

We have developed a theoretical model regarding the implications of contact resources and occupational homophily, and have preliminarily revealed that same-occupation profoundly determines the role of social capital in the US, by replicating 1970 DAS. We ascribe the significant interaction effect between contact resources and occupational homophily (SO1) to the feature of a modern labor market, where within-occupation job information flows through networks can be more productive in job searches. In order to further examine the empirical implications of our theoretical model, a useful method is to explore whether the causal role of contact resources and the moderating effect of occupational homophily can be observed in a society with completely different labor markets, where same-occupation does not lead to any advantages. The former GDR could be an excellent context in which to conduct such analyses, given its highly centrally planned economy and the rigid state job-assignment system, in which influence rather than information matters.

Despite the rich literature regarding social networks and job searches in post-unification East Germany (e.g., Volker and Flap, 1999; Benton et al., 2015; Rosenfeld et al., 2004; Solga and Diewald, 2001; Mayer et al., 1999; Truppe and Rosenfeld, 1998), studies focusing on the GDR before the fall of the Berlin Wall are relatively rare. However, they do provide us with a picture of how job mobility was deeply controlled by the state. Specifically, Uunk et al. (2005:395) have summarized that “in the planning schemes East German firm managers lacked autonomy in changing the set production goals for the firms. Mobility of persons between occupations and firms was not desirable (...) Central planning restricted East German employers in their personnel planning. Firms were controlled through centralized redistribution of investment capital, salary funds and other financial means. Managers could be sanctioned through reduced funds once their firm showed too much firm-external mobility.” Given the highly restricted job mobility, Grünert (1996:41, cited in Uunk et al. op. cit.) has actually labeled the labor market in the GDR as an “agglomeration of strongly from the outside protected firm-internal labor markets with high firm tenure of employees and relatively low rates of cross-firm mobility”.

The state-assignment job allocation process in the GDR implies that occupational homophily is not more productive for the social resources through the ties linking a job seeker and the contact person. As already mentioned, in such an institutional setting it is the position-related influence rather than information that matters for job outcomes (Bian, 1997; Lin, 2002). In addition, due to the large size of state-owned firms and the firm loyalty fostered by guaranteed social welfare and the state-socialist ideology in the GDR, many job changes are within-firm (Uunk et al., 2005). This further suggests that a same-occupation contact person in the GDR context has neither information edges

(e.g., vacancy information in other firms) nor influence advantages (e.g., connecting to the managers of the job-seeker's firm), as compared to a different-occupation contact person. Therefore, if our argument is correct, we expect in our GDR analyses that although contact resources causally matter for job outcomes, their effect does not hinge on the same occupation (SO1).

The GDR data analyzed here was collected by Volker and Flap (1999) in 1992, 1993 and 1994 in two cities, Leipzig and Dresden. In this paper, we focus on the data from 1992 to 1993, as the two surveys asked retrospective questions about job and social networks during the socialist period (the first job and the job in 1989). For a detailed description of the data collection one can refer to Volker (1995). We present the selective statistics of variables in Table A1 in the Appendix. To fit the first-difference models, we drop from the sample those who never used contacts and those who used them only once. There are 115 workers using contacts for their first job, with 194 workers using contacts for their 1989 job. The overlap between them is 66. Thus, the final number of “twice users” is 132. Considering the small sample size, in the robustness check, for those who merely used contacts for the 1989 jobs, we will recode the “missing value” of the contacts' prestige for their first jobs as zero, in order to obtain more observations. We will also use the bootstrap method and the least absolute value model.

Although Volker and Flap (1999) collected a two-wave panel data (first job and job in 1989), they actually fitted two OLS models (see Volker and Flap, 1999: Table 6, p. 25) respectively for the two time periods. In Table 2 we replicate their two models (see Model 1a and Model 2a), and extend their study by fitting the Heckit model (see Model 1b and Model 2b) to correct for sample selection. For respondents who used contacts to get their first and 1989 jobs, we fit the OLS, Heckit and Heckit-FD models respectively for comparison purposes (see Model 3a, 3b and 3c). We use the same set of variables, except that we include the age variable. The reason for incorporating the age variable is that we consider it as a proxy for experience, which predicts individual outcomes and increases between the first job and the job held in 1989.

As Models 1a and 2a show, contacts' prestige is significantly associated with a respondent's occupational prestige. The magnitude of the role is very close to the one reported in Volker and Flap (1999): getting a job from a contact with a one unit higher occupational prestige score is associated with a .338 higher occupational prestige score for the first job, or with a .187 higher status for the 1989 job, everything else being equal.⁶ Results from Model 1b and Model 2b show that, controlling for the inverse Mills ratio slightly changed the contact effects in both models (one from .338 to .339, the other from .187 to .171). Notice that the coefficient of the inverse Mills ratio in Model 2b is significant and the Wald test shows that Heckit estimates are more reliable than OLS estimates,⁷ implying the presence of a sample selection problem in modeling the 1989 job.

Turning now to 132 observations from 66 workers using contacts to get both jobs: importantly, it is very likely that those who used contacts for both jobs share some unobserved individual attributes. We therefore first fitted the OLS and Heckit model on them. As can be seen in Model 3a of Table 2, without allowing for sample selection, the estimated contact effect is .336. Comparing the results from Model 3a to Model 3b, we find that controlling for the inverse Mills ratio substantially decreases the effect from .336 to .151, which is now close to the estimation obtained from Model 2b (.171). This suggests that the Heckit method does effec-

⁵ The number of siblings is often determined by the gender structure of the first two children in a family. For example, parents are more likely to have a third child if their first and second children are of the same gender. In fact, sibling number has been used as an instrumental variable to identify neighbourhood effects on individual outcomes (see e.g., Bentolila et al., 2010).

⁶ The coefficients of the effect of contacts' prestige estimated by Volker and Flap (1999) are 0.35 and .17.

⁷ When heteroscedasticity-robust standard errors were reported, Stata 12.0 used a Wald test rather than a Likelihood Ratio test to compare OLS and Heckit models.

tively alleviate the biases derived from selection. Finally, using the Heckit-FD method, Model 3c takes into account both selection and omitted variables, and gives rise to a larger estimation (.206) than that from Model 3b. As for the implications of the larger FD estimates, we will interpret this below.

Given the small sample size of Model 3c, we carried out a robustness check on a larger sample. That is, for those who did not use contacts for the first jobs but used contacts for their 1989 jobs, we used social resources for obtaining the first job are regarded as zero.⁸ This increases the observations from 132 workers to 388. Although not reported here, the estimates obtained from the increased samples are very similar to what is reported in Table 2. The estimated effect obtained from the Heckit-FD analysis of the 388 observations is .190 and is statistically significant. In addition, we performed 1000 bootstrap replications and computed bootstrap estimates for the standard errors of the key coefficients. The bootstrap standard error of the estimated coefficient of contact prestige is .094, slightly larger than the original .090. However, this does not change our conclusion that the effect of social capital is significant. Finally, we also used the least absolute value model and obtained consistent results.

To further test our argument on the moderating effects of occupational homophily, we include SO1 and its interaction term with contact's prestige (CP) in the models in Table 3. The results from Model 1a through Model 3b show that none of the interaction terms between CP and SO1 are positive and significant. Taking into account both sample selection and time-fixed confounders, Model 3c shows that contact resources matter to respondents' jobs at a .05 level, with a coefficient of .555. In addition, SO1 does not affect job outcomes at all and the negative coefficient of the interaction term is not significant at a .05 level, meaning that the magnitude of the partial effect of CP does not hinge on SO1, consistent with our theoretical expectation and our interpretation of the different institutional settings. In fact, given a .1 alpha level, SO1 even decreases the role of contact prestige. A negative interaction term has also been found in Model 1a and 1b.

The findings in Table 3 strikingly contradict what we have found in the 1970 DAS – as we expected – demonstrating that resources provided by same-occupation contacts do not pay off as compared to that provided by different-occupation contacts in a socialist society. Since the sample size of Model 3c in Table 3 is not large and we cannot construct SO1 if we include those who only used contacts in securing their 1989 jobs, to ensure thoroughness we first of all performed 1000 bootstrap replications and computed bootstrap estimates for the standard errors of the key coefficients. Although not reported here in detail, results from our bootstrap analyses did not change our conclusions at all, since the bootstrap standard errors of the estimated role of contact prestige and the interaction terms are .275 and .279, respectively, showing a significant effect of social capital and an insignificant moderating role of SO1. To verify this, the least absolute value models are fitted to the data and yield similar results.

4.2. The case of China

Exploiting multiple identification strategies, we have already shown that there is a causal link between contact resources and job outcomes among workers in the GDR, and that the effects do not hinge on SO1, consistent with our theoretical model. We use the job-assignment system in a command economy in the

GDR to account for the ambiguous significance of the occupational homophily in affecting the role of contact resources. If our proposals about the causal role of social capital and its interaction with SO1 are indeed true, we should find that the setting in a transitional society (e.g., a society with both a command economy heritage and an emerging modern labor market) is somewhat in between the findings of the GDR and the US. We therefore extend our study to post-reform China.

Although pre-reform China had a command economy, similar to the GDR in many ways (Cai et al., 2008), the job-assignment system was gradually replaced by a labor contract system with greater labor market freedom after the reform and opening-up which began in the late 1980s (Bian, 1994; Meng, 2000). With the heritage of a centrally planned regime and burgeoning market forces, the Chinese economy and labor market during the social transition has been seen as a hybrid type (Nee and Yang, 1999). We suspect that like both the US and the GDR, contacts genuinely matter to job outcomes in China; like the US but unlike the GDR, the role of contact resources is larger in the case of same-occupation contact persons. The reasons for this are straightforward and twofold.

First, regarding the causal role of social capital, since contact resources (be they information or influence) have been proved to improve job outcomes in either a market economy (the US) or a command economy (the GDR), it is natural to expect that they also pay off in a transitional economy (China). In fact, empirical findings ranging from the late-1980s to recent days generally support this argument (e.g., Bian, 1997; Bian and Ang, 1997; Lin, 2003; Bian et al., 2015). Second, the literature shows that information plays a role in job searches in transitional China. For instance, Bian et al. (2015) explicitly distinguished network information and influence (favoritism) as two types of contact resources, and identified positive roles of both in relation to a series of job outcomes (e.g., wages, job-worker matched positions, positions of higher hierarchical bridging, and positions of higher market connectedness) among the Chinese people. In addition, migration studies have also revealed that job information is of key importance in matching people to jobs in urban China (Chen, 2012; Lu et al., 2013). In this regard, occupational homophily represents a pin-point channel through which information flows would increase the role of information. The data used in the present analysis is from the 2002 Chinese Household Income Project Survey (CHIPS, 2002). Jointly conducted by the Rural Survey Group of the National Bureau of Statistics of China (NBSC) and the Institute of Economics of the Chinese Academy of Social Science (CASS), CHIPS (2002) surveyed a sample of 20,632 urban residents from 22 provinces in 2003. In CHIPS (2002), the questionnaires asked respondents to provide detailed information regarding the respondents' current and previous jobs. In particular, the respondents were asked the question: "Have you changed jobs in the last three years?"⁹ This thus allows us to compare the different results for different levels of social resources used by the same respondent to find jobs within a 3-year period. We present selective statistics of key variable in Table A2 in the Appendix.

The CHIPS (2002) data does not provide any information on the occupational prestige of contacts and respondents. We therefore use the log value of annual wages as the dependent variable.¹⁰ Although incomes and status are different measures of job outcomes, the mechanisms of the effects of contact resources on them could be comparable, as our theoretical models propose. We measure contact resources by ascertaining whether a contact is

⁸ For those who used contacts for their first jobs but not for the 1989 jobs, things are more complicated. This is because it is possible that the resources of the contact for the first job still matter in finding the 1989 job. If so, it is problematic to assume the missing value to be zero for this group of people.

⁹ This refers to the period from 2000 to 2002.

¹⁰ Since wage information was within the last 3 years, memory error can be largely avoided.

Table 2
Models of the effects of contact prestige on occupational prestige (GDR data).

Independent variables	Contact-users for first job		Contact-users for 1989 job		Contact-users for both jobs		
	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 3c
	OLS Y = first job prestige	Heckit Y = first job prestige	OLS Y = 1989 prestige	Heckit Y = 1989 prestige	OLS Y = 1989 prestige	Heckit Y = 1989 prestige	Heckit-FD Y = prestige change
Contact's prestige	.338(.084)^{***}	.339(.081)^{***}	.187(.069)^{**}	.171(.064)^{**}	.336(.158)[*]	.151(.074)[*]	.206(.090)[*]
Sex (male = 0)	4.566(1.933) [*]	4.727(1.982) [*]	−.876(1.757)	−1.30(1.958)	−.036(3.307)	−.882(2.256)	−
Education	4.247(2.151)	4.228(2.064) [*]	1.869(.696) ^{**}	1.996(.824) [*]	2.375(1.315)	1.658(.804) [*]	−
Father's education	.500(.773)	.530(.738)	−.588(.672)	−.229(.720)	−1.195(1.384)	−.111(.781)	−
Father's prestige	.102(.091)	.105(.092)	.057(.076)	.005(.087)	.047(.153)	.085(.099)	−
First job prestige	−	−	.400(.090) ^{***}	.437(.099) ^{***}	.141(.181)	.536(.097) ^{***}	−
Age	−.614(1.366)	−.592(1.306)	.148(.112)	.158(.124)	−.005(.188)	.222(.140)	−.070(.251)
Inverse Mills ratio	−	1.237(4.502)	−	12.285(3.133) ^{***}	−	10.722(2.312) ^{***}	18.862(11.925)
_cons	16.229(19.690)	13.927(19.372)	6.569(5.052)	−5.390(5.905)	17.555(10.330)	2.599(7.502)	−3.807(6.746)
R ²	.421	−	.403	−	.360	−	.10
Wald test χ^2	−	.08	−	13.82	−	13.89	−
No. of observations	115	115/476	194	194/476	66	66/194	132/476

Notes: Standard errors are in parentheses and robust to heteroscedasticity.

^{*} $p < .05$.

^{**} $p < .01$.

^{***} $p < .001$.

Table 3
Models of the implications of SO1 and contact's prestige on occupational prestige (GDR data).

Independent variables	Contact-users for first job		Contact-users for 1989 job		Contact-users for both jobs		
	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 3c
	OLS Y = first job prestige	Heckit Y = first job prestige	OLS Y = 1989 prestige	Heckit Y = 1989 prestige	OLS Y = 1989 prestige	Heckit Y = 1989 prestige	Heckit-FD Y = prestige change
Contact's prestige (CP)	.794(.090)^{***}	.813(.074)^{***}	.137(.100)	.070(.101)	.237(.203)	.116(.122)	.555(.254)[*]
Same occupation (SO1)	1.034(1.632)	2.283(1.694)	3.175(1.744)	5.501(2.325)[*]	.223(2.941)	4.753(2.270)[*]	4.528(4.232)
CP # SO1	−.708(.139)^{***}	−.759(.134)^{***}	.142(.117)	.202(.115)	.177(.213)	.131(.142)	−.446(.247)
Sex (male = 0)	1.866(1.907)	.079(2.225)	−.799(1.736)	−1.606(2.102)	−.076(3.260)	−1.009(2.207)	−
Education	3.431(1.921)	3.633(2.411)	1.967(.692) ^{**}	2.198(.900) [*]	2.499(1.315)	1.722(.813) [*]	−
Father's education	.252(.654)	.148(.771)	−.619(.658)	−.154(.755)	−1.335(1.385)	−.272(.767)	−
Father's prestige	.123(.071)	.077(.077)	.061(.072)	.001(.092)	.034(.157)	.091(.092)	−
First job prestige	−	−	.379(.089) ^{**}	.424(.105) ^{***}	.135(.183)	.504(.104) ^{***}	−
Age	−.251(1.153)	−.484(1.496)	.144(.113)	.161(.134)	−.002(.190)	.238(.132)	−.111(.238)
Inverse Mills ratio	−	−11.146(3.734) ^{**}	−	16.318(4.108) ^{**}	−	−8.659(3.502) [*]	−12.479(13.376)
_cons	29.526(16.001)	50.149(24.409) [*]	15.724(6.079) [*]	−3.151(8.525)	36.762(12.467) [*]	7.871(7.192)	−2.678(6.740)
R ²	.540	−	.420	−	.367	−	.171
Wald test χ^2	−	8.63	−	7.83	−	4.34	−
No. of observations	115	115/476	194	194/476	66	66/194	132/476

Notes: Standard errors are in parentheses and robust to heteroscedasticity.

^{*} $p < .05$.

^{**} $p < .01$.

^{***} $p < .001$.

Table 4
Models of the effects of contact's resource on log-ages (Chinese data, CHIPS, 2002).

Independent variables	Contact-users for prior job		Contact-users for current job		Contact-users for both jobs		
	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b Heckit	Model 3c Heckit-FD
	OLS Y = prior wage	Heckit Y = prior wage	OLS Y = current wage	Heckit Y = current wage	OLS Y = current wage	Y = current wage	Y = wage change
CCP member contact	.238(.055)*	.284(.053)***	.116(.007)***	.120(.009)***	.187(.075)	.106(.007)***	.156(.027)*
Hukou status							
Rural (the resident city)	-.069(.037)	-.142(.083)	-.276(.009)***	-.262(.010)***	-.705(.120)*	-.238(.008)***	-
Urban (other city)	-.124(.024)*	-.168(.023)***	.298(.005)***	.305(.005)***	.513(.197)	.142(.007)***	-
Rural (other city)	-.329(.139)	-.337(.112)**	.218(.009)***	.224(.009)***	.066(.156)	.301(.013)***	-
Female	-.388(.021)***	-.320(.030)***	-.199(.015)***	-.139(.010)***	-.180(.069)	-.203(.013)***	-
Schooling years	.032(.009)*	.058(.007)***	.066(.002)***	.026(.003)***	.041(.011)*	.069(.001)***	-
Work unit sector							
State-owned enterprises	-.041(.076)	.354(.065)***	-.160(.019)**	-.211(.011)***	-.640(.332)	-.132(.016)***	.256(.053)
Collectives	.437(.064)**	.629(.060)**	-.295(.029)**	-.183(.012)***	-.233(.304)	-.306(.012)***	.214(.053)*
Joint ventures	.372(.054)**	.113(.078)	.046(.024)	.274(.025)***	-.228(.262)	.064(.046)	-.095(.017)*
Private/self-employed	.197(.081)	-.175(.105)	-.218(.029)**	-.004(.016)	.576(.258)	-.168(.016)***	.095(.017)*
Experience	.051(.010)*	.049(.007)***	.008(.002)*	.001(.002)	.038(.010)*	.00003(.0003)	.018(.010)
Experience-squared	-.001(.0003)*	-.0004(.0003)	.00001(.00002)	.00002(.0004)	-.001(.0002)*	.0002(.00005)***	-.0003(.0002)
Inverse Mills ratio	-	-1.039(.068)***	-	.581(.023)***	-	-.226(.043)***	-.123(.043)
Provinces dummy	Yes	Yes	Yes	Yes	Yes	Yes	-
_cons	8.499(.121)***	9.783(.100)***	9.027(.035)***	8.569(.034)***	9.403(.318)***	9.119(.074)***	10.293(.011)***
R ²	.461	-	.346	-	.540	-	.173
Wald test χ^2	-	182.65***	-	1786.97***	-	26.08***	-
Z of ER	-	2.33*	-	8.59***	-	2.31*	-
No. of observations	130	130/789	764	764/9366	71	71/764	142

Notes: Standard errors in parentheses are robust to heteroscedasticity and adjusted for clustering on hukou status; ER = exclusion restriction included in the selection model (marriage); the reference groups are urban hukou of the resident city, non-CCP member, governmental institution, and males.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

a member of the Chinese Communist Party (CCP).¹¹ It has been broadly discussed that party membership, which reflects political loyalty, plays a significant role in the socialist stratification process (Gerber, 2000; Wong, 1996; Walder et al., 2000).¹² In our models we will control for age, gender, education, experience, hukou (house registration) status, work unit sector and provincial dummies in the models. To fit the first-difference model, those who never used contacts or those who used contacts only once are also excluded from the sample. Finally, we get 764 workers who had used contacts for their current jobs, and 130 workers who had used contacts for prior jobs.¹³ The overlap between them is 71 “twice users”, providing us with 142 observations to fit the first-difference model.

Empirical results from regression models are presented in Table 4. First of all we examine “one-time users”. As is shown in Table 4 the results from the OLS models are consistent. Specifically, a contact's CCP membership is significantly associated with both the respondent's prior wage (Model 1a) and the current wage (Model 2a). Controlling for the unobserved individuals characteristics which determined whether or not the respondent used contacts to find a job does not change the result (Model 1b and Model 2b). The coefficients of the inverse Mills ratio are both significantly different from 0, indicating the presence of a sample selection problem that plagues standard OLS estimates.

¹¹ In addition, using a non-prestige indicator to operationalize contact resources and job outcomes can be of extra help since occupational prestige in itself is relatively ambiguous in measuring resources or labor market outcomes (e.g., positions with the level of same prestige may have different salary or administrative ranks).

¹² Although it can be speculated that political loyalty plays less of a role under a market economy in the making than it did in the Mao era (see e.g., Nee, 1989, 1991, 1996), the path-dependence argument maintains that the pre-existing political capital would take the new form of socioeconomic capital and would therefore ensure that individuals were able to maintain their advantageous position (Bian and Logan, 1996; Shu and Bian, 2003; Walder, 2003).

¹³ Hereafter “prior job” refers to the job before the current job and after 1999, although many non-changers (1999–2001) did have previous jobs before 1999.

We then move on to look at these “twice-users” of contacts. This shrinks the number of observations to 71, leading to an insignificant coefficient of a contact's political resources (Model 3a). This is almost certainly due to the unobserved individual attributes shared by “twice users”. As expected, using the Heckit approach to correct for sample selection, Model 3b reveals that a contact's CCP membership significantly pays off. Finally, ruling out time-fixed individual attributes, the Heckit-FD model (Model 3c) increases the contact effects from .106 to .156, implying that the time-fixed individual ability may be negatively correlated with social capital, as in the case of their peers in the GDR. Note that the role of a contact's political resources is not marginal: finding a job with the help of a CCP member contact produces a 15.6% higher annual wage than does using assistance from a non-CCP member contact. This is nearly comparable to the wage differences arising from gender or rural–urban difference.

We also carried out a robustness check on a larger sample, which includes those who did not use contacts for the previous jobs but used contacts for their current job. This increases the observations from 142 to 424 (212 respondents). Although not reported here, the estimates obtained from the increased samples are very similar to those reported in Table 4: the estimated effect obtained from the Heckit-FD analysis of 424 observations is .151 and is statistically significant. Like what we did with GDR analyses, we also computed the bootstrap standard error (.011) and found that it is even much smaller than the original standard error (.027) reported in Model 3c in Table 4, suggesting that our results are reliable. Finally, the results from a least absolute value model are also close to those from Model 3c.

We proceed to explore the wage implications of occupational homophily. In Table 5 we incorporate same-occupation (SO1) and its interaction term with contact's political resources in models. As Model 3c shows, allowing for sample selection and ruling out time-fixed factors, a contact's CCP membership still matters (.151) at a .05 alpha level, while occupational homophily (SO1) alone does not. The positive and significant coefficient of the interaction term (.119) shows that the partial role of contact's political resources

Table 5
Models of the implications of SO1 and contact’s resources on log-wages (Chinese data, CHIPS, 2002).

Independent variables	Contact-users for prior job		Contact-users for current job		Contact-users for both jobs		
	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 3c
	OLS Y = prior wage	Heckit Y = prior wage	OLS Y = current wage	Heckit Y = current wage	OLS Y = current wage	Heckit Y = current wage	Heckit-FD Y = wage change
CCP member contact	.178(.061)	.189(.053)***	.103(.010)***	.111(.010)***	.327(.040)**	.085(.012)***	.151(.030)*
Same occupation (SO1)	-.436(.021)***	-.685(.067)***	-.098(.027)*	-.567(.065)***	-.187(.093)	-.104(.033)**	-.064(.026)
CCP # SO1	.330(.036)**	.343(.032)***	.062(.044)	.019(.041)	.486(.135)*	.113(.035)***	.119(.003)***
Hukou							
Rural (the resident city)	-.195(.036)*	-.185(.041)***	-.279(.011)***	-.273(.010)***	-.659(.133)*	-.248(.011)***	-
Urban (other city)	-.225(.021)**	-.244(.015)***	.284(.005)***	.268(.006)***	.550(.188)	.135(.007)***	-
Rural (other city)	-.396(.149)	-.413(.124)***	.225(.009)***	.200(.008)***	.102(.191)	.314(.013)***	-
Female	-.419(.025)***	-.438(.022)***	-.200(.014)***	-.221(.016)***	-.157(.065)	-.203(.012)***	-
Schooling years	.025(.009)	.030(.006)***	.066(.002)***	.082(.002)***	.052(.016)*	.069(.002)***	-
Work unit sector							
State-owned enterprises	-.154(.078)	-.116(.074)	-.164(.019)**	-.138(.018)***	-.774(.427)	-.137(.015)***	.230(.027)***
Collectives	.347(.062)*	.379(.057)***	-.297(.033)**	-.342(.036)***	-.245(.354)	-.309(.017)***	.166(.039)**
Joint ventures	.220(.059)*	.178(.051)***	.049(.025)	.021(.027)	-.277(.315)	.064(.047)	-.109(.017)**
Private/self-employed	.103(.082)	.045(.077)	-.219(.029)**	-.301(.037)***	-.625(.305)	-.173(.014)***	.049(.012)*
Experience	.051(.010)*	.053(.010)***	.008(.002)*	.009(.002)***	.042(.015)	.0004(.003)	.001(.012)
Experience-squared	-.001(.0003)*	-.001(.0002)***	.00008(.00003)	.0001(.00003)***	-.001(.0002)*	.0002(.00005)***	-.00003(.0002)
Inverse Mills ratio	-	-.183(.050)***	-	-.283(.024)***	-	-.193(.067)*	-.145(.043)*
Provinces dummy	Yes	Yes	Yes	Yes	Yes	Yes	-
_constant	8.743(.127)***	8.959(.114)***	9.063(.034)***	9.359(.028)***	9.207(.347)***	9.132(.061)***	10.309(.010)***
R ²	.483	-	.348	-	.555	-	.202
Wald test χ^2	-	12.41***	-	180.66***	-	7.88**	-
Z of ER	-	3.20***	-	7.03***	-	2.09*	-
No. of observations	130	130/789	764	764/9366	71	71/764	142

Notes: Standard errors in parentheses are robust to heteroscedasticity and adjusted for clustering on hukou status; ER = exclusion restriction included in the selection model (marriage); the reference groups are urban-hukou of the resident city, non-CCP member, governmental institution, and males.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

is substantially larger among those who used within-occupation contacts. Note that the wage advantage brought about by same-occupation (.119) is quite considerable, comparable to the wage difference between working in a joint-venture and a government institution (.109), other things being equal. The moderating role of SO1 is inconsistent with what we revealed in the GDR study, but similar to the result obtained from the DAS replication. In general, as we expected, the Chinese picture lies between the GDR and the US cases. That is, contact resources causally improve wages among the Chinese, and the role of social capital is larger among contacts who provided within-occupation job-leads. Finally, given the small sample size, we performed bootstrap replications and obtained the bootstrap standard error of the coefficient of contact’s political resources (.0118) and that of the interaction terms (.0023), confirming that both the role of the social capital and the moderating effect of occupational homophily are significant at the .001 level. Again, as we did with the GDR study, we also fit least absolute value models and obtained similar results, although not reported here.

The differences and similarities between the results from the US and those from China deserve further interpretation. In our DAS replication, contact prestige merely works only among same-occupation contact-users. This is perhaps due to the fact that job information was the dominant mechanism through which contact prestige played its labor market role in the Detroit area in the 1970s. By contrast, in our China study, political contact resources (i.e., CCP membership of the contact person) may signal a mixture of a job information component and an influence component, working among both same-occupation and different-occupation contact-users. As a matter of fact, if we strictly assume that information pay off flows exclusively through same-occupation ties, we could even separate the effects that stem from the information component (.119) from the role played by the influence component alone (.151) in people’s political social capital.

5. Heterophily versus homophily in the selection of network members

In our analysis of the data from the two countries, the Heckit-FD estimates of contact effects are both larger than those obtained from the Heckit models. Apparently, the Heckit models underestimated the role of contact resources, which means that the omitted time-fixed variable in the Heckit models must be associated with both labor market outcomes and contact resources, but in opposite directions. If we assume that time-fixed individual ability is the major confounder and that ability is positively associated with job outcomes, this finding suggests that the omitted ability is negatively associated with contact resources. This implies the presence of “heterophilous selection” in the formation of social networks, by suggesting that individuals of a lower level of ability mobilized social capital from those who are in possession of more resources. Importantly, this is inconsistent with the argument of social homophily.

Although this appears to be counter-intuitive, similar findings are obtained in other social capital studies. For example, based on the 1988 Tianjin Study in China, [Bian \(1997\)](#) reported that around half of job seekers had used contact-chains (i.e., job search chains involving a second intermediary/indirect linkage). And, the ultimate helpers for contact-chain users are significantly more resourceful as compared to the helpers for direct-tie users. Importantly, [Bian \(1997\)](#) revealed that the ultimate helper was usually targeted first and the search for the “right” intermediary was planned accordingly by the job seeker. Another example is the finding that women in China rely more on contacts to find jobs and have longer and stronger contact-chains than men, suggesting that women compensate for their disadvantaged labor market position by accessing and mobilizing more resources ([Lin, 2003](#)).

The presence of “heterophilous selection” is not confined to the labor market in China. For instance, [Renzulli and Aldrich \(2005\)](#)

reported that female business owners' social networks in the US are more heterogeneous in terms of occupations, and they also activate more resources from others than male business owners. Based on the data from the 1990 Decennial Census for the Boston MSA, Bayer and Ross (2009) found that the effects of local social capital (i.e., neighborhood poverty rates, and average neighborhood education levels) on labor market outcomes are substantially underestimated without taking into account individual unobservables. Their interpretation is that "the finding that OLS estimates of neighbourhood effects are biased downwards is consistent with the hypothesis that individuals with poor unobservables in terms of labour market outcomes compensate for these unobservables by sorting into locations with better employment prospects" (Bayer and Ross, 2009:25).

Consistent with previous findings, our results reveal once more that people may initiate social ties with those who are dissimilar to them. From a social resource perspective, Lin (2008) argued that when resources embedded in homophily-based networks are not sufficient for a specific goal – to find a good job, for example – individuals may reach beyond the inner layer of their social networks to get access to resources from others who are different from themselves. Therefore, it is problematic to assume that homogeneous and heterogeneous networks are an either/or choice, since the motivation behind the selection of friends or accessing contact resources can be complex. In fact, social homophily is not an exclusive selection principle in the formation of social ties. For example, McPherson et al. (2001) conceded that individual attributes including education, occupation and network position do not necessarily lead to homophily-based selection. Empirical findings can be seen in Verbrugge (1977) and Louch (2000), see also examples for heterogeneity in intimate relationships (e.g., Robins and Boldero, 2003; Domínguez and Watkins, 2003).

Theoretically, selecting dissimilar others into ones' network may occur for two reasons: First, as Lin (2008) stressed, heterophilous selection is often beneficial for instrumental actions such as job-seeking, as it provides access to more social resources (Briggs, 1998, 2002; Wellman and Gulia, 1999). Second, meeting opportunities determined by socioeconomic compositions may also lead to heterophilous selection. As Blau (1977) argued, individual preferences are not the only factor that determines the composition of egocentric networks. E.g., in a society with high inequality of social resources, preference for similarity in the formation of social ties may result in an instrumental disadvantage, because those who are positioned in the lower level of the social ladder will never reach the higher social strata through their contacts if they prefer similar others. Future research should show study more deeply the conditions under which homophily maximizes returns to social capital and which dimension of homophily matters to what degree and for what type of goal.

6. Conclusion and discussion

While it is theoretically intriguing that contacts matter for individual outcomes, most previous studies that provided support for social resource theory (Marsden and Hurlbert, 1988; Volker and Flap, 1999; see Lin, 2001 for a review) have not adequately addressed the endogeneity problem. This is especially problematic in research on the effects of social capital. Mouw (2003) replicated Marsden and Hurlbert's (1988) 1970 Detroit Area Study and found that social homophily accounts for the association between contact resources and job outcomes. This finding profoundly challenges the social resource theory advanced by Lin (1999). However, in recent studies, Lin and his colleagues defended the theory by taking into account the "correctly" defined occupational similarity (SO2).

In the present paper, we have reviewed both sides of the Mouw–Lin debate, and we extended the discussion. On the one

hand, we noticed that Mouw's (2003) replication amounts to fitting models merely on sub-samples among those who used low-efficiency social resources to find jobs in the US. Consequently, the findings in Mouw (2003) in general do not undermine social resource theory. On the other hand, we argued that Mouw's (2003) concern about same-occupation (SO1) points to the possibility that the effects of social capital are conditioned by similarity. We extended the 1970 DAS by including SO1 and its interaction term with contact resource, and thereby found that SO1 actually strengthened the role of contact resources. We suspected that same-occupation contacts affect job attainment in a modern labor market, but that this is not necessarily the case in a different institutional context, such as a state job-assignment system – where job information does not matter.

Based on findings from the DAS replication, we then moved on to analyze data from other institutional contexts, i.e. from the GDR and post-reform China, to explore the role of contact resources for job outcomes, and how that role is affected by same-occupation contacts. In general, we tested our arguments by comparing findings from three countries with quite different labor market institutions: a free market economy in the US, a command economy in the GDR, and a transitional economy in China. Regarding the methodological issues, we followed Mouw's (2006) call for appropriate advanced models and rich data to be used when conducting causal analyses. Paying close attention to the omitted variable and sample selection problem in the GDR and China analyses, we compared the estimates from the standard OLS model, the Heckit model and the Heckit-FD models. Although our DAS replication is cross-sectional, due to data limitations, conclusive evidence from the GDR and China study were both obtained under the counterfactual framework.

One might consider it as a limitation of our study that we applied Mouw's SO1 instead of Lin's SO2. However, we were in particular interested in how returns of social capital change when homophily is accounted for, since we believe that Mouw's concern about similarity should not just be considered as inappropriate coding. In fact, studies that include SO2 show that it does not moderate the role of social capital. Our analysis in Table 1 also arrives at this conclusion, when including SO2. We also ran models with the DAS data that included both SO2 next to SO1 (available upon request), which did not alter this conclusion. Taken this together, our focus was on SO1 and we did not include SO2 to keep the models parsimonious.

Our results call for a further study of "heterophilous selection", since we revealed that individuals with unobserved labor market disadvantages may have adopted a compensatory strategy to mobilize more social resources during job searches. Our findings are consistent with the proposition that the formation of social networks could be a conscious process in which "individuals develop social capital by cultivating, maintaining, and utilizing certain social relationships" (Bian, 2008:81). In fact, what has been found here echoes Lin's (2008) argument that we cannot assume that a high homophily over some characteristics implies a low heterophily across all characteristics. At least, we argue that the presence of social homophily in regard to a particular set of dimensions does not preclude the possibility that individuals also initiate social ties with those who are dissimilar to themselves with respect to social resources.

We have contributed to the accumulated sociological literature of social capital by using the combined-model method to alleviate potential estimation biases. Despite the intuitive appeal of the idea that the effects of social capital are strong, scepticism based on an unobserved ability or social homophily perspective may always exert its own appeal. Unfortunately, datasets that can be used for such analyses are relatively rare. As McDonald (2015:310) put it, "comprehensive understanding of these processes is hampered by the dearth of long-term panel datasets containing detailed employment and job finding information alongside information

on contacts and occupational networks". From this point of view, although sample sizes in our study are small we have offered some tentative causal evidence regarding the role of contact resources. We thus recommend that more panel data or rich data containing exogenous variables that are capable of serving as instrumental variables be on the top of any future social capital research agenda.

Perhaps more important, we have added to the literature by providing insights into how the effects of information and influence on job outcomes vary given different institutional contexts. We have revealed that the significance of Mouw's (2003) replication is further reaching than thought at first; it is more than erroneous coding of similarity. Further, the implication of Mouw's version of occupational homophily in the process of job matching deserves further investigation under different labor market conditions. In general, we have found that in a free market institutional

system with a modern labor market, job information facilitates job attainment and within-occupation information is more productive. In contrast, in a totalitarian institutional system, with a job assignment system influence rather than information matters causing the same-occupation advantages to disappear. In a transitional society, the scenario is somewhat in between the two crystalized types of social institutions. Although other studies in the field of social capital like Granovetter (1995), Lin (1999), Bian (1997), and Volker and Flap (2001) have already drawn our attention to variations in the returns of social capital in different institutional contexts, the mechanisms of job information and influence have yet to be distinguished and adequately investigated. Future studies have to delve further into this issue.

Appendix.

Table A1
Descriptive statistics of GDR data.

Individual attributes	Contact-users (for first job) Observations = 115		Contact-users (for 1989 job) Observations = 194		Contact-users (for both jobs) Observations = 132		All respondents Observations = 476	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	38.13	8.15	38.49	8.02	37.70	8.40	38.44	8.02
Contacts' prestige 1 (SEI first job)	49.08	13.42	-	-	50.13	13.39	49.08	13.42
Contacts' prestige 2 (SEI job 1989)	-	-	54.02	14.54	54.94	14.37	54.02	14.54
Occupational prestige 1 (SEI first job)	47.49	12.44	48.05	13.03	48.98	13.27	47.38	12.56
Occupational prestige 2 (SEI job 1989)	48.80	13.97	49.45	14.26	50.83	14.69	47.92	13.16
Father's prestige (SEI)	46.10	12.53	44.43	11.83	46.12	11.95	44.84	12.21
Father's educational attainment	3.37	1.44	3.30	1.45	3.53	1.32	3.24	1.48
Educational attainment	4.12	1.48	4.11	1.60	4.29	1.56	4.04	1.55
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
Female	72	62.61%	107	55.15%	82	62.12%	266	55.88%
Having siblings	63	54.78%	120	61.86%	68	51.52%	205	64.08%
Same occupation (first job)	41	35.65%	-	-	28	42.42%	-	-
Same occupation (job 1989)	-	-	80	41.24%	32	48.48%	-	-

Notes: Occupational prestige is measured by the Treiman occupational prestige scale for the Germany, developed with the National Bureau for Statistics. Education: 1 = finished school before 8th grade; 2 = high school until 8th grade; 3 = high school until 10th grade; 4 = high school (university qualification); 5 = technical college; 6 = finished university.

Table A2
Descriptive statistics of Chinese data (CHIPS, 2002).

Individual attributes	Contact-users prior job (N = 130)		Contact-users current job (N = 764)		Contact-users both jobs (N = 142)		All respondents (N = 9366)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	35.81	8.47	39.47	8.38	36.17	8.69	41.34	8.29
Years of schooling	10.79	2.49	10.33	2.81	10.66	2.62	11.39	2.99
Annual wage of prior job (RMB)	8022	8058	-	-	8420	8939	9130	8930
Annual wage of current job (RMB)	-	-	10,995	9490	9120	7359	12,479	8964
Experiences (prior job)	18.28	9.51	-	-	18.86	9.64	23.90-	9.38
Experiences (current job)	-	-	23.15	9.37	19.50	9.82	23.96	9.42
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
Female	68	52.31%	402	52.62%	36	50.70%	4083	43.59%
CCP member contact (prior job)	43	33.08%	-	-	19	26.76%	-	-
CCP member contact (current job)	-	-	356	46.60%	20	28.17%	-	-
Hukou status (house registration)								
Urban (the resident city)	121	93.08%	725	94.90%	63	88.73%	9187	98.09%
Rural (the resident city)	4	3.08%	23	3.01%	4	5.63%	97	1.04%
Urban (other city)	3	2.31%	7	.92%	3	2.82%	45	.48%
Rural (other city)	2	1.54%	9	1.18%	3	2.82%	37	.40%
Work unit sector (prior job)								
Governments/institutes	12	9.23%	-	-	6	8.45%	99	12.55%
State-owned enterprises	25	19.23%	-	-	11	15.49%	325	41.19%
Collectives	9	6.92%	-	-	4	5.63%	100	12.67%
Joint ventures	3	2.31%	-	-	2	2.82%	13	1.65%
Private/self-employed	81	62.31%	-	-	48	67.61%	252	31.94%
Work unit sector (current job)								
Governments/institutes	-	-	146	19.11%	8	11.27%	2950	31.50%
State-owned enterprises	-	-	152	19.90%	10	14.08%	3218	34.36%
Collectives	-	-	61	7.98%	3	4.23%	603	6.44%
Joint ventures	-	-	28	3.66%	4	5.63%	173	1.85%
Private/self-employed	-	-	377	49.35%	46	64.79%	2422	25.86%
Same occupation (prior job)	21	16.15%	-	-	12	16.90%	-	-
Same occupation (current job)	-	-	149	19.50%	21	29.58%	-	-
Being married	101	77.69%	676	88.48%	54	76.06%	8639	92.24%

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