Motherhood Penalties, Referral Networks, and Labor Market Outcomes

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A thesis submitted for the degree of

Doctor of Philosophy

Department of Economics

University of Essex

October 2024

AUTHOR’S DECLARATION

I, Guohua He, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

I declare that the work in this thesis was carried out in accordance with the requirements of the University’s Regulations and that it has not been submitted for any other academic award.

Chapter 1 is a collaborative effort with Jingyi Li, a PhD candidate at Institute for Social and Economic Research (Essex). Chapters 2 and 3 of this thesis are authored solely by me. I am responsible for any errors.

Signed by: Guohua He

Date: 15th of October 2024

Summary

This thesis consists of three separate papers:

The first paper, titled “How Does Fertility Relaxation Policy Affect the Motherhood Wage Penalty?”. This chapter examines the impact of China’s two-child policy on the motherhood wage penalty using CFPS and the DiD approach. The study reveals that, post-policy, one-child mothers face a 9% increase in wage penalty, as employers anticipate higher maternity costs. In contrast, two-child mothers experience an 8% reduction in wage penalty due to the policy legitimizing their status. Moreover, the study identifies statistical discrimination against one-child mothers and taste-based discrimination relief for two-child mothers as the main mechanisms.

The second paper, titled “how do different types of referrals affect inequality?”. This paper examines the effect of referral on labor outcomes by distinguishing between the information transmission and screening mechanisms. Using the SCE dataset, we isolate these effects and find that referral significantly increase job-finding probability through the information transmission mechanism, while improve matching quality and starting wages only through the screening mechanism. To further examine the screening mechanism, we define two types of referrals engage the different roles, namely screening ability and reputational cost. For low-noise signal job seekers, employee referrals improve outcomes, while co-worker referrals benefit high-noise signal job seekers.

The third paper, title “The Role of Networks Size and Quality in Labor Market Outcomes”. This paper addresses these gaps by examining both the direct and indirect effects of network size and quality on starting wages. We find that larger networks significantly raise starting wages, especially for low-ability job seekers through higher hiring probability. Additionally, while network quality alone does not increase wages, its interaction with referrals significantly boosts starting wages and wage growth, especially for high-ability job seekers in network-dependent occupations like sales.

To my family, for their constant encouragement and belief.

Acknowledgements

I would like to express my deepest gratitude to my main supervisor, Dr. Ran Gu, for his continuous support, insightful guidance, and encouragement throughout my PhD journey. His expertise and mentorship have been instrumental in shaping this research, and I am profoundly thankful for his unwavering support and dedication.

I would like to extend my sincere thanks to Dr. Aditya Kuvalekar, my second supervisor, for his invaluable contributions to the theoretical framework of this research. In addition, I would like to thank Professor Matteo Richiardi for his valuable feedback on my second chapter. His expertise and constructive suggestions were essential in refining this part of the dissertation.

I am also deeply indebted to Professor Jayant Ganguli, Chair of my supervisory board, for his valuable feedback and guidance. His thoughtful insights have greatly enhanced the quality of my work, and his support has been crucial throughout the process.

Lastly, I extend my gratitude to my two examiners, Thomas Cornelissen and Naijia Guo, as well as the anonymous reviewers for their thorough evaluations and constructive feedback, which have greatly contributed to enhancing this dissertation. Their thoughtful critiques have been invaluable in strengthening both the rigor and clarity of my research.

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Chapter 1

How Does Fertility Relaxation Policy Affect the Motherhood Wage Penalty

Abstract

This paper examines the effect of fertility relaxation on the motherhood wage penalty, utilizing the China's two-child policy as quasi-experiment. Our findings reveal heterogeneity in the policy's effect. For one-child mothers, the policy change signals employers to update their beliefs about potential for future fertility, leading to a significant 9% increase in the wage penalty for them post-policy, compared to the non-mothers. Conversely, mothers who previously violated the policy by having a second child experienced an 8% decrease in their wage penalty as their illegal status was lifted post-policy. These outcomes are primarily attributable to changes in job discrimination rather than shifts in human capital. A further mechanism analysis suggests that the increase in the anticipatory wage penalty for one-child mothers is driven by statistical discrimination, whereas the decrease for two-child mothers is linked to taste-based discrimination post-policy.

This chapter is a collaboration with Jingyi Li, a PhD student at ISER, University of Essex

1.1 Introduction

In labor economics, the relationship between female labor market participation and fertility choices highlights a significant economic dilemma. As more women enter the workforce, shifts in fertility patterns become evident, prompting both developed and developing countries to enact policies aimed at achieving equilibrium. These measures, including parental leave, maternity leave, and childcare support, are intended to mitigate the post-birth penalty and increase the fertility rates. However, while aimed at supporting families, these policies increase anticipated costs for employers, such as maternity insurance, maternity leave, and the need for replacement hires, due to the expectation of future fertility plans (Jessen et al., 2019). This can adversely affect women's labor market outcomes, potentially leading to a rise in the anticipatory motherhood wage penalty through statistical discrimination, even before the occurrence of childbirth. [1] While much of the existing literature has concentrated on the motherhood wage penalty post-childbirth (Correll et al., 2007; Lundborg et al., 2017; Kleven et al., 2019; He et al., 2023), our research probes the theory that the motherhood wage penalty emerges not only as a consequence of actual motherhood (ex-post) but also from anticipatory actions (ex-ante) by employers discriminating on the basis expected future fertility. To be our best knowledge, the exploration of the anticipatory motherhood wage penalty prior to childbirth has been limited, with the available studies seeming to yield mixed results (Kleven et al., 2020; He et al., 2023; Agarwal et al., 2024).

This hypothesis can be tested within the quasi-experimental framework provided by the reform of China's two-child policy (TCP).[2] Before the TCP, which known as the one-child policy (OCP), women were restricted to only have one child, hiring one-child mothers could have been seen by employers as a more stable option, with less likelihood

[1] Mothers earn less than non-mothers, a phenomenon known as the "motherhood wage penalty".

[2] The two-child policy was nationwide, and no expert or media outlet had accurately predicted their implementation prior to its announcement (Jia et al., 2021; He et al., 2023; Agarwal et al., 2024).

of having to accommodate costs related to maternity leaves, replacement hires (Jessen et al., 2019). The relaxation of the OCP, which granted women the right to have a second child and was officially enacted on 28th December 2013 (2014, hereafter), serves as a pivotal shift in employers' expectation and belief.[3] This policy change signals to employers to update their beliefs that one-child mothers could potentially have another child at any time. As a result, in pursuit of maximizing profits, employers may engage in statistical discrimination against these mothers by offering them lower wages, anticipating the additional costs this change might entail.[4,5] One the other hand, for mothers who previously violated the OCP by having a second child, the motherhood wage penalty they face may stem from taste-based discrimination.[6] Employers might hold a bias against those mothers who flout the legal norms established by the OCP. However, the introduction of the TCP could mitigate this taste-based discrimination, potentially leading to an increase in wages for these mothers post-policy.

To estimate the immediate effect of fertility relaxation on anticipatory motherhood wage penalty, we use a representative dataset, the China Family Panel Studies (CFPS), and employs Difference-in-Differences (DiD) regression. Our baseline approach consists of comparing mothers of one-child to non-mothers to derive the motherhood child penalty for the first child, and then to test whether this changed with the switch from the OCP to the TCP. We argue that, under the assumption that non-mothers remain unaffected by the TCP, this approach identifies changes in the motherhood penalty that are driven by employers updating their expectation of how many children a one-child mother is planning to have. However, even though the policy's intended focus was on one-child and two-child mothers, the policy could also change employers' anticipation

[3] The initiation of the two-child policy, known as the selective two-child policy, rolled out across all provinces in January 2014 and fully enacted by September of the same year, applying to families where either partner is an only child. Subsequently, in 2016, the Chinese government enacted the universal two-child policy, permitting all couples to have two children. For more detailed information, please refer to Section 2.

[4] Some women may bear an additional child as a consequence of the policy, attributing any increase in the wage penalty to this child effect rather than to the policy itself. To isolate the child effect, we only concentrate on a subset of women who did not have an additional child after the two-child policy.

[5] Economists have traditionally modeled statistical discrimination as fully rational based on the signaling model, which also is called "rational stereotyping" (Phelps, 1972; Arrow, 1972; Coate and Loury, 1993).

[6] Taste-based discrimination involves bias in hiring and wage decisions driven by employers' personal prejudices against certain groups, independent of their qualifications (Becker, 1957; Charles and Guryan, 2008; Lang and Lehmann, 2012).

of how many children a non-mothers might decide to have in the future. Under this scenario we would still identify a valid reform effect on the motherhood penalty, but it would be unclear whether the change in the motherhood penalty would be driven by wage changes of mothers, or of non-mothers. To address this concern, we exploit the fact that in 2014 the two-child policy was initially introduced as a selective policy (applying to families where either partner is an only child), before turning into a universal policy (permitting all couples to have two children) in 2016. Based on the one-child policy was enacted in December in 1982, we operate under the assumption that individuals born after 1982 are likely to be "only child", while those born before or in 1982 are not. With this assumption, the selective two-child policy only applied to women from households where at least one of the partners in the couple was born after 1982, but it did not apply to couples where both partners were born before or in 1982. Based on whether any of the partners was born after 1982, this allows us to estimate the effects of the selective two-child policy within the group of one-child mothers (and for the group of non-mothers), conditional on a quadratic of age effects.

Our empirical analysis uncovers five primary findings. Firstly, wage penalty for the one-child mothers is significantly increase 0.09 log points, while for the two-child mothers is significantly reduce 0.08 log points after the relaxation of OCP. This pattern aligns with trends observed in the China Statistical Yearbook, where an increase in second births and a decline in first births are documented. To ascertain that our effects are indeed driven by one-child mothers (rather than non-mothers), we initially compare the non-mothers were born after 1982 and born before or in 1982, examining the periods before and during the selective two-child policy (i.e., 2012 and 2014). Our analysis reveals that whether born after 1982 does not affect non-mothers. Then, we conduct a tripe DiD model using as an additional control group women from households where both partners were born before or in 1982.[7] The triple DiD coefficient indicates a significant increase in the wage penalty for targeted one-child mothers by 0.11 log points during the selective two-child policy period. Collectively, both results confirm

[7] The treatment group under the selective two-child policy, we focus on women from households where either partner born after 1982.

our conjecture that the effect of the two-child policy on the motherhood penalty is mainly driven by one-child mothers. Moreover, as expected, given that the universal two-child policy affects all one-child mother's cohorts, whether one of the partners was born after 1982 does not affect the motherhood penalty under the universal two-child policy regime. Nonetheless, we conduct a series of robustness checks to confirm the validity of our baseline findings. These checks included examining pre-trends, potential confounding events, and issues related to self-selection. Our results have proven to be consistent and robust across various model specifications and definitions of the sample.

Secondly, we examine the mechanisms behind the post-policy increase in wage penalty for one-child mothers due to statistical discrimination, and the reduction in wage penalty for two-child mothers resulting from taste-based discrimination. We conduct two analyses in this regard. On one hand, if the policy's effect is rooted in statistical discrimination, younger mothers might face more pronounced effects of statistical discrimination. Our findings indicate that post-policy, younger one-child mothers face a wage reduction of 0.08 log points, whereas the wages of older one-child mothers show no significant change. Furthermore, among these younger one-child mothers, those with children aged 4-7 years face the highest statistical discrimination, with their wage penalty increasing by 0.11 log points post-policy.[8] On other hand, we extend our investigation through a comparative analysis of the public and private sectors, informed by OCP which historically enforced more stricter regulations on women in the public sector. This scenario suggests public sector employers had higher expectations of women having a second child post-policy. Our findings corroborate this, showing a post-policy increase in the wage penalty for public sector one-child mothers by 0.12 log points, whereas those one-child mothers in private sector see no such increase.

To explore how taste-based discrimination affects the wage penalty for two-child mothers, we examine the illegal stigma effect's persistence. Specifically, we compare mothers who had a second child illegal under OCP with those who had one legal under

[8] This is attributed to the historical pattern where the typical gap between the first and second child falls within 4-7 years.

SCP, during the two-child policy period in 2016 and 2018. Our analysis supports the hypothesis, revealing that during the TCP period, mothers who had their second child illegally under the OCP experience a lower log hourly wage by 0.11 log points compared to those who had their second child legally under the TCP. Nonetheless, we do not observe any significant changes in education levels for one-child or two-child mothers post-policy. Interestingly, post-policy, the working hours for one-child mothers even have significantly increased by 0.03 log points compared to non-mothers. This suggests that the human capital channel is unlikely to be a primary driver behind the increased motherhood wage penalty according to the policy.

This study considerably expands three strands of literature on in three significant aspects. Firstly, it contributes to research on fertility policy on the labor market outcomes. Most of existing studies focus on the impact the fertility policies on the female labour market outcome, such as maternity leave and parental leave (Havnes and Mogstad 2011; Dahl et al., 2016; Kleven et al., 2024), but they found the mixed effect or even positive effect. Although some research shows the fertility policy have negative effect, a few paper focus on the "anticipation effect" of the fertility policy (Kleven et al., 2024; He et al., 2023; Agarwal et al., 2024). Among these paper, they mainly explored how the fertility policy on the gender wage gap, but we focus on the "anticipation effect" of fertility policy on the motherhood wage penalty.

Secondly, although studies by Budig and England (2001; 2012), Anderson et al. (2002), and Killewald and Bearak (2014) have explored aspects of the motherhood wage penalty, none have specifically investigated how fertility policy intersects with the motherhood wage penalty, particularly in terms of the anticipatory effects of the motherhood wage penalty. Our research addresses this void by examining the mechanisms behind the anticipatory changes in the motherhood wage penalty, focusing on shifts in statistical and tasted-base discrimination following fertility policy changes.

Thirdly, our study contributes to the literature on fertility policy in China and other developing countries. Under the OCP, there was a significant reduction in the total fertility rate (TFR), and post-TCP, the TFR remained relatively stable (Zhang 2017; He et al., 2023; Agarwal et al., 2024). The fertility relaxation policy may lead to changes of anticipatory motherhood wage penalty in the labor market through the job discrimination channel, which in turn may have feedback effects on fertility behavior (Catalina and Jean, 2005; Schoen et al., 1999). Consequently, following the relaxation of fertility restrictions, individuals without a strong inclination towards parenthood may decide against having children, contributing to a decrease in lower-order births in the short term and a dramatic long-term increase in the TFR.

The structure of this paper is organized as follows: The ensuing section delve into the policy background. Section 3 introduces the dataset, while Section 4 outlines the empirical strategy employed. Section 5 presents the empirical analyses conducted. Following this, Section 6 explores the underlying mechanisms driving the observed effects. Section 7 is dedicated to a heterogeneity analysis. Finally, Section 8 concludes with a discussion.

1.2 Policy Background

The voluntary family-planning initiative launched in 1971 evolved into a more structured policy by 1979. By December 1982, the principles of this policy were embedded in the Constitution. It is important to clarify that this family-planning program is often associated with limiting couples to a single child, hence commonly known as the "one-child policy". Couples who surpassed the policy's birth limitations encountered strict penalties, such as the inability to secure local household registration for their offspring, facing heavy fines, or the risk of losing their employment.

After the implementation of the OCP in 1982, the total fertility rate (TFR) in China fell from 5.1 to 2.23, with a net decrease in population of 11.58 million. Since then, China has entered a new period with low fertility rates coexisting with an ageing population. In January 2014, the Chinese government enacted the selective two-child policy, which allows a couple to have two children if either partner is an "only-child".[9,10] However, as reported by the NHFPC[11], the TFR only increased from 1.60 in 2011 to 1.65 in 2014, and then it decreased to 1.60 in 2015 (see Appendix A1.2).[12,13] In other words, the selective two-child policy appears not to have achieved its purpose of actively promoting fertility. The Chinese government decided to implement the universal two-child policy[14,15] in 2016. Compared to the selective two-child policy, this new policy allowed any couple to have two children, irrespective of their own sibling statuses. The aim of both policies was to relax the restriction on fertility for households and was expected to result in a substantial increase in births. However, as reported by NHFPC, the number of births in China has not increased significantly, and the TFR even decreased by 0.12 during two years following the introduction of universal two-child policy.

Nonetheless, the effect of the adjustment should be evaluated primarily by changes in second births, rather than broad shifts in overall fertility levels. A closer analysis of the fertility data reveals that second births have increased after the policy adjustment. Between 2013 and 2017, second child births rose from 5.11 million to 8.83 million, whereas first child births declined from 10.56 million to 7.24 million (see appendix A1.3). The proportion of second children in total births also rose from 31.2% to 51.8% (China Statistical Yearbook, 2020). In contrast, the proportion of first children in total births decreased from 65.6% to 41.4% after the two-child policy was implemented (see Figure 1). Thus, the insignificant improvement of the overall fertility level may be due to the decline of first births.

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[9] Notably, the "only-child" must have no siblings or half-siblings.

[10] Prior to the enactment of the selective two-child policy, which permitted couple to have two children if either partners are an "only-child", the Chinese government had introduced a different version of a selective two-child policy. This earlier policy allowed couples to have two children only if both partners were "only child". Implementation of this earlier policy varied regionally between 2000 and 2012; for instance, Guangdong province adopted it in 2002, while Hunan province followed suit in 2011. However, the target demographic of this early selective two-child policy accounted for only approximately 4% of China's total population, rendering its overall impact relatively limited (see Appendix A.3).

[11] National Health and Family Planning Commission of the People's Republic of China.

[12] Although the National Health and Family Planning Commission of the People's Republic of China (NHFPC) and many demographers have adjusted the statistics and concluded that the total population and fertility rate in the new century is around 1.6 (see Figure 1).

[13] Notably, the Chinese Population census does not count the total fertility rate after 2015.

[14] The universal two-child policy liberalizes restrictions on whether a couple is an "only-child", allowing every couple to have two children.

[15] In the appendix A.1, further details about the policy are provided.

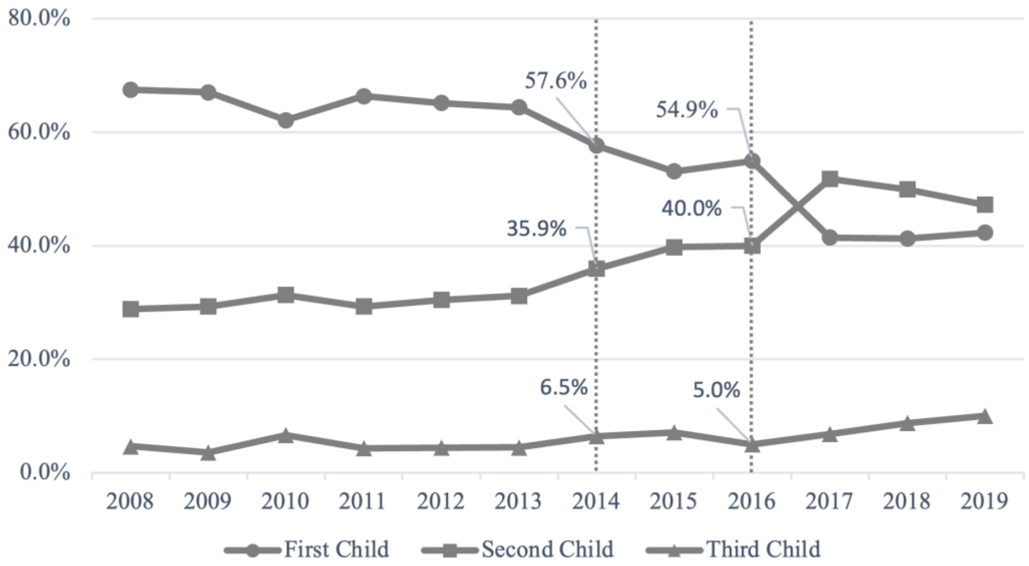

FIG. 1.1— Order-specific Fertility Rate.

Source: China Statistical Yearbook, 2020

1.3 Data

The data for this study comes from the China Family Panel Studies (CFPS) follow-up surveys conducted in 2010, 2012, 2014, 2016, and 2018 (Xie et al., 2020). The CFPS is a nationally representative, large-scale household survey conducted by the China Social Science Survey Centre of Peking University. The survey was conducted in 162 counties across 25 provinces using the stratified multi-stage sampling method. The study covers a variety of topics including the economy, education, family relations, family dynamics, and population movement, with a target sample of 16,000 households. Based on the research objectives, the raw sample was modified in the following ways. Firstly, women between 16- and 49-year-old are kept. The minimum legal working age in China is 16, and the fertile period of women is considered to be between 14 and 49. Secondly, employed individuals with a labor income lower than the threshold set in the Minimum Labor Income Act are dropped.[16] Thirdly, we exclude women with more

[16] In China, the Minimum Labour Income Act is about 460, 530, 700, 1000, 1280 yuan in 2010, 2012, 2014, 2016, 2018, respectively. Some observations below the Act are dropped because of misreported or underreporting.

than two children from our analysis as they represent a small fraction of our sample (only around 5.6%), and the majority of them are unemployed.[17] After these adjustments, the effective sample contains 15,405 women aged from 16 to 49, 67% of whom are employed.

As Table 1 shown, the hourly wage is from the main job, including all salary, bonuses, cash perks, and in-kind allowances, after taxes have been deducted. The average hourly wage in the full sample is 14.52 yuan, with 15.08 yuan for the non-mothers and 14.14 for the mothers. Only about 61.3% of women are employed in the labor market. Among them, 73.5% women without children are employed and 63.5% of women with one child are employed, while only 43.9% of women with two children are employed. The primary explanatory variable in our study is the total number of children a woman has.[18] The mean number of children per working women in the full sample is 0.855. Additionally, 48.5% are the one-child mothers, and 18.6% women have two children, reflecting the impact of the "one-child" policy previously imposed by the Chinese government. For more detailed information on individual, occupational, and family characteristics, refer to Table 1.

Table 1.1: Summary Statistics

	All		Non-mothers		Mothers		
Variables	Mean	SD	Mean	SD	Mean	SD	Diff
Dependent							
Hourly Wage	14.52	33.03	15.08	23.16	14.14	38.11	0.945
Independent							
Num. child	.855	.704	-	-	-	-	-
One child	.483	.5	-	-	-	-	-
Two children	.186	.389	-	-	-	-	-
Personal Chara.							
Age (year)	33	8.8	24	4.7	37	6.7	12.72***
Education (1-9)	2.04	.425	2.15	.427	2.07	.392	0.081***
Work Chara.							

[17] This is likely due to the one-child policy, which made it rare for families to have three or more children.

[18] This approach is inspired by Waldfogel (1997), who posited that the presence of children can impact a mother's salary throughout her lifetime. This is because changes in human capital and the job discrimination, resulting from motherhood, are likely to influence a woman's entire working life. In the Chinese context, this is particularly relevant as legislation regarding birth policies focuses on the total number of children a woman has, rather than the ages of the children.

Working hours	219	71	214	66	212	68	1.41
Public (private)	.266	.442	.245	.43	.324	.468	-0.07***
Firm size	541	4409	623	5656	603	4843	19.46
Family Chara.							
Savings (k)	7.199	4.63	7.053	4.602	7.534	4.639	-0.48***
House hours	59	28	50	29	62	26	12.45***
Parents at home	.264	.441	.67	.47	.085	.28	0.58***
Parents education	2.491	1.105	2.544	.941	2.273	.907	0.27***
Heckman							
Employed rate	.613	.487	.735	.441	.635	.482	0.10***
Married Status	.773	.419	.225	.418	.946	.225	-0.72***

NOTE.—Data is sourced from the CFPS spanning 2010 to 2018. ‘Wage’ denotes hourly income from the primary job. ‘Working hours’ and ‘House hours’ are represented monthly. ‘Parents at home’ signifies the presence of the respondent’s both parents in their household. ‘Parents education’ refers to the average educational attainment of the respondent’s parents. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

1.4 Empirical strategies

To estimate the causal effect of the policy on motherhood wage penalty, we use a difference-in-differences (DiD) model. The selective two-child policy, rolled out across all provinces in 2014, applying to families with either partner is an only child.[19] This expanded eligibility to encompass around 18% of the population (refer to appendix A1.4 for more details). With the universal two-child policy in January 2016, the policy’s application became nationwide. Importantly, the application of both the selective and universal two-child policies was uniform across the country, eliminating the feasibility of distinguishing treatment and control groups based on geographical differences.[20]

Building on the above policies, we initially assume that non-mothers are unaffected by the two-child policy, an assumption grounded in the policy’s aim towards mothers with one child and two children. Additionally, non-mothers, particularly those who are

[19] The initiation of the selective two-child policy occurred in 1984. However, it is important to note that eligibility for this policy was restricted to households where both the mother and father were only children. Consequently, this target demographic constitutes a relatively minor segment of the population, accounting for approximately 3% of the total (see appendix A1.4). Furthermore, the implementation of the selective two-child policy varied regionally between 1984 and 2012; for example, Guangdong province adopted this policy in 2002, whereas Hunan province did so in 2011. Given its limited scope, our analysis shifts focus to the subsequent selective two-child policy.

[20] The selective two-child policy was progressively introduced across various provinces from January 2014 and was comprehensively enforced nationwide by June of the same year. However, the CFPS predominantly collected its data in July and August, with over 90% of the data gathered during these months. This timing significantly constrains the ability to delineate treatment and control groups based on geographical variations.

currently not mothers, might project a perception that they do not intend to have children in the future. Hence, our "control group" consists of non-mothers, whereas the "treatment groups" include one-child and two-child mothers in the baseline model. However, concerns persist regarding the possibility that employers may still harbor expectations that some non-mothers, especially younger ones, might choose to have two children in the future post-policy. Such anticipatory actions by employers could introduce bias into the DiD analysis, as both control and treatment group affected by the policy.

To address this concern, we conduct two robustness checks by capitalizing on the distinctions in family types targeted by the various iterations of the two-child policy. Specifically, the selective two-child policy of 2014 targeted families where "either partner is an only child", while the universal two-child policy of 2016 applied to all families. Therefore, for the 2014 analysis, women in couples where at least one partner is an only child serve as the "control group", since they fall outside the policy's scope.[21] Addressing the challenge of accurately identifying women belonging to the specified "control group" involves utilizing the one-child policy initiated in December 1982 as a reference point.[22] We operate under the assumption that individuals born after 1982 are likely to be "only child", while those born before or in 1982 are not (additional details can be found in appendix A1.4). This presumption allows us to classify "only child" non-mothers as the treatment group and non "only child" non-mothers as the control group. Should the non-mothers truly remain unaffected by the policy, we anticipate the treatment effect to be insignificant post-policy.

Except for the non-mothers, we apply a similar identification strategy for a robust test focusing on one-child mothers. The control group consists of one-child mothers from households where both partners were born before or in 1982, indicating they were not directly targeted by the selective two-child policy. The treatment group comprises

[21] Despite the brief two-year interval between the selective and universal two-child policies, it is important to contextualize this within the more extensive timeline of the one-child policy, which spanned over 32 years. Employers in 2014, accustomed to the longstanding one-child policy framework, would likely not have anticipated the rapid introduction of the "universal two-child" policy just two years later, in 2016.

[22] In the CFPS, identification of an "only child" can be directly through the question, "*How many brothers/sisters do you have?*" However, this question was only posed in the 2010 survey. Given the CFPS's low follow-up rate of 1.6, this creates a substantial challenge in utilizing this method for identifying "only children" in subsequent periods.

women from households where either partner born after 1982.[23,24] Additionally, we incorporate this treatment dummy in the analysis of the 2016 universal two-child policy to perform a placebo test. Given that the selective two-child policy of 2014 was only applicable to either partner is an only child, we anticipate a significant treatment effect in this context. In contrast, given that the universal two-child policy of 2016 was applicable to all households, we anticipate an insignificant treatment effect in this scenario (more details see Section 1.5.C).

Additionally, heterogeneity might affect the wage regression. One of the heterogeneity problems relates wage levels varying across years, provinces, and industry. To eliminate this time-invariant unobserved factor, we use the fixed effect DiD model. We do not use individual fixed effects because the average repeat rate over the five collections was only 1.6, implying the data is closer to cross-sectional (although the CFPS is designed to be longitudinal). Overall, the fixed effect DiD model can be expressed by:

$$lwage_{it} = \sum_{k\in\{1,2\}} \left(\beta_k D_{it}^{(k)} + \gamma_k D_{it}^{(k)} P_t\right) + X_{it}\eta + \varphi_t + \gamma_o + \mu_p + \varepsilon_{it} \qquad (1)$$

where $lwage_{i,t}$ represents the log of hourly wage of female respondent i at time t; $D_{it}^{(k)}$ denotes a group of dummy variables that indicate whether the woman has one child and two children ($k \in \{1,2\}$), in contrast to control group is non-mothers. P_t is a dummy variable signifying the two-child policy, assigned a value of 1 for year in and after 2014 and 0 for all other years. X_{it} is a vector of control variables, including personal characteristic (e.g., age, age square and education), family characteristics (e.g.,

[23] To process it, we match the household information to the women. In the CFPS, accurately identifying a woman's husband presents a challenge due to the data structure, where individuals residing together are assigned a shared family ID. Consequently, we match women to other individuals using the household ID, resulting in multiple records that could potentially represent a woman's husband, father, or son. To refine our matching process and more accurately identify spousal pairs, we exclude records where the absolute age difference between the woman and the matched individual is greater than 25 years.

[24] In our analysis, we conduct two separate regressions to examine the policy's impact on non-mothers and one-child mothers. This distinction is necessary because the majority of non-mothers are typically unmarried. Incorporating household information into our analysis results in the loss of 20.3% of observations for unmarried non-mothers groups.

household working hours, family saving, parents' education and parents at home), and working background (e.g., type of sector, and firm's size).[25] Additionally, we include year fixed effects φ_t, industry fixed effect γ_o, province fixed effect μ_p.

Nonetheless, there are five principal concerns regarding the identification of the causal impact of the fertility relaxation policy on the motherhood wage penalty. Firstly, it is necessary to consider that some women may bear an additional child as a consequence of the policy, attributing any increase in the wage penalty to this child effect rather than to the policy itself. To isolate the child effect, we only concentrate on a subset of women who did not have an additional child after the selective two-child policy. By doing so, we can mitigate the impact on the motherhood wage penalty that arises from human capital impacts due to having additional child. Secondly, in the DiD framework, ensuring unbiased causal inference hinges on the establishment of equivalent groups for analysis. To achieve a balanced and precise comparison, we use Propensity Score Matching (PSM) with nearest neighbour matching based on the observed personal controls. This methodological approach leads to the exclusion of 114 observations, enhancing the comparability of treatment and control groups.

Thirdly, the DiD methodology could be influenced by confounding events. According to our assessment, no event in 2014 and 2016 seems to have selectively affected the motherhood wage penalty. However, to rigorously validate our analysis, we conduct a permutation test as a robustness measure. In this test, instead of employing the actual data on mothers and non-mothers, they are randomly allocated while ensuring a consistent ratio. Following this, we implement the model (1) to evaluate the policy's effect on this placebo cohort and document the resulting DiD estimate. By repeating this process 500 times, we are able to create a histogram of the placebo DiD estimates.

Fourthly, the validity of causal inference in the DiD approach also relies on the assumption of parallel trends. This assumption posits that, absent any policy alterations, the average outcomes for the control and treatment groups would have followed identical trends over time. To substantiate this critical assumption, we employ the event

[25] In general, controlling for these characteristics addresses compositional changes in these variables and helps to ensure that any such changes are not driving the results, since the policy effect is identified from time variation.

study approach. Building on this methodological foundation, we estimate the following event study model:

$$lwage_{it} = \sum_{j=-2, j\neq -1}^{2} \delta_j T_j + \sum_{k=\{1,2\}} \theta_k D_{it}^{(k)} + \sum_{j=-2, j\neq -1}^{2} \sum_{k=\{1,2\}} \lambda_{jk} \left(T_j \times D_{it}^{(k)}\right) + X_{it}\eta + \varphi_t + \gamma_o + \mu_p + \varepsilon_{it} \quad (2)$$

where T_j representing a series of dummy variables for the years 2010, 2014, 2016, and 2018, relative to the reference year of 2012 ($j \neq -1$). The interaction term $T_j \times D_{it}^{(k)}$ isolates the treatment effect for each year j since the event. $\lambda_{j,-2}$ are the coefficients of interest, identifying the pre-trend of the two-child policy on hourly wage.

A fifth issue arises from self-selection into employment, which presents two concerns. Firstly, prior to the policy, approximately 67% of women were employed. If mothers opt out of employment upon having a child, then the wage regression analysis may be biased due to observing only a truncated sample. Secondly, the policy itself could influence employment patterns. Should women either choose to leave employment or face discrimination leading to unemployment post-policy, excluding these individuals from the analysis could lead to an underestimation of the policy's effect. To rectify this self-selection, we use a Heckman correction, with "marital status" as the exclusion restriction based on its impact on labor force participation but not on wages for women (Heckman, 1997). There are some papers where Heckman himself has used marital status in the first stage of his selection model (Heckman, 1974; Heckman and MaCurdy, 1980). However, later research has generally indicated that marriage leads to some growth in wages for women, calling into question the validity of marital status as an exclusion restriction (Hill, 1979; Krashinsky, 2004). However, Meng et al. (1997) found no evidence of marital status affecting female workers' wages once accounting for education, occupation and other factors in China. Thus, in the context of our study, we assume that marital status remains a valid exclusion restriction. We also conduct a series of tests to demonstrate that "marital status" serves as a valid instrument variable in the subsequent analyses (see Table 6). Applying the Heckman two-step model typically involves the following stages. In the first stage, a Probit model predicts the likelihood of employment ($employ_{it}$) for individual i at time t, formulated as:

$$
\begin{aligned}
&Probit(employ_{it}) \\
&\qquad = \zeta marr_{it} + \sum_{k\in\{1,2\}} (\beta_k T_{itk} + \gamma_k T_{itk} S_t + \rho_k T_{itk} U_t) \\
&\qquad + X_{it}\eta + \varphi_t + \gamma_o + \mu_p + \varepsilon_{it}
\end{aligned}
\tag{3}
$$

where $marr_{it}$ indicates marriage status. Following this, we calculate the inverse Mills ratios imr_{it} to address selection bias. The second stage involves estimating the primary regression model for respondents who are employed, incorporating the imr_{it} to adjust for selection bias:

$$
\begin{aligned}
lwage_{it} = &\sum_{k\in\{1,2\}} (\beta_k T_{itk} + \gamma_k T_{itk} S_t + \rho_k T_{itk} U_t) + X_{it}\eta + imr_{it} + \varphi_t \\
&+ \gamma_o + \mu_p + \varepsilon_{it}
\end{aligned}
\tag{4}
$$

1.5 Empirical Results

This section discusses the effect of fertility relaxation policy on motherhood wage penalty. The analysis proceeds in four steps. Initially, a naïve DiD model is utilized to examine how the policy changes the motherhood wage penalty.[26,27] Subsequently, a series of robustness checks, including Heckman and triple-DiD models, are conducted to further validate the policy's effect on the motherhood wage penalty. The third step involves examining potential mechanisms behind these observed effects. The analysis

[26] We also document how the "two-child" policy changes fertility behavior for each birth order by using sequential Logit model in Appendix A1.5

[27] As displayed in Figure 2 and Appendix A1.5, the "two-child" policy appeared to influence the fertility decisions of women in two significant ways: fewer non-mothers transitioned into one-child mothers, and a greater number of one-child mothers transitioned into two-child mothers.

concludes with a heterogeneity analysis to uncover varying impacts across different groups.

A. Main results

Table 2 shows the effect of fertility relaxation policy on motherhood wage penalty by number of children. Recognizing age as a crucial determinant of both fertility and wages, we initially adjust for age and its square in column 1 to account for the age effect comprehensively. Beyond capturing the age effect, we also include controls for education level and urban residency, acknowledging these as significant factors influencing both fertility rates and wage levels in China, as evidenced by previous studies (Jia et al., 2013; Yu and Xie, 2014). The results show that the hourly wage gap between one-child mother and non-mothers is about -0.02 log points prior to the policy implementation, a finding that lacks statistical significance. In contrast, for the two-child mothers, the wage gap is significantly larger at -0.24 log points, with statistical significance at the 1% level. These wage gaps also could stem from other various factors, with the primary consideration being the control of differences in observable, among mothers and non-mothers in the workforce and family characteristics.[28] Taking these factors into account and controlling for firm and family characteristics, the wage gap for one-child and two-child mothers becomes lower compared to model (2). However, this adjusted analysis indicates that motherhood wage penalty for one-child and two-child mothers remain the similar to model (1), as detailed in column 2.

It is important to note that model (2) does not incorporate spatially fixed effects, predicated on the anticipation that the magnitude of the motherhood wage penalty varies by spatially. But as we discussed before, the one-child and two-child policy is enforced differently in different spatially, such as provinces and industry. When province and industry fixed effects are accounted for, the observed motherhood wage

[28] Prior research emphasizes the significant role of both firm and family characteristics in influencing the motherhood wage penalty. For instance, Duvivier and Narcy (2015) note that larger corporations often implement more “family-friendly” policies to alleviate the motherhood wage penalty. Additionally, wealthier and larger families often mitigate the motherhood wage penalty by engaging childcare support, either through assistance from the women’s parents or by employing childcare providers (Ruhm, 2004).

penalties for one-child remain insignificant, while for two-child mothers reduces to 0.18 log points ($p<0.01$), prior to the policy implementation, as shown in column 3. This finding diverges from Lalive and Zweimüller (2009), who observed that mother's wage penalties still persist approximately 84 months after childbirth. Our results imply that one-child mothers were somewhat insulated from wage penalties due to the one-child policy. This insulation could be attributed to employers have lower expectations for one-child mothers to have more children in the future during the one-child policy period. In contrast, the high wage penalty for the two-child mothers primarily attributed to the fact that having two children was illegal at the time of the one-child policy period, and to the changes in human capital associated with raising multiple children.

After the implementation of the two-child policy, there is a notable divergence in the motherhood wage penalty: it increases for one-child mothers while decreases for two-child mothers, as detailed in column 3. Specifically, the motherhood wage penalty for one-child mothers significantly increases by approximately 0.09 log points ($p<0.01$) following the two-child policy. This trend corroborates our hypothesis that the policy possibly sends a signal that one-child mothers may have a second child anytime. Consequently, employers might anticipate additional costs related to maternity leave and reduced productivity following childbirth. As these anticipated costs mount, profit-motivated employers make rational predictions and exhibit statistical discrimination against one-child mothers, leading to lower wages for these women. This shifting labor market landscape may inadvertently deter non-mothers from undertaking the transition to motherhood, thereby contributing to a decline in the one-child fertility (see Figure 2 and Appendix A1.5).

In contrast, the wage penalty for two-child mothers significantly reduces by about 0.09 log points ($p<0.01$) after the two-child policy. Since it was the first time China recognized the legality of having two children, employers became less likely to taste-based discriminate against two-child mothers who were considered rule breakers during the one-child policy period, leading to a reduction in the wage penalty. As a result, one-child mothers with a strong inclination toward larger families are more likely to have a second child after the policy (see Figure 2 and Appendix A1.5). Nonetheless, the surge in the number of two-child mothers can be attributed to a dual-factor incentive: firstly, the amplified wage penalty for one-child mothers acts as a deterrent against remaining with a single child, and secondly, the diminished wage penalty for two-child mothers encourages one-child mothers to expand their families further.

Table 1.2: The effect FR Policy on the Anticipatory Motherhood Wage Penalty

Log Hourly Wage	Person	W & F	Fixed	PSM	Exclude
	(1)	(2)	(3)	(4)	(5)
	Baseline motherhood wage penalty (during the one-child policy), conditional on number of children				
One child	-0.02	-0.03	-0.02	-0.02	-0.02
	(0.027)	(0.028)	(0.026)	(0.027)	(0.027)
Two children	-0.24***	-0.23***	-0.18***	-0.18***	-0.18***
	(0.035)	(0.035)	(0.034)	(0.034)	(0.034)
	Interaction of motherhood wage penalty with two-child policy				
One child × Policy	-0.08***	-0.10***	-0.09***	-0.08***	-0.09***
	(0.029)	(0.029)	(0.027)	(0.027)	(0.028)
Two children × Policy	0.10***	0.08**	0.09***	0.10***	0.08**
	(0.036)	(0.036)	(0.034)	(0.034)	(0.034)
	Pre-trend test (2010 vs. 2012)				
One child × 2010	-0.07	-0.02	-0.04	-0.03	-0.03
	(0.046)	(0.046)	(0.043)	(0.043)	(0.043)
Two children × 2010	-0.02	0.05	0.01	0.01	0.01
	(0.060)	(0.059)	(0.056)	(0.056)	(0.056)
N	10,399	10,399	10,399	10,285	9,860
R^2	0.3069	0.3198	0.4053	0.4038	0.4020
Year FE	✓	✓	✓	✓	✓
Age & Edu	✓	✓	✓	✓	✓
Work & Family	-	✓	✓	✓	✓
Province & Industry FE	-	-	✓	✓	✓
Matched	-	-	-	99%	99%

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. In DiD framework, we use non-mothers as the control group. The policy effect on the hourly wage is captured by the interaction terms. *P* represents the two-child policy in 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in column (5). To examine pre-policy trends, we employ an event study approach, omitting the year 2012 to serve as the reference. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

As we discussed before, there are four main considers. Firstly, within the DiD analytical framework, the accuracy of causal inference critically depends on creating equivalent comparison groups. To facilitate a balanced and precise analysis, we employ PSM with nearest neighbor matching. As demonstrated in column 4, the coefficients of motherhood wage penalty for one-child and two-child mothers, both before and after the policy's implementation, align closely with those found in model (3). This analysis confirms that the wage penalties for one-child and two-child mothers, both before and after the policy's implementation, are robust. Secondly, some women may bear an additional child during the two-child policy period, attributing any increase in the wage penalty to this child effect. To isolate the child effect, we only concentrate on a subset group of women who did not have an additional child during the two-child policy period. Specifically, we observe the wage penalty coefficient of one-child and two-child mothers increase by 0.09 log points ($p<0.01$) and reduce by 0.08 log points ($p<0.05$) after two-child policy, respectively, in this subset group. Given that these coefficients mirror those found in models (3) and (4), we reaffirm the robustness of our findings on the wage penalties for one-child and two-child mothers.

Thirdly, the validity of causal inference in the DiD approach also relies on the assumption of parallel trends. Panel of pre-trend test in Table 2, presenting empirical evidence crucial for evaluating this assumption and conducting a parallel trend test. Specifically, for the period leading up to the policy implementation, the year 2010, compared to the reference year of 2012, the analysis indicates a wage penalty of -0.03 log points for one-child mothers and 0.01 log points for two-child mothers in both including and excluding groups (see columns 4 and 5). Given the standard errors, 0.043 for one-child mothers with and 0.056 for two-child mothers, these variations are not statistically significant. These patterns, particularly the pre-policy observations for both groups, support the parallel trends assumption.

Fourthly, the consideration that non-mothers might be impacted by the policy through employers' anticipatory actions could potentially bias the DiD analysis. To strengthen the robustness of our findings, we delineate "only child" non-mothers and non "only child" non-mothers as the treatment and control groups, respectively. Our analysis is concentrated on comparing the periods immediately before and after the policy implementation (i.e., 2012 and 2014). According to the results detailed in Appendix A1.6, the coefficient of the treatment effect is 0.027 log points, and with a standard error of 0.173, this effect is statistically insignificant. This outcome suggests that the policy does not significantly impact non-mothers, reaffirming that the observed changes in our baseline model are primarily attributable to the effects on mothers. In general, this result reinforces the initial assumption that non-mothers are not impacted by the two-child policy is robust.

Fifthly, the DiD model may be affected by confounding events. To address this concern, we perform a permutation test. As illustrated in Appendix A1.7, the placebo DiD estimates cluster around zero and follow a normal distribution. Crucially, our primary DiD estimate shown in column 3 of Table 2 (represented by the vertical solid line in Appendix A1.5) significantly deviates from these placebo estimates, which lends additional support to the robustness of our findings.

B. The effect of policy on Employment Probability and Self-selection

Furthermore, one of the issues arises from self-selection into employment (more detail, refers to Section 5). To rectify this self-selection, we employ the Heckman correction, with "marital status" as the exclusion restriction. As seen in column 1 in Table 5, the likelihood of employment among one-child mothers and two-child mothers is reduced by about 0.03 ($p>0.1$) and 0.47 ($p<0.01$) on the Probit scale compared to the non-mothers before the policy. Following the two-child policy, the employment probability for one-child mothers significantly declines by approximately 0.14 on the Probit scale ($p<0.05$). Conversely, for two-child mothers, the policy brings about a significant reduction in the employment penalty, by about 0.19 on the Probit scale ($p<0.01$). Interestingly, employment and wage patterns tend to move in tandem; a policy influencing wages directly impacts employment patterns, as wages and the decision to engage in the labor market are intrinsically linked (Goldin and Lawrence, 2008).

Given the significant impact of the policy on employment patterns, overlooking employment self-selection could introduce potential bias into the wage regression. The outcomes presented in column 2 reveal that, after correcting for self-selection, the motherhood wage penalty for mothers with one or two children before the policy remains largely unchanged. Crucially, the insignificance of the inverse Mills ratio suggests the absence of a self-selection problem in our wage analysis. The robustness of our results hinges on the validity of our instrument variable, “marital status”. To substantiate its validity, we carry out tests aimed at satisfying two principal assumptions: Relevance and Exogeneity. The Relevance assumption is confirmed through a strong negative correlation between marital status and labor market participation, evidenced by a coefficient of -0.70 on the Probit scale ($p<0.01$), as shown in column 1. Although empirically validating Exogeneity is challenging, our reduced-form equation indicates that marital status is not significantly correlated with log hourly wage, thereby lending some credence to the instrument’s validity. A Wald test, comparing the constant terms in different models, yields a p-value of 0.4792, suggesting that the models are statistically equivalent. This analysis further solidifies the conclusion that our wage regression is not affected by self-selection issues.

Table 1.3: The effect FR Policy on the labor market outcomes

	Heckman DiD		Reduce Form
	Employment (F)	Wage (S)	Wage
	(1)	(2)	(3)
	Baseline motherhood penalty (during the one-child policy), conditional on number of children		
One Child	-0.03	-0.02	-0.02
	(0.053)	(0.028)	(0.029)
Two Children	-0.47***	-0.16***	-0.17***
	(0.060)	(0.041)	(0.036)
	Interaction of motherhood penalty with two-child policy		
One Child × Policy	-0.14**	-0.08***	-0.09***
	(0.057)	(0.028)	(0.028)
Two Children × Policy	0.19***	0.07**	0.08**
	(0.062)	(0.036)	(0.034)
	Relevance and Exogeneity Test		
Married	-0.70***	-	-0.01
	(0.044)	-	(0.021)
imr	-	-0.04	-
	-	(0.052)	-
N	16,196		9,860
R^2	0.4069		0.4068
Year & Prov & Ind FE	✓		✓
Person & Work & Fam	✓		✓
Wald Test	0.4792		

NOTE.—Estimates from regressions on employment status for all women and the log of the hourly wage for the working women. Employment status is modelled using a Probit model. In DiD framework, the control group is non-mothers. *Policy* represents the two-child policy in 2014. The complete set of controls described in equation (3) and (4) is included but not reported. We exclude the women opt to additional child after the policy in all columns. Standard errors are presented in parentheses below the point estimates. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

C. Robust check: Birth cohort

Above results rely on the assumption that non-mothers are unaffected by the two-child policy, but the employer expectations that non-mothers might bear a second child in the future, leading to introduce bias into the DiD analysis. To address this concern, we further identify the “control group” that women from households where both partners were born before or in 1982 as they are not the primary targets of the selective two-child policy of 2014. Additionally, we incorporate this treatment dummy in the analysis of the 2016 universal two-child policy to perform a placebo test (for more detail, see Section 5). Notably, we only use the triple DiD to test the one-child mothers as women who previously contravened the law to have a second child continue to face lifelong repercussions for their actions in China, even post-policy change.

$$
\begin{aligned}
lwage_{it} = \beta D_{it}^{(1)} + \lambda tr_{it} + \tau S_t tr_{it} + \delta U_t tr_{it} + \vartheta D_{it}^{(1)} tr_{it} + \gamma D_{it}^{(1)} tr_{it} S_t \\
+ \rho D_{it}^{(1)} tr_{it} U_t + X_{it}\eta + \varphi_t + \gamma_o + \mu_p + \varepsilon_{it}
\end{aligned}
\tag{5}
$$

The results, as illustrated in column 1 in Table 4, the treatment effect we examine through the triple DiD coefficient specifically isolates the selective two-child policy's influence on wage penalty of one-child and two-child mothers, by focusing on its primary target group, "only child" families. Specifically, the triple DiD coefficient of $One \times Tr \times S$ indicates that the treatment effect leads to a significant reduction in the wage penalty by -0.11 log points ($p<0.01$). This significant outcome highlights the distinct influence of the selective two-child policy on reducing the wage penalty for one-child mothers among the targeted group. Remarkably, this coefficient magnitude is similar to the DiD coefficient that reported in Table 3, reinforcing the baseline results that one-child mothers are impacted by the two-child policy is robust. Moreover, we employ another triple DiD coefficient, $One \times Tr \times U$, as a placebo test. Given that the "universal two-child" policy potentially affects all cohorts, irrespective of whether births occurred before or after the one-child policy, we anticipate this coefficient to be close to zero and statistically insignificant. The findings in column 1 bolster our hypothesis: the treatment effect on the wage penalty for one-child mothers across both cohorts following the universal two-child policy is minimal, at 0.02 log points, with a standard error of (0.029), thereby indicating no significant difference. This outcome lends further credence to the robustness of our chosen treatment and control groups in examining the motherhood wage penalty under the selective two-child policy.

Table 1.4: The triple DiD effect of FR Policy on Anticipatory Motherhood Wage Penalty

	Triple DiD	PSM
	(1)	(2)
	Interaction of motherhood wage penalty with policy of selective (S) and universal (U) two-child policy	
One Child × Tr × S	-0.11***	-0.11***
	(0.000)	(0.001)
One Child × Tr × U	0.02	0.02
	(0.029)	(0.029)
N	6,247	6,228
R^2	0.3858	0.3857
Year & Prov & Ind FE	✓	✓
Person & Work & Fam	✓	✓
Matched		99%

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. The policy effect on the hourly wage is captured by the triple interaction terms. *S* represents the "selective two-child" policy in 2014, and *U* represents the "universal two-child" policy in 2016. The term *Tr* refers to whether women from households where both partners were born before or in 1982. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in all columns. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

1.6 Mechanism

The earlier sections confirmed that the two-child policy can escalate the wage penalty for one-child mothers, while mitigating the wage penalty for two-child mothers. This wage penalty fluctuation may be traced back to two principal channels: human capital (Becker, 1985; Mincer, 1989) and job discrimination (Budig and England, 2001; Gough and Noonan, 2013).[29]

A. Statistical discrimination

Job discrimination can be divided into statistical and tasted-based discrimination. In

[29] The two-child policy has limited effect on the motherhood wage penalty through compensating differential channel. Women find it challenging to switch between sectors in China, and within each sector, family-friendly policies tend to be consistent (Zhou and Xie, 2019). Also, the Bivariate Probit regression test results indicates the p-value of correlation coefficient is 0.1875, suggesting no sector selection problem in our findings.

this section, we delve into the hypothesis that the post-policy increase in the wage penalty for one-child mothers is driven by statistical discrimination. To confirm this argument, we analyse two heterogeneity groups, namely age and sector. On the one hand, if the policy's effect is rooted in statistical discrimination, its impact might differ based on the ages of both the mother and her child. This notion relies on the belief that older mothers are less inclined to have another child, implying that younger mothers might face more pronounced effects of statistical discrimination. To explore this hypothesis, identifying an age threshold for mothers becomes essential. Below this threshold, employers may statistically discriminate against younger mothers, assuming a higher likelihood of them having a second child following the policy. Beyond this threshold, however, the likelihood of engaging in statistical discrimination diminishes, as employers may perceive these mothers as less likely to have additional childbirth post-policy. Upon determining the threshold for mothers' ages, the next step involves examining the child's age to gauge the extent of statistical discrimination effects. For this purpose, we categorize children's ages into three groups reflective of stages in the Chinese educational system: 0-3 years (before kindergarten), 4-7 years (kindergarten), and 8+ years (primary school). If employers exhibit statistical discrimination against one-child mothers, we anticipate the 4-7 age group to experience the highest wage penalty post-policy, as the typical age gap between the first and second child in China falls within this range. On the other hand, we conduct another analysis through a comparative analysis of the public and private sectors, informed by China's one-child policy which historically enforced more stricter regulations on women in the public sector. Consequently, our result reveals that women employed in the public sector were half as likely to have a second child compared to those in the private sector, as detailed in Appendix A1.9. This discrepancy underscores a higher expectation among employers in the public sector regarding the likelihood of women having two children after the policy, indicating that one-child mothers in the public sector may face heightened statistical discrimination post-policy.[30]

[30] Moreover, due to the policy may affect the women wage in the different sectors, women may self-select to work in the different sector to mitigate wage penalty. But unlike the European public sector, it is difficult for workers to

The age heterogeneity findings presented in Table 5, consistent with the baseline results, indicate that during the one-child policy period, the motherhood wage penalty was not evident for one-child mothers. However, after the two-child policy, younger one-child mothers (aged 36 and below) experience more pronounced statistical discrimination, leading to a wage reduction of approximately 0.08 log points ($p<0.01$). In contrast, older one-child mothers (aged over 36) do not face any statistical discrimination, even their wages increase by about 0.03 log points although it is insignificant (see columns 1 and 5). As previously outlined, if statistical discrimination is present, it would be most noticeable within the child's age group of 4-7 for younger one-child mothers. Our analysis supports this hypothesis, revealing that within the 4-7 child's age group, younger one-child mothers face the highest level of statistical discrimination, with their wage penalty increasing by 0.11 log points ($p<0.01$) post-policy. For children aged 0-3, the discrimination is less severe, with a decrease in wage penalty of 0.08 log points ($p<0.1$) post-policy, as employers likely do not anticipate mothers to have a second child in the immediate future. Finally, for children aged above 8 years, the wage penalty change post-policy is minimal and statistically insignificant, decreasing by 0.03 log points ($p>0.1$), due to the rare occurrence of having a second child with such an age gap in China prior to the policy.

Table 1.5: The effect of FR Policy on Anticipatory Motherhood Wage Penalty through Statistical Discrimination

	Women's age: Age <= 36				Age > 36
Child's age	Full	0-3	4-7	8+	Full
	(1)	(2)	(3)	(4)	(5)
	Baseline motherhood wage penalty (during the one-child policy)				
One Child	0.02	-0.03	0.05	-0.04	-0.03
	(0.026)	(0.037)	(0.034)	(0.038)	(0.093)
	Interaction of motherhood wage penalty with two-child policy				
One Child × Policy	-0.08***	-0.08*	-0.11***	-0.03	0.03
	(0.032)	(0.048)	(0.043)	(0.048)	(0.130)
N	5,496	3,790	3,987	3,807	2,709
R^2	0.4052	0.4097	0.3997	0.4021	0.4474

freely move between the two sectors in China (Zhou and Xie, 2019). Also, the Bivariate Probit regression test results indicates the p-value of correlation coefficient is 0.1875, suggesting no sector selection problem in our findings.

Year & Prov & Ind FE	✓	✓	✓	✓	✓
Person & Work & Fam	✓	✓	✓	✓	✓

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. In DiD framework, the control group is non-mothers. *Policy* represents the two-child policy in 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women who have two children in all columns. Standard errors are presented in parentheses below the point estimates. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

The findings on sector heterogeneity, detailed in Table 6, reveal that during the one-child policy period, the wage penalty was not significant for one-child mothers within the public sector; a slight wage premium was observed, although it was not statistically significant, with a coefficient of 0.06 log points ($p>0.1$). This suggests that one-child mothers in the public sector were somewhat shielded from the wage penalties due to the stringent enforcement of the one-child policy. In contrast, in the private sector, where adherence to the one-child policy was less stringent and employers expect a higher likelihood of one-child mothers having a second child, lower wages were offered, evidenced by a coefficient of -0.08 log points ($p<0.05$). However, after the two-child policy, the protective effect of the one-child policy on wage penalties for one-child mothers in the public sector has diminished. As a result, the wage penalty for these mothers in the public sector saw a significant rise, increasing by 0.12 log points ($p<0.05$) post-policy. In the private sector, where there was already an anticipation of mothers having a second child during the one-child policy, the change in the wage penalty for one-child mothers post-two-child policy was not significant, with a decrease of -0.04 log points ($p>0.1$).

Table 1.6: The effect of FR Policy on the Anticipatory Motherhood Wage Penalty by Sector

	Private	Public	Diff.
	(1)	(2)	(3)
	Baseline motherhood wage penalty (during the one-child policy)		
One child	-0.08**	0.06	-0.14***
	(0.032)	(0.051)	-
	Interaction of motherhood wage penalty with two-child policy		
One child × Policy	-0.04	-0.12**	0.08***
	(0.032)	(0.052)	-
N	5,601	2,319	-
R^2	0.4182	0.3665	-
Year & Prov & Ind FE	✓	✓	-
Person & Work & Fam	✓	✓	-

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. In DiD framework, the control group is non-mothers. *Policy* represents the two-child policy in 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women who have two children in all columns. Column 3 reports the coefficient differences between the private and public sectors by employing Bootstrap and Permutation tests to assess the differences in coefficients between the two groups, conducted 500 times. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

B. Tasted-based discrimination

To investigate the reduction in wage penalty for two-child mothers as a result of taste-based discrimination, we consider the persistence of a stigma effect. Specifically, we consider a scenario where, within the timeframe of the two-child policy, women who had a second child under the one-child policy was still in effect may not experience as significant a reduction in the motherhood wage penalty as those who had their second child after the two-child policy was enacted. To delve into this stigma effect associated with taste-based discrimination, our analysis is confined to samples in 2016 and 2018 (two-child policy period). However, it is necessary distinguish between women who bore their second child during the one-child or two-child policy periods. This requires the creation of a dummy variable to represent these distinct groups. Additionally, considering the different age distributions of children born within and outside the two-child policy period, controlling for the child's age becomes essential to our analysis.

Furthermore, we include one-child mothers as part of our robustness check. Given that having one child was allowed both before and after the two-child policy, we do not expect to observe significant differences between these groups.

The results displayed in Table 7 corroborate our hypothesis regarding the persistence of a stigma effect among two-child mothers, reflecting taste-based discrimination. As illustrated in column 3, the log hourly wage for two-child mothers who had their second child illegally under the one-child policy is lower by 0.11 log points ($p<0.05$) compared to those who had their second child legally after the two-child policy was enacted. This finding remains robust when employing PSM with nearest neighbor matching. Additionally, our analysis does not reveal any significant taste-based discrimination against one-child mothers, as the wage comparison between these groups shows no significant difference, support our argument the wage penalty for one-child mothers is mainly driven by statistical discrimination.

Table 1.7: The effect of FR Policy on Anticipatory Motherhood Wage Penalty through tasted-based discrimination

	One-child mother		Two-child mother	
	(1)	(2)	(3)	(4)
one- vs. two-child policy	0.03	0.03	-0.11**	-0.13**
(illegal vs. legal)	(0.046)	(0.049)	(0.048)	(0.052)
N	1,458	1,309	953	866
R^2	0.3421	0.3423	0.2575	0.2760
Year & Prov & Ind FE	✓	✓	✓	✓
Person & Work & Fam	✓	✓	✓	✓
Child’s age	✓	✓	✓	✓

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. The term “*One- vs. two-child policy*” refers to the dummy variable whether the child born under the one-child policy or two-child policy period. The complete set of controls described in equation (1) is included but not reported. Columns 2 and 4 include the PSM method with the nearest matching, given the person controls. Standard errors are presented in parentheses below the point estimates. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

C. Human capital

From the perspective of human capital theory (Becker, 1985; Mincer, 1989), the motherhood wage penalty could experience indirect shifts after relaxation of OCP. This can be attributed to the fact that some one-child mothers who desire to have a second child might reduce their working hours or educational level to prepare for the future child after the two-child policy. Such a reduction in work engagement and educational activities could slow down their rate of human capital accumulation, which could, in turn, lead to a decrease in wages (Goldin and Lawrence, 2008).

The hypothesis is that if the wage variations were genuinely rooted in shifts through human capital channel, then we would expect to observe significant changes in these two variables among one-child and two-child mothers after the policy. Since the CFPS employs a scale ranging from 1 to 8 to assess educational attainment. However, our analysis reveals that the bulk of the data is skewed, with a concentration in the 1-3 range; only 0.75% of the respondents fall within the 7-8 range. To mitigate these limitations, we create a binary variable, classifying individuals into categories of either 'high' or 'low' educational attainment. We use high school as the cutoff, as it follows the 9-year free and compulsory education policy in China, which targets children up to age 15.[31,32] To isolate the causal impact of the policy on human capital variables, it is crucial to control for log monthly wage, as the change of wage could directly influence education level and working hours.[33]

The findings, as detailed in column 1 of Table 6, reveal that the two-child policy does not significantly affect the education levels of one-child and two-child mothers. Interestingly, after the policy, the working hours for one-child mothers significantly increase by 0.04 log points ($p<0.05$) in comparison to non-mothers, as shown in column 2. However, as evidenced by our previous findings in Table 2 and Appendix A1.10, both log hourly and monthly wages decrease for one-child mothers after the policy compared to the non-mothers. Logically, an increase in working hours post-policy would suggest a corresponding rise in monthly wages. However, the observed decrease in monthly wages post-policy suggests that mothers with one child might be increasing their working hours in an effort to offset this wage reduction. In general, we can conclude

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31 In Appendix A1.11, we explore the robustness of our findings by using different education levels as the cut-off. However, the results almost remain consistent with our initial observations.

32 Our analysis reveals that approximately 38.2% of employed women in our sample fall into the 'high education level' category.

33 Given that the hourly wage is derived by dividing the monthly wage by the number of working hours, including hourly wage as a control variable introduces an endogeneity issue due to its direct calculation from the dependent variable.

that the change in the wage penalty of one-child and two-child mothers post-policy is unlikely to be primarily driven by human capital channels. Were the changes driven by human capital channels, we would expect to see a reduction in both educational levels and working hours.

Table 1.8: The effect of FR Policy on the human capital variables

	Education	Work Hours
	(1)	(2)
One child × Policy	0.00	0.04**
	(0.018)	(0.018)
Two children × Policy	-0.01	-0.01
	(0.023)	(0.022)
N	10,399	10,399
R^2	0.4030	0.1228
Year & Prov & Ind FE	✓	✓
Person & Work & Fam	✓	✓

NOTE.—Estimates from regressions on working hours and education level by using the linear model. Working hours are represented using the logarithmic scale. Education level is divided into two categories: low and high education. In DiD framework, the control group is non-mothers. *Policy* represents the "two-child" policy after 2014. The complete set of controls described in equation (5) is included but not reported. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

1.7 Heterogeneity analysis

We have shown that the motherhood wage penalty for the one-child mothers increases, while the wage penalty for the two-child mothers is reduce after the two-child policy. An important question is whether these effects were heterogeneous across subpopulations. That is, the effects of the two-child policy on the wage penalty may differ by wage level (high vs. low and middle) and employment status (part-time vs. full-time).[34] Table 7 reveals that the effects of the two-child policy on the motherhood

[34] Since CFPS does not directly collect information regarding part-time and full-time employment, we generate a dummy variable for part-time and full-time employment, dependent on whether working hours are above or below 8 hours daily. Our findings indicate that roughly 26.3% of employed women with children are engaged in part-time jobs. This is consistent with the report on Statista "*Employment status of women who have children in China in 2018 and 2020*" indicating that around 24% of employed Chinese women with children were in part-time positions in both 2018 and 2020.

wage penalty do indeed differ by wage level and employment status. For mothers at high wage levels, the two-child policy does not lead to significant changes in the wage penalty for either one-child or two-child mothers, as shown by coefficients of 0.03 and 0.04 log points ($p>0.1$), respectively. Conversely, for mothers at low and middle wage levels, there is a significant increase in the wage penalty for one-child mothers, with a coefficient of -0.08 log points ($p<0.01$) post-policy, and a reduction in the wage penalty for two-child mothers, indicated by a coefficient of 0.05 log points ($p<0.05$) post-policy. These results corroborate our hypothesis that the post-policy variation in wage penalties is primarily through the discrimination channel, with workers at higher wage levels being less susceptible to discrimination in the labor market (Arulampalam et al., 2007).

Nonetheless, for one-child mothers in part-time employment, the implementation of the two-child policy does not result in a significant change in the wage penalty, as shown by a coefficient of 0.03 log points with a standard error of 0.071. In contrast, there is a significant increase in the wage penalty for one-child mothers in full-time employment, indicated by a coefficient of -0.10 log points ($p<0.01$) post-policy. For two-child mothers in part-time employment, the two-child policy significantly reduces the wage penalty, with a coefficient of 0.38 log points ($p<0.01$). While for those in full-time employment, it leads to a slight increase but not significant, evidenced by a coefficient of 0.05 log points ($p>0.1$). In general, this heterogeneity analysis suggests that taste-based discrimination is more prevalent among part-time two-child mothers, whereas statistical discrimination is more likely to occur in full-time one-child mothers post-policy.

Table 1.9: The effect of FR policy on Anticipatory Motherhood Wage Penalty (Heterogeneity analysis)

	Hourly Wage		Employment Status	
	Low & Mid	High	Part-time	Full-time
	(1)	(2)	(3)	(4)
One child × Policy	-0.08***	0.03	0.03	-0.10***
	(0.022)	(0.047)	(0.071)	(0.026)
Two children × Policy	0.05**	0.04	0.38***	0.05
	(0.026)	(0.081)	(0.098)	(0.031)
N	6,954	2,904	2,540	7,319
R^2	0.3665	0.0625	0.1664	0.4578
Year & Prov & Ind FE	✓	✓	✓	✓
Person & Work & Fam	✓	✓	✓	✓

NOTE.—Estimates from regressions on the log of the hourly wage for the working women. In DiD framework, the control group is non-mothers. *Policy* represents the "two-child" policy in 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in all columns. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

1.8 Conclusions

The focus of this paper is the exploration the impact of fertility relaxation policy on the anticipatory motherhood wage penalty. By drawing from China's policy transition from a one-child to a two-child norm, we find that after the fertility relaxation, wage penalty for the one-child mothers is significantly increase 0.09 log points, while for the two-child mothers is significantly reduce 0.08 log points resulting. This change in the motherhood wage penalty post-policy is primarily driven by job discrimination rather than changes in human capital. Furthermore, our analysis delves into the mechanisms contributing to the post-policy rise in wage penalty for one-child mothers through statistical discrimination and the decrease in wage penalty for two-child mothers due to taste-based discrimination.

A direct theoretical implication of our study is the expansion of the motherhood wage penalty literature to include channels that have not been fully explored. Despite controlling for observable factors like labor market experience or tenure, a substantial motherhood wage penalty persists, and while admittedly smaller, a wage gap remains. We explore that this wage gap originates from anticipatory effects prior to birth, mediated through mechanisms of statistical and taste-based discrimination. Additionally, a crucial policy insight from our research is that fertility relaxation policies can influence fertility rates in divergent manners. For one-child mother possess a strong desire to expand their family, such policy facilitate the possibility of having a second child. Conversely, the policy might increase in the anticipatory motherhood wage penalty within the labor market via the statistical discrimination channel, leading to potential feedback effects on fertility. This, in turns, could deter non-mother from undertaking the become mothers post-policy.

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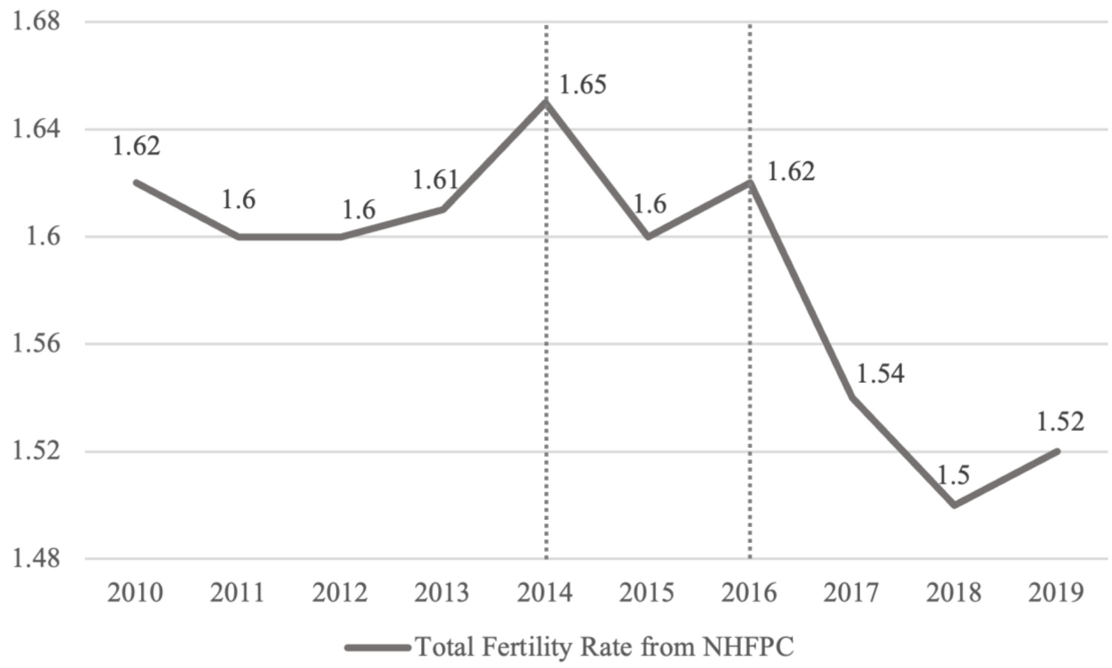
1.10 Appendix

Table 1.A: Evolution of China's family planning policy

Time	Issue Unit	File Name	Content
1980	Party Central Committee	*"An Open Letter to all Communist and CYL Members on The Issue of Controlling China's Population Growth"*	Couples are encouraged to have only one child
1982	The 12th National People's Congress (NPC)	*"Constitution of People's Republic of China(1982)"*	Family planning was written into the Constitution
1991	Central Committee of the Communist Party of China、State Council	*"Decision on Strengthening Family Planning Work and Strictly Controlling Population Growth"*	Implement the current family planning policy and strictly control population growth
2013	The 18th Central Committee of the Communist Party of China	*"Decision of the CPC Central Committee on Major Issues concerning Comprehensively Deepening Reform"*	Couples can have two children if one of them is an only child
2015	The Standing Committee of National People's Congress	*"Amendments to the Population and Family Planning Law"*	Implement the universal two-child policy

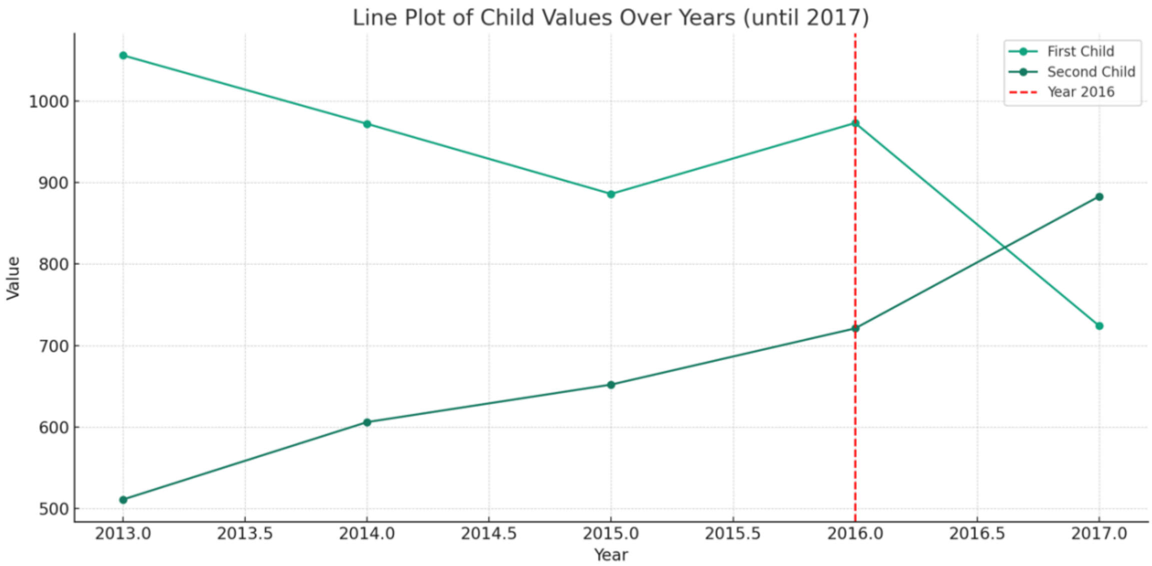
CYL: Communist Youth League

FIG. 1.B — Total fertility rate over time.



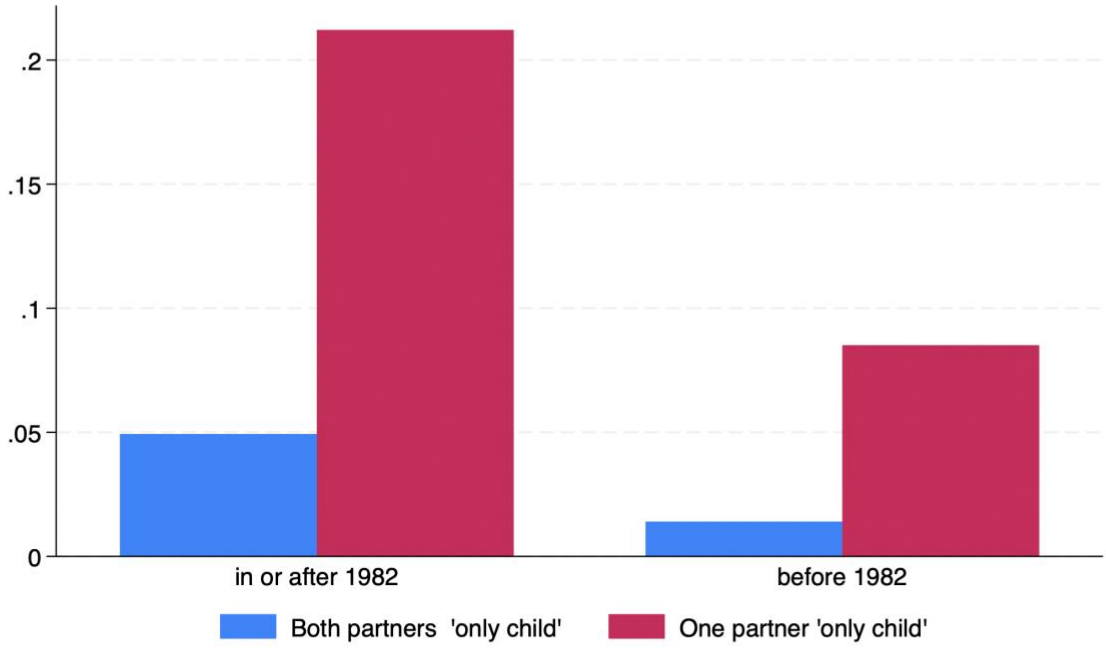
Source: National Health and Family Planning Commission of the People's Republic of China (NHFPC)

FIG. 1.C — Total number of newborns over time



Source: China Statistical Yearbook, 2020

FIG. 1.D — Proportion of Partners Who Are 'Only Children' in 2010



E. The effect of extended FA on fertility behavior

Our primary focus is understanding how extended FA impacts fertility behavior. As discussed in section II, the extended FA might spur high-order births while decreasing low-order births. To empirically test this in our dataset, we use sequential Logistic regression to assess the likelihood of women having one or two children under both policies:

$$Log\left(\frac{P(N_{it}=j|N_{it-1}\geq j-1)}{P(N_{it-1}=j-1)}\right)=\gamma_j P_t+X_{it}\delta+\varepsilon_{it} \tag{A.1}$$

where N_{it} represents the number of children for woman i at time t (with $j \in \{1,2\}$).

Table A.1 illustrates the effect of both "selective" and "universal two-child" polices on the fertility behavior of women, focusing on the proportion of one-child and two-child mothers compared to the non-mothers. Our initial step involves estimating a regression that includes a range of individual characteristics. As evident in columns 1 and 2, the "two-child" policy appear to influence these proportion, with an observable decrease in the probability of women choosing to have their first child. Concurrently, there is an increase probability of one-child mothers deciding to have a second, as shown in columns 3 and 4. In particular, following the introduction of the "universal two-child" policy, the proportion of one-child mothers significantly reduce by around 0.04 log points compared to the non-mothers. However, following "selective two-child" policy, we noted a modest 0.03 log points increase the proportion of two-child mothers compared to the non-mothers, and this effect size increase further to 0.12 log points after "universal two-child" policy. In general, the model suggests that the "two-child" policy appeared to influence the fertility decisions of women in two significant ways: fewer non-mothers transitioned into one-child mothers, and a greater number of one-child mothers transitioned into two-child mothers. This is consistent with the data from the China Statistical Yearbook (2020), which documented a decline in first births and an increase in second births (see Figure 2).

Table 1.E: The effect extended FA on the fertility decision

	Zero → One	One → Two
	(1)	(2)
Policy	-0.03***	0.09***
	(0.004)	(0.007)
N	17,361	
Individual controls	✓	✓
Family controls	✓	✓

NOTE.—Estimates are from sequential logistic regressions on probability of women having 0, 1 and 2 children, with coefficients reported in the marginal level. We use the code "seqlogit" in Stata. S represents the "selective two-child" policy in 2014, and U represents the "universal two-child" policy in 2016. The complete set of controls described in equation (A.1) is included but not reported. Standard errors are presented in parentheses below the point estimates. $*/**/***$ denote statistical significance at the 10%/5%/1% level, respectively.

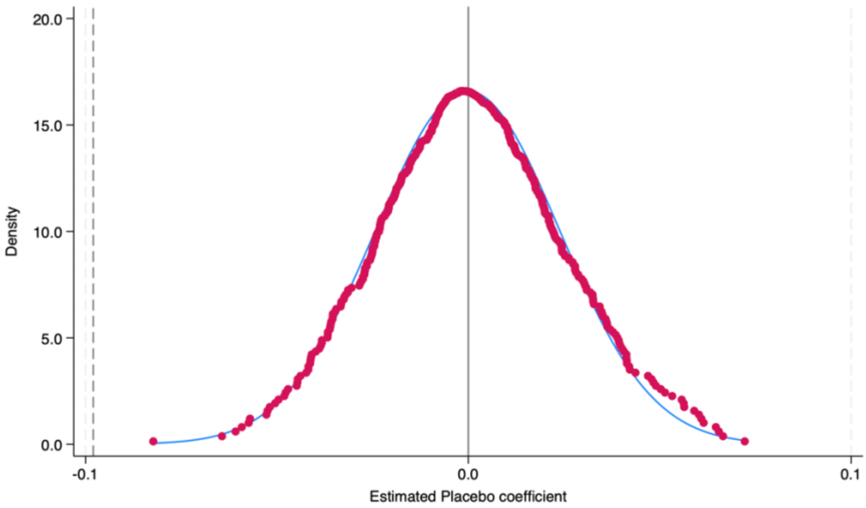
Table 1.F: The effect policy on the log monthly wage

Non-mothers	Fixed	Industry	PSM
	(1)	(2)	(3)
Tr	-0.238	-0.313	-0.285
	(0.276)	(0.279)	(0.279)
Tr × S	0.035	0.035	0.027
	(0.171)	(0.173)	(0.173)
N	1,234	1,158	1,158
R^2	0.3144	0.3377	0.3134
Prov &Year & Age FE	✓	✓	✓
Person & Work & Fam	✓	✓	✓
Industry FE			✓
Matched		94%	94%

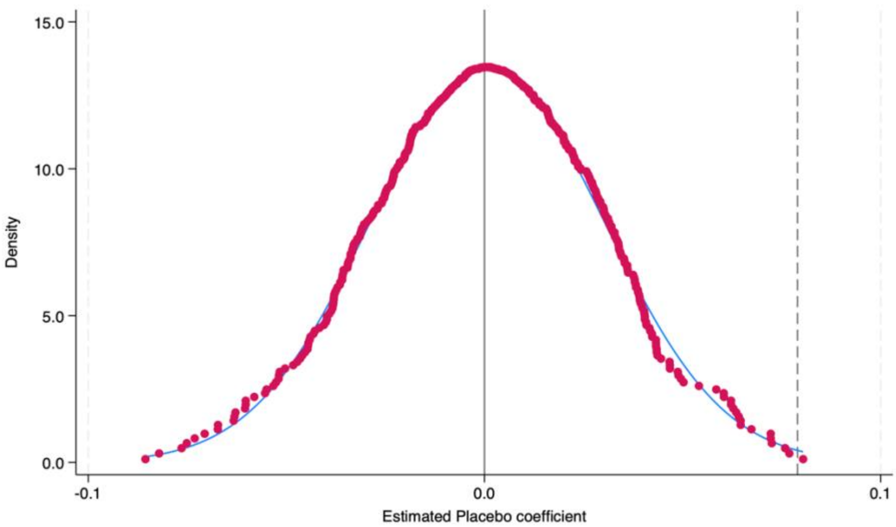
NOTE.—Estimates from regressions on the log of the monthly wage for the working women. In DiD framework, the control group is non-mothers. *S* represents the selective two-child policy in 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in all columns. We opted not to incorporate industry fixed effects to avoid overfitting the model, given the relatively small number of observations. A higher and negative coefficient for the *Tr* indicates that younger non-mothers earn lower wages compared to older one. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

FIG. 1.G — Permutation Test

(a)



(b)



Note: These figures present the permutation test by randomly assigning the one-child/two-child mothers and non-mothers and repeating the main analysis 500 times in panel (a)/(b). The sample period is from 2010 to 2018. The outcome is the log of hourly wage. The complete set of controls described in equation (1). This graph plots the distribution of the placebo DiD estimates. The vertical solid line is the true DiD estimate in column 3 of Table 2.

Table 1.H: Summary Statistics by sector

	All		Private		Public		
Variables	Mean	SD	Mean	SD	Mean	SD	Diff
Dependent							
Monthly Wage	2393	2170	2319	2112	2598	2310	-279***
Independent							
Num. child	.855	.704	.874	.735	.801	.609	0.073***
One child	.483	.5	.445	.497	.589	.492	-0.145***
Two children	.186	.389	.215	.411	.106	.308	0.109***
Personal Chara.							
Age (year)	33	8.8	33	8.8	35	8.4	-1.817***
Education (1-9)	2.048	.425	1.978	.372	2.243	.494	-0.265***
Urban (Rural)	.686	.464	.641	.48	.811	.391	-0.170***
Working hours	219	71	230	72	187	55	42.855***
Public (private)	.266	.442	-	-	-	-	-
Firm size	541	4409	550	5000	514	2021	36.66
Family Chara.							
Savings (k)	7.199	4.63	7.065	4.593	7.568	4.712	-0.503***
House hours	59	28	59	28	58	28	1.004
Parents at home	.264	.441	.263	.44	.267	.442	-0.00400
Parents education	2.491	1.105	2.391	1.041	2.767	1.225	-0.376***
Heckman							
Employment rate	.613	.487	-	-	-	-	-
Married Status	.773	.419	.697	.459	.744	.437	-0.046***

NOTE.—Data is sourced from the CFPS spanning 2010 to 2018. 'Wage' denotes monthly income from the primary job. 'Promotion satisfaction' ranges from 0-5. 'Working hours' and 'House hours' are represented monthly. 'Parents at home' signifies the presence of the respondent's both parents in their household. 'Father/mother education' refers to the educational attainment of the respondent's parents. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

Table 1.I: The effect of FR policy on the log monthly wage

Log Monthly Wage	Unadjusted	Person	W & F	Fixed	PSM
	(1)	(2)	(3)	(4)	(5)
			Main effect		
One	-0.02	-0.03	-0.05**	-0.04*	-0.04*
	(0.018)	(0.022)	(0.022)	(0.021)	(0.021)
Two	-0.32***	-0.18***	-0.19***	-0.14***	-0.14***
	(0.024)	(0.028)	(0.028)	(0.027)	(0.027)
			Policy effect		
One × S	-0.04	-0.03	-0.05**	-0.05**	-0.04*
	(0.025)	(0.023)	(0.023)	(0.021)	(0.022)
Two × U	0.11***	0.11***	0.09***	0.08***	0.09***
	(0.031)	(0.029)	(0.028)	(0.027)	(0.027)
N	9,860	9,860	9,860	9,860	9,731
R^2	0.2185	0.3228	0.3447	0.4383	0.4372
Year FE	✓	✓	✓	✓	✓
Age & Edu control	-	✓	✓	✓	✓
Work & Family control	-	-	✓	✓	✓
Province & Industry FE	-	-	-	✓	✓
Matched	-	-	-	-	98%

NOTE.—Estimates from regressions on the log of the monthly wage for the working women. In DiD framework, the control group is non-mothers. *S* represents the "selective two-child" policy in 2014, and *U* represents the "universal two-child" policy in 2016. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in all columns. Standard errors are presented in parentheses below the point estimates. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

Table 1.J: The effect FR policy on the different category educational level

	Junior	Undergraduate
	(1)	(2)
One × Policy	0.03^{**}	0.01^{*}
	(0.013)	(0.004)
Two × Policy	-0.01	-0.00
	(0.016)	(0.005)
N	10,399	10,399
R^2	0.2983	0.0463
Year FE	✓	✓
Age & Edu & Urban	✓	✓
Work & Family	✓	✓
Province & Industry FE	✓	✓

NOTE.—Estimates from regressions on the education level for the working women. In DiD framework, the control group is non-mothers. *Policy* represents the "two-child" policy after 2014. The complete set of controls described in equation (1) is included but not reported. We exclude the women opt to additional child after the policy in all columns. Standard errors are presented in parentheses below the point estimates. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

Chapter 2

How do different types of referrals affect inequality?

Abstract

This paper investigates the impact of strong- and weak-tie referrals on labor market outcomes, distinguishing between the information transmission and screening mechanisms. Using the SCE dataset, which provides detailed records on job seekers' use of referrals, we isolate the effects of these mechanisms. Our SSIV-Heckman estimates reveal that weak-tie referrals significantly enhance job-finding probability through the information transmission mechanism, while strong-tie referrals do not show similar effects. Additionally, weak-tie referrals improve matching quality and starting wages primarily through the screening mechanism, with these effects persisting over time, leading to higher current wages and wage growth due to better job matches. To further explore the screening mechanism, we introduce the concepts of screening ability and reputational cost. For low-noise signal job seekers, employee referrals (lower screening ability but higher reputational cost) result in improved job-finding probability, matching quality, and starting wages. In contrast, co-worker referrals (higher screening ability but lower reputational cost) benefit high-noise signal job seekers. These findings highlight the importance of both reputational cost and screening ability in shaping the labor market effects of referrals.

2.1. Introduction

Labor markets often face challenges of asymmetric information, resulting in search frictions and uncertainty in job matching. One potential solution to this problem lies in "job referral" networks, where individuals such as friends and colleagues recommend job seekers to potential employers (Neugart and Richiardi, 2012). As reported by Holzer (1988), more than 85% of workers in the U.S. rely on informal contacts when searching for jobs. Previous theoretical research suggests that referrals impact labor market outcomes through two main mechanisms, namely information transmission and screening. On the job seeker side, if information about job opportunities is passed from both formal and informal market, additionally using the informal market can directly enhance hiring probability, thereby indirectly increase wage outcomes (Calvó-Armengol and Zenou, 2005; Ioannides and Loury, 2006). On the employer side, employers may leverage referrals as a screening tool to mitigate the effects of asymmetric information, leading to the increase of wage outcomes (Montgomery, 1991; Ekinci, 2016; Galenianos, 2014; Casella and Hanaki, 2008; Horvath, 2014).

However, empirical studies on the impact of referrals on labor market outcomes present a nuanced picture, with findings indicating both positive and negative effects. This complexity can be attributed to the different types of referrals, such as strong and weak tie referrals.[35] Weak tie referrals, for instance, are often found to be more effective than strong tie. This is consistent with the notion that current employees have more accurate information about job requirements and the capabilities of the referred candidates. Several studies, including those by Bayer et al. (2008), Dustmann et al. (2016), and Hellerstein et al. (2011) use employee-employer administrative data to proxy the weak tie referral used finding that positive impact of weak tie referrals on labor market outcomes, such as better job matches, higher wages, and longer job tenure.

[35] A weak tie referral occurs when job opportunities or information are shared through a co-worker or professional acquaintance with whom interactions are infrequent or lack personal depth. In contrast, a strong tie referral arises from close relationships, such as those with friends or family.

Similar findings are reported in studies using referral survey data (Brown et al., 2016; Simon and Warner, 1992; Loury, 2006; Fernandez et al., 2000) and experimental data (Beaman and Magruder, 2012; Pallais and Sands, 2016).

In contrast, strong tie referrals, or referrals from family members, have been found in some studies to have less positive, or even negative, impacts on labor market outcomes. For instance, Mouw (2003) reported that while kinship referrals can increase the likelihood of job interviews, they do not necessarily lead to job offers. Carrillo-Tudela et al. (2023) found that workers hired through kinship referrals had lower wages compared to those hired through other channels, suggesting that kinship referrals may lead to poorer job matches. Furthermore, Bentolila et al. (2010) and Kramarz and Skans (2014), both focusing on youths, find a negative association between the use of referral and starting wages. Finally, Pellizzari (2010) found that the use of kinship referrals in job search can lead to lower wages, possibly because job seekers who rely on their kinship networks may end up in jobs that are not the best match for their skills and abilities.

Despite the varying effects of referral types, there is limited empirical research distinguishing between the information transmission and screening mechanisms to explore how strong- and weak-tie referrals impact labor market outcomes. This distinction is crucial, as theoretical models suggest that wage increases from referrals can be driven by either the information transmission or screening mechanism. However, using proxies for referrals often fails to isolate these mechanisms, as they do not directly capture whether a job seeker was referred or not. Furthermore, to the best of our knowledge, most existing research relying on survey data uses the variable "*learned about their current job through referral*" to explore referral effects, which may capture the compositional effects of referrals. Thus, in the first part of our paper, we examine how strong- and weak-tie referrals influence job-finding probability, matching quality, and starting wages through either the information transmission mechanisms or screening mechanisms. To investigate these effects, we use the unique Survey of Consumer Expectations (SCE) dataset, which provides detailed records on job seekers' use of referrals. Specifically, the SCE captures whether individuals found their job through a referral, learned about their current job through referral connections, or obtained their job directly through a referral. These three variables are crucial, as they allow us to isolate the information transmission mechanism from the screening mechanism. Moreover, recent studies by Moon (2023) differentiate between weak- and strong-tie referrals to explore how the screening mechanism operates, introducing concepts such as screening ability and reputational cost. Building on this work, we further divide weak-tie referrals into co-worker (internal) and employee (external) referrals. Co-worker referrals are characterized by higher screening ability but lower reputational cost, while employee referrals have the opposite attributes. The basic framework is that when employers receive a referral, they evaluate both the screening ability and its credibility. To test this, we define "noisy signals" based on the job seeker's education level. Job seekers with higher education are assumed to have "high-noisy signals", while those with lower education have "low-noisy signals" (Spence, 1973; Gibbons and Lawrence, 1991). For low-noisy signal job seekers, employers tend to trust employee referrals more due to their higher reputational cost. However, for high-noisy signal job seekers, employers may trust co-worker referrals more, despite their lower reputational cost, due to better screening ability.

A key concern in the empirical analysis of referral is the self-selection and endogeneity of network characteristics with respect to outcomes. According to Calvó-Armengol and Zenou (2005)'s model, the hiring probability depends both on formal and informal market.[36] If job seekers with using informal market are more likely to be hired, the wage regression analysis may be biased due to the analysis being conducted on a truncated sample. Additionally, individuals with better communication skills and social abilities are more likely to use informal referrals (Diaz, 2012; Cappellari and Tatsiramos, 2015). To address both self-selection and endogeneity issues, we employ both Heckman and shift-share instrument variable methods. On average, our SSIV estimate show that workers who use weak tie referrals are 14.7 percentage points more

[36] The formal job market includes structured and regulated channels such as job boards, company websites, and recruitment agencies, where hiring processes follow standardized procedures. In contrast, the informal job market relies on personal networks, such as referrals from friends, family, or acquaintances, where opportunities are shared through word-of-mouth rather than formalized systems.

likely to find a job, compared to those using formal channels, while the effect of strong tie referrals on job finding probability is negative, although this result is not statistically significant. These results confirm that referrals can increase job-finding probability through the information transmission mechanism, but only via weak-tie referrals, consistent with previous research by Brown et al. (2016), Simon and Warner (1992), Loury (2006), and Fernandez et al. (2000).

In the subsequent empirical analysis, we then estimate the effect of strong and weak ties referrals on matching quality and starting wage by distinguishing the information transition and screening mechanism. On average, our IV-Heckman estimates indicate that workers who use weak-tie referrals through the screening mechanism to obtain jobs experience an increase in subjective matching quality of 4.5%, compared to those who do not use referrals. These findings align with our framework, which posits that weak-tie referrers possess more screening ability about job seekers than strong-tie referrers. Interestingly, using a similar identification strategy but examining the information transition mechanism, our IV-Heckman estimates indicate that workers who learned about their jobs through weak tie connections do not experience a significant increase in matching quality. This evidence supports the previous theoretical model that the referral effect on the matching quality mainly depend on the screening mechanism (Montgomery, 1991; Casella and Hanaki, 2008; Horvath, 2014).

Moreover, if weak tie referrals improve the job search and matching process, it is reasonable to expect that they would also lead to higher labor income. However, the key question is whether this increase in starting wages is driven by the information transmission mechanism or the screening mechanism. Our IV-Heckman estimates reveal that job seekers who use weak tie referrals through the screening mechanism experience a significant increase in their real starting wage, by approximately 11.2%, compared to those who do not use referrals. In contrast, merely learning about a job through weak-tie connections does not result in a significant increase in starting wages. Additionally, unlike previous research that suggests referrals lead to lower wage growth over time (Dustmann et al., 2016; Brown et al., 2016; Simon and Warner, 1992) due to the gradual revelation of unobserved productivity, our findings indicate that using weak-tie referrals has a statistically significant and persistent effect on both current wages and wage growth, with increases of 8.6% and 6.9%, respectively. This is because referrals effectively screen a worker's productivity, leading to better matching quality, which in turn results in higher current wages and sustained wage growth over time.

Nonetheless, another main objective is to explore the benefits of employee referrals (lower screening, higher reputational cost) and co-worker referrals (higher screening, lower reputational cost) for job seekers with different levels of signal noise. Consistent with our framework, our IV-Heckman estimates show that employee referrals only benefit for the low-noise job seekers, with 22.8% increase job-finding probability and 6.3% in matching quality. In contrast, the co-worker referrals only benefit for the high-noise job seekers, with 12.3% increase job-finding probability and 2.7% in matching quality. According to learning theory, the wage premium also depends on these dynamics: low-noise signal job seekers gain from employee referrals, while high-noise signal job seekers benefit from co-worker referrals. Our IV-Heckman results support this, with employee referrals leading to a 25.7% wage increase for low-noise job seekers, and co-worker referrals resulting in a 22% wage increase for high-noise job seekers. These findings reinforce our hypothesis that the screening mechanism in the labor market is shaped by reputational cost and screening ability.

The structure of this paper is as follows. Section 2 comprehensively introduces the dataset and describes the essential dependent and independent variables. Section 3 introduces how the IV-Heckman model solves the endogeneity and self-selection issues, and Section 4 uses the model to present the results.

2.2. Theoretical framework

Our model builds on the work of Calvó-Armengol and Zenou (2005), Ioannides and Loury (2006), and Moon (2023). Workers and firms populate the economy. A free entry condition determines the measure of firms. There are two distinct stages in the model. In the first stage, workers form a referral. In the second stage, workers and firms search and produce in a frictional labour market. Further assume that that jobs are homogeneous and that firms are identical.

A. Screening ability and reputation cost

The formation of the referral network is modelled as a non-cooperative game with non-transferable utility. Suppose that a worker's productivity signal s_i according to the following function:

$$s_i = \theta_i + \varepsilon_i \tag{1}$$

where θ_i represents the true productivity, assumed to be normally distributed, with $\theta_i \sim N(\mu_\theta, \sigma_\theta^2)$; and ε_i is the white noise, assumed to be normally distributed, with $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. A job seeker using referrals can reduce the noisy signal of their unobservable productivity through the referrer's screening ability, leading to a reduction in noise to $\frac{\sigma_{\varepsilon,i}^2}{a}$. Reducing this noise improves matching quality, so the job seeker's expected productivity through the referral becomes:

$$y_{i,R} = E[\theta_i | s_{i,R}] = \mu_\theta + \frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_\varepsilon^2/a}(s - \mu_\theta) \tag{2}$$

For job seekers who find a job through the formal market, their expected productivity becomes $y_{i,F} = E[\theta_i | s_{i,F}] = \mu_\theta + \frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_\varepsilon^2}(s - \mu_\theta)$. In the general case, we can assume the noisy signal for employer is infinite, where $\sigma_\varepsilon^2 \to \infty$, the employer's expectation of $E[\theta_i | s_{i,F}]$ becomes μ_θ. In addition to screening ability, we incorporate the concept of reputational cost, similar to Moon (2023), to account for the credibility of the information provided by the referrer. The job seeker's expected productivity in the firm then becomes:

$$y_i = \gamma(r)E[\theta_i | s_{i,R}] + (1-\gamma(r))E[\theta_i | s_{i,F}] \quad (3)$$

where $\gamma(r)$ represents the weight (or probability) that the employer trusts the referral based on the higher reputational cost r.[37] Intuitively, if the employer trusts the referral, the job seeker's productivity is expected to improve due to better matching quality. However, if the employer does not trust the referral, they randomly assign the job seeker, and the expected productivity reverts to $E[\theta_i | s_{i,F}] = \mu_\theta$. However, this naïve model does not fully account for the actions of the referrer and employer. In Appendix F, we provide a detailed analysis using a signaling game model, illustrating how an increase in the reputational cost imposed by the employer can lead to a decrease in the referrer's investment in screening ability.

B. Searching and Matching Function

In each period, the hiring probability of job seeker i is given by the sum of two components: the probability of receiving a job offer from the formal market (v), the probability of receiving a referral. For clarity and ease of exposition, we omit subscripts i in the following. Specifically, that is expressed as:

$$h(v, R) = v + \pi R \quad (4)$$

where π refers to coefficient for the effect of referral usage R on the hiring probability. Moreover, there are u unemployed workers, and since each hiring probability is independent across individuals, the rate at which job matches occur in each period is just $uh(v, R)$. Consequently, the matching function can be expressed as:[38]

[37] Reputational cost is crucial for the referrer's strategy. If the reputation cost is low, the referrer may be incentivized to inflate the job seeker's productivity by sending an overly favorable referral message, especially if the referrer's payoff is tied to the outcome of the referral. In the situation of employer always believe referrer, firm may loss the profits (see Appendix F).

[38] Similarly, given by $\frac{dh}{ds} = (1-v)\frac{dR}{ds}$, as $\frac{dR}{ds} > 0$ and $(1-v) > 0$, it follows $\frac{dh}{ds} > 0$.

$$m(u, v, R) = u[v + \pi R] \tag{5}$$

Finally, from Equation (5), we can derive the following expression for the probability $f(s, u, v)$ for firms to fill a vacancy:

$$f(u, v, R) = \frac{m(u, v, R)}{v} = u\left[1 + \frac{1}{v}\pi R\right] \tag{6}$$

C. The employer's problem

In our economy, firms are identical and offer homogeneous jobs. Employed workers have the expected productivity equals $y > 0$, as indicated in Equation (2). The wage paid by firms to employed workers is denoted by w.[39] Unfilled positions generate no productive, and firm incur a search cost γ. Each period, there is a probability δ that a job is lost, and r is the discount factor. The job filling rate at the beginning of period t is $f(s, u_{t-1}, v_t)$. Let $I_{F,t}$ and $I_{V,t}$ represent the intertemporal profit of a filled job and a vacancy, respectively, at the beginning of period t. Then, the Bellman equations for these profits are as follows:

$$I_{F,t} = y - w + \frac{1}{1+r}\left[(1-\delta)I_{F,t+1} + \delta I_{V,t+1}\right] \tag{7}$$

$$\begin{aligned} I_{V,t} = -\gamma + \frac{1}{1+r}\Big[&\big(1 - f(u_{t-1}, v_t, R)\big)I_{V,t+1} \\ &+ f(u_{t-1}, v_t, R)\Big((1-\delta)I_{F,t+1} + \delta I_{V,t+1}\Big)\Big] \end{aligned} \tag{8}$$

[39] In the models proposed by Calvó-Armengol and Zenou (2005), as well as Ioannides and Loury (2006), workers undergo a probationary period of one period during which their productivity and wage are represented by y_0 and w_0, respectively, with the specific condition that $y_0 = w_0 = 0$.

At steady state, we have $I_{F,t} = I_{F,t+1} = I_F$, $I_{V,t} = I_{V,t+1} = I_V$, $u_{t-1} = u_t$ and $v_{t+1} = v_t$. Following Pissarides (2000), we assume that in equilibrium, the value to firms of posting an additional vacancy is zero. With $I_V = 0$ and Equation (8), we derive:

$$I_F = \frac{(y-w)(1+r)}{r+\delta} \tag{9}$$

Additionally, under the free-condition and using Equation (9), we have:

$$\frac{y-w}{r+\delta} = \frac{1}{1-\delta}\frac{\gamma}{f(u,v,R)} \tag{10}$$

D. The worker's problem

Let $I_{E,t}$ and $I_{U,t}$ denote the intertemporal gains of an employed and an unemployed worker, respectively, at the beginning of period t. When vacancies are posted at the beginning of t, with a hiring probability $h(s, u_{t-1}, v_t)$, the Bellman equations can be expressed as:

$$I_{E,t} = w + \frac{1}{1+r}\left[(1-\delta)I_{E,t+1} + \delta I_{U,t+1}\right] \tag{11}$$

$$\begin{aligned} I_{U,t} = \frac{1}{1+r}\Big[&\big(1-h(u_{t-1}, v_t, R)\big)I_{U,t+1} \\ &+ h(u_{t-1}, v_t, R)\Big((1-\delta)I_{E,t+1} + \delta I_{U,t+1}\Big)\Big] \end{aligned} \tag{12}$$

Similarly, at steady state, we have $I_{E,t} = I_{E,t+1} = I_E$, $I_{U,t} = I_{U,t+1} = I_U$. The worker's surplus can be obtained by subtracting Equation (12) from Equation (11):

$$I_E - I_U = \frac{1+r}{r+\delta+(1-\delta)h(u,v,R)}w \tag{13}$$

E. Equilibrium wage

Workers and firms bargain over the surplus associated with the match. The wage w is derived from a generalized Nash-bargaining process over the total intertemporal surplus:

$$w = \text{argmax}(I_E - I_U)^{\beta}(I_F - I_V)^{1-\beta} \tag{14}$$

where $0 \le \beta \le 1$ is the worker's bargaining power. Given the free-entry condition (i.e., $I_V = 0$), and using the Equations (3), (13), (9), and (10), we obtain the final Equation under the condition of using the referral:

$$w_i = \underbrace{\frac{\beta[r + \delta + (1-\delta)h(R_i)]}{r + \delta + \beta(1-\delta)h(R_i)}}_{\text{Wage responses to use } R \text{ enhancing hiring probability}} \underbrace{\left[\gamma(r)E\left[\theta_i|s_{i,F}\right] + \left(1-\gamma(r)\right)\mu_\theta\right]}_{\text{Wage responses to increased } a \text{ enhancing matching quality}} \tag{15}$$

where $E\left[\theta_i|s_{i,F}\right] = \mu_\theta + \frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\varepsilon^2/a}(s - \mu_\theta)$.

The wage outcome reflects two primary mechanisms of referral effects. The first mechanism captures the information transmission effect, where a worker's wage responds to the increased hiring probability $h(R)$ as they use the referral. The second mechanism reflects the screening effect, where the use of referrals improved matching quality, further boosting wages.

F. Screening ability vs. Reputation cost

Based on Equation (15), we find that wages are determined by both reputational cost and screening ability. In this section, we define two types of referrals and discuss the situations in which each is more beneficial. The first type is external referral (R_E), which has higher screening ability but lower reputational cost. The second type is internal referral (R_I), which has higher reputational cost but lower screening ability. The wage difference between using these two types of referrals is given by:

$$\Delta w = w_E - w_I = \Gamma[\gamma(r_E)\tau(a_E) - \gamma(r_I)\tau(a_I)] \tag{16}$$

where $\Gamma = \frac{\beta[r+\delta+(1-\delta)h(R_i)]}{r+\delta+\beta(1-\delta)h(R_i)}$ and the matching quality function is defined as $\tau(a_j) = \frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\varepsilon^2/a_j}(y-\mu_\theta)$. The matching quality function is monotonically increasing and concave, as $\frac{d\tau(a_j)}{da_j} > 0$ and $\frac{d^2\tau(a_j)}{da_j^2} < 0$. Rearranging the condition for a job seeker to prefer using an external referral over an internal referral, with $\Delta w > 0$, we obtain:

$$\frac{\tau(a_E)}{\tau(a_I)} \geq \frac{\gamma(r_I)}{\gamma(r_E)} \tag{17}$$

This inequality indicates that the relative improvement in matching quality due to higher screening ability must outweigh the relative loss in credibility from lower reputational costs. To further explore this, we can derive the cutoff value of signal noise that determines whether a job seeker should utilize an external or internal referral. Substituting the matching quality function into Equation (17), we have:

$$\sigma_{\varepsilon,cut}^2 = \frac{\sigma_\theta^2\left(1-\frac{\gamma(r_E)}{\gamma(r_I)}\right)}{\left(\frac{\gamma(r_E)}{\gamma(r_I)}\frac{1}{a_I} - \frac{1}{a_E}\right)} \tag{18}$$

The cutoff signal noise σ_{cut}^2 delineates the boundary at which a job seeker transitions from preferring an internal referral to an external referral. When $\sigma_\varepsilon^2 > \sigma_{\varepsilon,cut}^2$ (i.e., job seeker experiences high signal noise), the benefit of higher screening ability ($a_E > a_I$) provided by external referrals overcomes the loss in credibility, with $\gamma(r_E) < \gamma(r_I)$. Under this condition, the external referrals become the preferred choice. When $\sigma_\varepsilon^2 <$

$\sigma^2_{\varepsilon,cut}$ (i.e., job seeker experiences low signal noise), the credibility of internal referrals $\gamma(r_E) < \gamma(r_I)$ dominates the lower screening ability ($a_E > a_I$). Then, the internal referrals become the preferred choice.

Furthermore, $\sigma^2_{\varepsilon,cut}$ decreases with an increase in a_E and increases with a_I. This implies that if the external screening ability improves, the cutoff signal noise will decrease, leading more high-noise signal job seekers to prefer external referrals. Conversely, if the reputational cost of internal referrals (r_I) increases, the cutoff signal noise rises, resulting in more low-noise signal job seekers favoring internal referrals. In the following empirical analysis, we will investigate this relationship in detail.

2.3 Empirical Implementation

Preliminary evidence indicates that workers who find and obtain jobs through referrals have a higher hiring probability and better job matching, which leads to a higher starting wages. According to our theoretical model, there are two main pathways through which the referral effects operate, namely information transmission and screening mechanisms. Moreover, it is crucial to examine the different referral types, such as internal and external referrals, affect workers with varying levels of noisy signals.

A. Baseline Specification and Identification

To distinguish between the information transmission and screening mechanism, we are firstly to separate the strong and weak tie referral, as strong tie referral are less likely to affect the labour outcome through the screening mechanism either because such referral has less screening ability or reputation cost. The analysis employs a linear regression model to examine the effects of referrals on job-finding probability. Previous studies have shown that with sufficiently large sample sizes, the marginal effects of Probit or Logit models approximate those of linear regression. The fixed effects model can generally be expressed as:

$$F_{its} = \alpha_0 + \sum_{k=1}^{2} \gamma_k SR_{its}^{k} + \boldsymbol{X}'_{its}\eta + \varphi_t + \mu_s + \varepsilon_{its} \tag{19}$$

where F_{its} represents the dummy variable indicating whether worker i receives any offer in state s during period t; $SR_{its} \in \{SR_{its}^{S}, SR_{its}^{W}\}$ denotes whether the worker i search for a job in the last four weeks through the weak and strong tie referrals; $\boldsymbol{X}'_{its}$ is a vector including a set of personal variables, such as age, age square, gender, education, marital status, number of children, family income, and searching efforts. We also include the time and state fixed effect, with φ_t and μ_s, to control the time and state specific unobserved factors; ε_i denotes random disturbance. For the regression on matching quality and starting wages, we employ a log-linear model:

$$y_{itso} = \alpha_0 + \sum_{k=1}^{2} \delta_k R_{itso}^{k} + \boldsymbol{X}'_{itso}\eta + \boldsymbol{W}'_{itso}\nu + \varphi_t + \mu_s + \tau_o + \varepsilon_{itso} \tag{20}$$

where y_{itso} is either the matching quality or starting wage at time t. R_{itso}^{k} is either the $LR_{itso}^{k} \in \{LR_{itso}^{S}, LR_{itso}^{W}\}$ denotes whether the worker i learn about their current job through the weak or strong tie referrals.[40] Notably, the variable R_{itso}^{k} explain the referral networks utilized during the job search period, while starting wage is received at the working period, which helps to mitigate the reverse causality issue. Also, variable R_{itso}^{k} from job searching period helps avoid capturing contemporaneous peer effects, such as peer ability and peer pressure, as discussed in Cornelissen et al. (2017). $\boldsymbol{W}'_{itso}$ is a vector including a set of additionally working variables, such as, working hours, working sector, job satisfaction, full-time and firm size. And τ_o capture the occupation fixed effect.

There are five main concerns regarding the identification of the causal effects of

[40] The referral variables used in the job-finding model in Equation (1) and those for matching quality and starting wage in Equation (2) differ. The first captures how the job seeker searched for a job, while the second captures how the worker learned about their current job.

referrals on labor market outcomes. First, bias may arise from the fact that workers often use other job search channels, such as advertisements, employment agencies, or online platforms, rather than formal referral channels. To address this, we control for all informal job search methods, leading to the formal channels as the reference group. Second, the referral variables in matching quality and starting wage regressions, namely $LR_{itso}^{k} \in \{LR_{itso}^{S}, LR_{itso}^{W}\}$, may capture the composition effect through job information or screening mechanisms. To isolate the information transmission mechanism and focus specifically on the signaling mechanism, we introduce an additional set of referral variables $DR_{itso}^{k} \in \{DR_{itso}^{S}, DR_{itso}^{W}\}$, which ask workers whether they obtained their current job directly through a referral. Nonetheless, The difference between LR_{itso}^{k} and DR_{itso}^{k} can further isolates and obtains the effect of the information transmission mechanism.

B. PSM and Heckman correction

Thirdly, job searchers are likely to use the referrals based on their observable personal characteristics, which may lead to a potential selectivity bias. The aim of Propensity Score Matching (PSM) is to balance the observable characteristics between the treated and control groups. This method allows us to create a counterfactual group that closely resembles the treated group in terms of observable characteristics. The propensity score, which is the probability of treatment assignment conditional on observed personal characteristics ($\boldsymbol{X}_{its}'$), is estimated using three logistic regressions:

$$R_{its}^{j} = \alpha_0 + \boldsymbol{X}_{its}'\eta + \varphi_t + \mu_s + \varepsilon_{ist} \quad (21)$$

where R_{its}^{j} is either strong or weak tie referral in job-finding, matching quality, and starting wage regressions. Once the scores are estimated, we match each treated individual with a control individual who has a similar propensity score. This process is facilitated by using a kernel matching method, which uses a weighted average of all control individuals to construct the counterfactual outcome. The bandwidth parameter for the kernel matching is set to 0.006, which determines the weight of control individuals in the construction of the counterfactual outcome. The common support option is also used to exclude treated individuals who have propensity scores higher than the maximum or lower than the minimum propensity score of the control individuals. This ensures that the comparison between the treated and control groups is only made within the range of common support.

Fourthly, despite the adoption of the PSM, there is a possibility of bias in the estimation due to the fact that the data used for matching quality and starting wage is collected only from individuals with jobs. This absence of data from unemployed individuals introduces a non-random element to our dataset, which could potentially skew our results and lead to biased conclusions, which also known as self-selection issue (Heckman et al., 1997). To rectify this self-selection, we use a Heckman correction, with "*married status*" (M_{its}), as the relevant and exclusion restriction based on its impact on labor force participation but not on matching quality and starting wage. There are some papers where Heckman himself has used marital status in the first stage of his selection model (Heckman, 1974; Heckman and MaCurdy, 1980). Moreover, to confirm our instrument variable validity, we use a reduced-form approach to directly test the exclusion restrictions on M_{its}, and we do not expect them to have any statistically significant direct effect on matching quality and starting wage (see Appendix C).[41] Applying the Heckman model typically involves the following two stages. In the first stage, a Probit model predicts the likelihood of employment (Em_{its}) for individual i in state s at time t, formulated as:

$$Em_{its} = \alpha_0 + \beta M_{its} + \boldsymbol{X}'_{its}\delta + \gamma_t + \tau_s + \varepsilon_{its} \tag{22}$$

[41] The reduced form in the Heckman model differs from that of the instrumental variable (IV) method. In the IV model, we expect the instrument z to be correlated with the outcome y through the endogenous variable, with $cov(y, z) \neq 0$, but z must be uncorrelated with the error term, with $cov(\varepsilon, z) = 0$. In contrast, the Heckman model addresses selection bias by assuming the instrument z is unrelated to the outcome but influences selection into the sample. This leads to the assumption $cov(y, z) = 0$ in the reduced form for the Heckman model, which simplifies the process of checking instrument validity.

where the mar_{its} denotes the marital status (*0=no married, 1=married*). Following this, we calculate the inverse Mills ratios $imrEm_{its}$ and incorporate them into the baseline regression model (2) to address self-selection problem.

C. Shift-share instrument variable

So far, we have discussed the identification strategy for examining the effect of referrals on job-finding probability, matching quality, and starting wage. However, to ensure the robustness of our results, we have to address one more issue: the potential bias arising from unobserved variables related to the SR_{it}^{k} in the job-finding analysis as well as LR_{it}^{k} and DR_{it}^{k} in the matching quality and starting wage analysis that may affect labor outcomes, which also known as the endogeneity issue. Previous research has suggested that individuals with better communication skills and social abilities are more likely to use informal referrals (Diaz, 2012; Cappellari and Tatsiramos, 2015). However, these skills and abilities cannot be directly observed in the dataset being used for this study. To address this issue and obtain causal estimates, we employ the shift-share instrumental variable (SSIV) approach (Autor et al., 2013; Goldsmith-Pinkham et al. 2020; Acemoglu et al., 2016). The shift-share IV is effective due to two key factors: relevance and the exclusion restriction. Relevance is ensured by leveraging exogenous variation in referral usage across states, occupations, and time periods. Historical data and growth rates at these levels generate variation in referral likelihood, unrelated to individual characteristics. The exclusion restriction holds because the instrument, based on past referral patterns, influences current labor market outcomes only through its effect on referral usage, without directly impacting individual outcomes. Specifically, in the matching quality and starting wage regressions, the basic idea is to use historical data on the distribution of referral types within each occupation in state s in year $t-1$ and the overall growth rate of different type of referral usage in the state from year $t-1$ to year t. This allows us to predict the proportion of each type of referral in state each state s in year t is, denoted as ssR_{tso}^{k}, which is highly correlated with the actual value of R_{itso}^{k}, and uncorrelated with the other residual terms (Adao et al., 2019;

Borusyak et al., 2022). The following section conducts a placebo test to further verify the validity of our instrument. Specifically, we construct the following measures:

$$R_{ts}^{k} = \sum_{o} R_{tso}^{k} \text{ and } P_{ts} = \frac{R_{ts}^{k} - R_{(t-1)s}^{k}}{R_{(t-1)s}^{k}} \tag{23}$$

The predicted value of the referral proportion is then:

$$ssR_{tso}^{k} = R_{(t-1)so}^{k} \times (1 + P_{ts}) \tag{24}$$

The ssR_{tso}^{k} is used in the job-finding, matching quality and starting wage regressions.[42] Moreover, in the job-finding regression, given that both of our dependent variables (F_{its}) and potential endogenous variables (SR_{it}^{k}, LR_{it}^{k} and DR_{it}^{k}) are dummies, applying a linear IV model may result in what is known "forbidden regression" (Angrist and Pischke, 2008). Hence, it would be better to consider the control function (CF) method to obtain a more efficient estimator (see Wooldridge, 2015). Overall, in the first stage regression, three Probit models predict R_{its}^{j} for job-finding analysis:

$$SR_{itso}^{j} = \alpha_0 + \sum_{k=1}^{2} \zeta_k ssSR_{tso}^{k} + \boldsymbol{X}_{itso}'\eta + \gamma_t + \tau_s + \varepsilon_{itso} \tag{25}$$

Following this, we calculate three Inverse Mills ratios $imrS_{itso}$, $imrE_{itso}$ and $imrI_{itso}$, and incorporate them into the Equations (1) to address endogeneity problem for the job-finding regression. For the matching quality and starting wage regressions, we include the lagged values of the different types of referrals used and compute the Inverse Mills Ratios by first estimating the following regression:

[42] Since some workers are unemployed, their occupation is unobservable. Therefore, we assign these unemployed workers to a separate group (about 26.7%).

$$R_{itso}^{j} = \alpha_0 + \sum_{k=1}^{2} \zeta_k ssR_{tso}^{k} + \boldsymbol{X}'_{itso}\eta + \boldsymbol{W}'_{itso}\nu + \varphi_t + \mu_s + \tau_o + \varepsilon_{itso} \qquad (26)$$

where R_{itso}^{j} is either LR_{it}^{k} or DR_{it}^{k}; and ssR_{tso}^{k} is either $ssLR_{it}^{k}$ or $ssDR_{it}^{k}$.

D. External vs. Internal

As discussed in our theoretical framework, workers with low-noise signals who use internal referrals (i.e., higher reputational cost but lower screening ability) are more likely to receive higher wages, while high-noise signal workers benefit from external referrals (i.e., lower reputational cost but higher screening ability). To identify external referrals, we use the variable "co-worker referrals", which captures referrals from former co-workers (more details see Appendix A). A co-worker referrer, having previously worked with the job seeker, is better positioned to assess the job seeker's true productivity compared to an employee referrer. However, such referrers are less likely to face consequences from the employer, as they work for different firms. For internal referrals, we use the variable "employee referrals", which captures referrals from current employees. Although an employee referrer may have lower screening ability, employers are more likely to trust these referrals due to the potential reputational consequences for the referrer. Additionally, we define "noisy signals" based on the education level of job seekers. Job seekers with higher education are assumed to have "high-noise signals", while those with lower education are considered to have "low-noise signals" (Spence, 1973; Gibbons and Lawrence, 1991). To test this, we run separate regressions for high- and low-noise signal workers using the following specification:

$$y_{itso} = \alpha_0 + \sum_{k=1}^{3} \delta_k R_{itso}^{k} + \boldsymbol{X}'_{itso}\eta + \boldsymbol{W}'_{itso}\nu + \varphi_t + \mu_s + \tau_o + \varepsilon_{itso} \qquad (27)$$

where y_{itso} is either the job-finding, matching quality or starting wage at time t. R_{itso}^{k}

is either the $SR_{itso}^{k} \in \{SR_{itso}^{C}, SR_{itso}^{E}\}$ or $DR_{itso}^{k} \in \{DR_{itso}^{C}, DR_{itso}^{E}\}$ indicating whether the worker found the job or learned about their current job through co-worker or employee referrals.

2.4 Data and main variables

In this paper, the representative dataset from the Survey of Consumer Expectation (SCE) by the Federal Reserve Bank of New York is used. To be specific, the SCE is designed as a rotating panel and used as a monthly survey of around 1,000 to 1,300 participants for one year from 2013 to 2019. Every month, the SCE asks the participants various economic questions, such as consumer's expectations regarding inflation, household finance, and the labor and housing markets. This paper focuses on the labor market survey, because the questionnaire includes how the participants learn about their current job and the job search method they have adopted. Among them, one of the questions is about whether the participants make use of referral to find their job, which is highly correlated with this research. Indeed, the SCE has many other important features, such as the detailed information about working hours, searching effort and firm's background, which can help control the observed heterogeneity. Due to the fact that this paper investigates the effect of referral on labor market, it merely focuses on the 18-65-year-old workers (not self-employed). Additionally, we exclude individuals who used multiple types of referrals in their job search to avoid confounding effects. After that, the effective sample contains 5,286 observations, with there are about 1,365 participants searching the jobs in the last 4 weeks (i.e., including on-the-job search), and 4,114 employed workers report their searching methods.

A. Identify for dependences

In order to capture the effect of referral on the labour market, this paper introduces job finding probability, matching quality, and labour income as the dependent variables. First, for the job finding probability, it can be observed by the question: "*How many job* *offers did you receive in the last 4 months?*" Based on this question, this paper generates the dummy variable, which equals to 1 if the respondent receives any offer over the last four months, otherwise equals to 0. The preliminary result reveals that job searchers who use the referral are 13.5% more likely to find jobs than those who do not (see Table 1). In terms of the matching quality, this study uses the subjective measurement and generates the ordered variable based on the question: "*How well do you think this job fits your experience and skills from 1 to 7?*" In the mean level, the referred workers have higher matching quality than the non-referred workers (see Table 2.2), but it is not significant. To obtain the net effect of referrals on potential labor income, this paper prefers to use the starting wage rather than current wage because the current wage may be influence by various other factors, such as working experience and working performance, and peer effect. To identify the starting wage, this paper considers the question: "*How much did you make when you started your main/current job, before taxes and other deduction?*". Since the starting wage may refer to a period several years ago, we adjust these values for inflation based on the respondent's current job tenure to calculate the real starting wage. As displayed in Table 2.2, the average real starting hourly wage is about 28.407 dollars in the whole sample. Obviously, a job searcher who uses the referral gets 3.799 dollars more than those who do not. In general, a job searcher with a referral has higher job finding probability, matching quality, and starting wage.

Table 2.1: Descriptive Statistics (Job-finding analysis)

	Full		Non-Referral		Referral		
	Mean	SD	Mean	SD	Mean	SD	Diff
Dependent variable							
Job finding probability	.50	.5	.43	.49	.57	.49	-0.13***
Independent variable							
Social referral	.37	.483					
Co-worker referral	.327	.469					
Employee referral	.2	.4					
Personal characteristics							
Age	42.9	11.7	42.9	11.7	42.8	11.7	0.114
Gender	.437	.496	.425	.495	.447	.498	-0.0220
Marital status	.576	.494	.58	.494	.572	.495	0.00800

Education (1-8)	4.555	1.434	4.456	1.473	4.642	1.394	-0.186**
Unemployment history							
Weekly searching hours	7.1	54.6	3.7	6.1	10.0	74.5	-6.346**
Tenure	1.235	.942	1.169	.819	1.293	1.034	-0.124**
On-the-job search	.78	.414	.828	.378	.739	.44	0.089***

NOTE.—Data are sourced from the Survey of Consumer Expectations (SCE), covering the period from 2013 to 2019. Job-finding probability is defined as the likelihood that a job seeker received at least one job offer in the past four months. Social referrals refer to those made by friends or relatives, co-worker referrals are from former colleagues, and employee referrals are from current employees. The total sample consists of approximately 1,365 observations.

B. Identify for different types of referrals

The SCE provides detailed information on the job search methods used by workers, capturing whether individuals found their job through a referral, learned about their current job via referral connections, or obtained their job directly through a referral. Specifically, one question asks, "*What were all the things you have done to look for work during the last 4 weeks?*" to capture the channels used during the job search period. Another question asks, "*How did you learn about your current job?*" to identify the composition effect of referral, specifically the information transmission and screening mechanisms, through which the worker obtained their current position. Additionally, to isolate the information transmission mechanism and focus specifically on the signaling mechanism, we use an additional question: "*Did potential employers contact you for your current job because of weak tie, co-worker, or employee referrals?*" (see Appendix A for responses).[43] The difference between questions (2) and (3) can further isolates the effect of the information transmission mechanism, where workers receive job-related information through a referral but do not secure the job directly through it.

We use the questions in different ways. The question (1) is only used for the job finding probability regression, because respondents do not need to get a job to answer this question. However, the questions (2) and (3) cannot be used for the matching quality and labor income regression, as some respondents may not have the current jobs. As a supplement to question (1), the questions (2) and (3) is used for the matching

[43] Unlike previous research that proxies or infers referrals based solely on job information shared, which may involve a job information mechanism without requiring the referrer to formally recommend the candidate, our approach allows us to directly observe the referral process. This enables us to isolate the job information mechanism and focus specifically on the screening and signaling mechanisms.

quality and labor income regression because it focusses on interviewees with current jobs. In general, we construct two dummy variables, $SR_{it}^{j} \in \{SR_{it}^{S}, SR_{it}^{W}\}$, where each equals 1 if the worker found their job through a strong- or weak-tie referral, based on question (1). Additionally, we construct $LR_{it}^{j} \in \{LR_{it}^{S}, LR_{it}^{W}\}$, where each equals 1 if worker directly used strong- or weak-tie referrals to learn the current jobs, according to question (3). Lastly, $DR_{it}^{j} \in \{DR_{it}^{S}, DR_{it}^{W}\}$ equals 1 if the worker learned about their current job through job-related information from a strong- or weak-tie connections, based on questions (2) and (3) (more details see Appendix A).

In our sample, referrals are the most common job search method, with 37%, 33%, and 20% of respondents using social, co-worker, and employee referrals, respectively, to search for new jobs (see Table 2.1). This aligns with findings by Schmutte (2016) and Topa (2011), who report that roughly half of workers search for jobs through referrals. Approximately 21% and 17% of respondents learned about their job through strong-tie and weak-tie connections, while only about 4% and 9% obtained their job directly through weak-tie and strong-tie connections, respectively (see Table 2.2). These lower proportions compared to previous research may reflect our unique dataset's ability to isolate only the screening mechanism.

Table 2.2: Descriptive Statistics (matching quality and starting wage analysis)

	Full		Non-referral		Referral		
	Mean	SD	Mean	SD	Mean	SD	Diff
Dependent variables							
Matching quality (1-7)	5.937	1.142	5.926	1.136	5.985	1.165	-0.0580
Hourly real starting wage	28.40	84.16	27.688	85.50	31.487	78.119	-3.799
Screening							
Strong tie referral	.041	.199					
Weak tie referral	.091	.288					
Information transmission							
Strong tie referral	.210	.408					
Weak tie referral	.171	.377					
Personal characteristics							
Age	43.13	11.54	43.44	11.52	41.805	11.558	1.635***
Gender	.514	.5	.504	.5	.556	.497	-0.05***
Marital status	.665	.472	.656	.475	.702	.458	-0.046**

Education (1-8)	4.645	1.425	4.601	1.439	4.833	1.353	-0.23***
Employment history							
On-the-job search	6.878	25.55	6.888	26.50	6.834	21.032	0.0540
Working Hours (weekly)	40.49	10.47	40.279	10.65	41.395	9.601	-1.11***
Commuting time (Monthly)	43.80	34.86	43.85	40.63	44.084	37.731	-0.234
Satisfied (1-7)	3.783	.989	3.766	.974	3.856	1.048	-0.090**
Promotion (1-7)	4.11	1.789	4.068	1.755	4.29	1.921	-0.22***
Full time	.73	.444	.689	.463	.903	.297	-0.21***
Firm background							
Firm size	2.832	1.467	2.827	1.462	2.85	1.486	-0.0230
Type of firm	.262	.44	.26	.439	.267	.443	-0.00700

NOTE.—Data are sourced from the SCE, covering the period from 2013 to 2019. Starting wage refers to the hourly income from the primary job and inflate it according to the tenure. Marching quality refers to the subjective measuring by the workers sides. Social referrals refer to those made by friends or relatives, co-worker referrals are from former colleagues, and employee referrals are from current employees. Type of firm is either public or private sector. The total sample consists of approximately 3,921 observations.

C. Control variables

In the regression, the factors of personal characteristics and working background are controlled. The personal characteristics consist of gender (*0=female, 1=male*), age (*in year*), educational level (*1-8*). According to some literature, there is probably an inverted U-shaped relationship between dependent variables and age, searching effort. To capture them, the square of age and searching effort variables are added. Moreover, for avoiding the composition effect of using referrals (i.e., unemployed job searchers may search more hours), this paper considers more about the unemployment and working background, such as *unemployment time*, *length of looking for jobs*, *tenure*, and *whether searching on-the-job*. Except for above control variables, the "firm background" is added in the matching quality and labor income regression, such as "job type (*0=private, 1=public*)" and "firm size (*in year*)". Overall, on average, workers who used referrals tend to have higher matching quality, higher hourly starting wages, and work longer hours compared to those who did not use referrals. They also exhibit more favorable personal characteristics, such as being slightly younger, more likely to be male, and having higher education levels.

2.5 Empirical results

This section investigates the effect of weak- and strong-tie referrals on labor outcomes, examining whether these effects occur through the information transmission or screening mechanisms, following four steps. First, we use a naïve baseline model to explore the association between referral types and labor outcomes. Second, we address self-selection and employment issues by applying the SSIV-Heckman model. In the third step, we delve deeper into how the screening mechanism operates, introducing concepts such as screening ability and reputational cost. Finally, we conduct a series of robustness checks, including first-stage, reduced-form, and placebo tests, to validate our instrumental variables.

A. Baseline results

To examine the information transmission mechanism of referrals, we first analyze their effect on job-finding probability. Table 2.3 shows the effect of strong and weak ties of referrals on the job finding probability based on the fixed effect, PSM and PSM-SSIV models. As column 1 shown, after controlling for basic variables, as well as year and state fixed effects, job seekers who utilize weak tie referrals see a significant increase in their job-finding probability by about 19.0 percentage points ($p<0.01$), compared to those who do not use referrals. In contrast, job seekers who rely on strong tie referrals experience a decrease in job-finding probability by about 5.8 percentage points ($p>0.1$), although this result is not statistically significant. After controlling the other informal channels such as such as advertisements, employment agencies, and online platforms, leading to the reference group is formal market, the coefficient remain similar (see column 2). Also, these results remain similar when excluding unmatched groups based on the observable personal characteristics by PSM.

Moreover, to obtain a more accurate and comprehensive understanding of the causal effect, it is important to account for potential bias arising from unobservable characteristics that may influence both job seekers' use of different ties of referrals and the outcome variables, such as social skills. To address this endogeneity problem, we employ the shift-share instrumental variable (SSIV) method, as presented in the fourth column of the results. The small difference in coefficients between the PSM and SSIV methods suggests that there may not be significant confounding effects from unobserved variables. Furthermore, the insignificance of the three Inverse Mills ratios (i.e., *imr_S* and *imr_W*) from the first stage also indicates no evidence of endogeneity in our job-finding analysis (for additional robustness checks, see the next section). Overall, our results show that job seekers who utilize weak tie referrals in their job search are approximately 6.0 percentage points ($p>0.1$) less likely to find a job compared to those who find job through formal market, although this result is not statistically significant. This finding is consistent with previous studies by Ioannides and Loury (2004) and Kramarz and Skans (2014), which suggest that relying on strong-tie referrals can disadvantage job seekers in the labor market due to less reliable sources of information. In contrast, workers who use weak tie referrals are 14.7 percentage points ($p<0.01$) more likely to find a job, compared to those using formal channels. These findings are consistent with studies by Calvó-Armengol and Zenou (2005), Ioannides and Loury (2006), and Cappellari and Tatsiramos (2015), which suggest that weak-tie referrals can increase job-finding probabilities, because the job information comes from employees who are better connected to the labor market.

Table 2.3: The effect of different type referrals on job finding probability

	(1) Basic control & state and time FE	(2) Plus other job channels	(3) PSM	(4) SSIV
Strong tie	-0.058	-0.069	-0.055	-0.060
	(0.048)	(0.048)	(0.048)	(0.048)
Weak tie	0.190***	0.175***	0.145***	0.147***
	(0.045)	(0.046)	(0.046)	(0.046)
imr_S				0.042
				(0.091)
imr_W				-0.027
				(0.085)
N	954	954	954	954
R^2	0.1247	0.1441	0.1560	0.1569
Control variables	Yes	Yes	Yes	Yes
Time and State FE	Yes	Yes	Yes	Yes

Other job channel	Yes	Yes	Yes
Matched		99%	99%

Notes: Estimates are from regressions on the job finding probability for the workers. Strong tie referral represents workers find jobs through relative social friends; Weak tie referral refers to find jobs through past co-worker or current employee. The full set of controls described in equation (1) is included but not reported. The total number of observations is 941, as relatively few workers searched for jobs in the past four weeks, and the use of SSIV results in the loss of one period of observation to construct the growth index. Columns 1-4 present linear models for the dummy variable outcomes. Standard errors are presented in parentheses below the point estimates. ∗∗∗ denotes significance at the 1% level, ∗∗ denotes significance at the 5% level, and ∗ denotes significance at the 10% level.

In addition to job-finding probability, we expect referrals to impact both matching quality and starting wages, specifically through the screening mechanism. To distinguish whether referrals affect these outcomes via the screening mechanism or the information transmission mechanism, we use two different referral variables. First, we observe whether workers directly obtained their current job through a referral, capturing the screening mechanism. Second, we observe whether workers learned about their current job through a referral, which combines both the screening and information transmission mechanisms. The difference between these two variables isolates the effect of the information transmission mechanism, where workers receive job-related information through a referral but do not secure the job directly through it. First, we examine matching quality by focusing on referrals used directly to obtain jobs, testing the screening mechanism. As shown in column 1 of Table 2.4, we apply the IV-Heckman model to address potential self-selection and endogeneity issues (see columns 1-2). The insignificance of the Inverse Mills ratio for employment probability (*imr_Em*) suggests that self-selection is not a concern in the matching quality regression. However, the significance of the Inverse Mills ratio for weak-tie referrals (*imr_W*) supports the presence of endogeneity (see Wooldridge, 2015).

On average, our IV-Heckman estimates indicate that workers who use weak-tie referrals to obtain jobs experience an increase in subjective matching quality of 4.5% ($p<0.05$), compared to those who do not use referrals.[44] In contrast, using the strong-

[44] The reference group consists of those who did not use referrals, as we do not control for other informal channels. However, this does not bias our results, as shown in Table 3.3, where the findings remain consistent before and after controlling for other informal channels.

tie referrals do not significantly impact subjective matching quality. These findings align with our framework, which posits that weak-tie referrers possess more screening ability about job seekers than strong-tie referrers. Notably, weak-tie referrals are primarily composed of co-worker and employee referrals (further details are provided in the next section). On the one hand, past co-workers, having worked directly with the job seeker, are well-positioned to assess their suitability for specific roles, leading to more accurate referrals and better matching. On the other hand, current employee referrers may have more knowledge about the job requirements than the job seeker's abilities, resulting in less precise referrals. The lower and insignificant effect of social referrals on matching quality could be attributed to the fact that social connections often lack detailed knowledge of both the job seeker's professional skills and the specific job requirements.

Interestingly, when considering the information transmission mechanism, the results differ from those based on the screening mechanism. Using a similar identification strategy, our IV-Heckman estimates indicate that workers who learned about their jobs through weak tie referrals do not experience a significant increase in matching quality (see column 2). This suggests that simply learning about a job through weak tie connections does not enhance matching quality. One possible explanation is that information shared through informal channels is often limited to general job availability or basic job characteristics, rather than providing specific insights that could better align the worker's skills with the job's requirements. This finding is consistent with Calvó-Armengol and Zenou (2005), Ioannides and Loury, (2006); Schmutte, (2016), who demonstrated that learning jobs using informal connections only increases hiring probability (see Table 2.3) but not matching quality. Moreover, the lack of a formal referral may reduce the employer's incentive to screen candidates more thoroughly, further explaining the absence of a significant effect on matching quality. In general, we find that workers who use weak-tie connections to learn about their jobs through the information transmission mechanism do not experience improvements in matching quality. However, those who secure jobs directly through weak-tie referrals via the screening mechanism do see an improvement in matching quality. Nonetheless, our IV-

Heckman estimates indicate that workers who learned about their jobs through strong-tie referrals experience a significant decrease in matching quality, by 1.8% ($p<0.05$). This may be due to the referrers' limited understanding of both the worker's qualifications and the specific job requirements.

Table 2.4: The effect of different type referrals on subjective matching quality and starting wage

	Matching quality		Starting wage	
	Screening	Information	Screening	Information
	(1)	(2)	(3)	(4)
Strong tie	-0.015	-0.018**	-0.108*	-0.041
	(0.021)	(0.009)	(0.064)	(0.030)
Weak tie	0.045**	-0.002	0.112***	0.048
	(0.018)	(0.009)	(0.036)	(0.033)
imr_Em	0.026	-0.066	1.242*	1.348**
	(0.119)	(0.108)	(0.647)	(0.683)
imr_S	-0.049	0.151	-1.109*	0.977
	(0.071)	(0.121)	(0.628)	(0.987)
imr_W	0.129*	-0.186*	-0.062	0.281
	(0.067)	(0.100)	(0.570)	(0.423)
N	2,653	2,653	2,752	2,752
R^2	0.1298	0.1327	0.3384	0.3338
Control variables	Yes	Yes	Yes	Yes
T & S & O FE	Yes	Yes	Yes	Yes
Matched	Yes	Yes	Yes	Yes

Notes: Columns 1 (2) and 3 (4) present the SSIV-Heckman estimated effects of strong and weak tie referral on matching quality and real starting wage by screening (information transmission) mechanism, respectively. Both matching quality and starting wage are expressed in logarithmic form. The full set of controls described in equation (2) is included but not reported. "T & S & O FE" refers to time, state and occupation fixed effects. The use of SSIV results in the loss of one period of observation to construct the growth index. Standard errors are presented in parentheses below the point estimates. ∗∗∗ denotes significance at the 1% level, ∗∗ denotes significance at the 5% level, and ∗ denotes significance at the 10% level.

In line with learning theory, if weak tie referrals improve the job search process, it is reasonable to expect that they would also lead to higher labor income (reference). However, the key question is whether this increase in starting wages is driven by the information transmission mechanism or the screening mechanism. Column 3 of Table 2.4 present the regression results of the effect of weak and strong ties of referrals on real starting wages through the screening mechanism. Unlike the matching quality regression, we observe evidence of self-selection issue in the wage regression, as indicated by the significance of the Inverse Mills ratio for the employment probability (*imr_Em*). Additionally, the significance of the Inverse Mills ratio for weak tie referrals (*imr_W*) also suggests the presence of endogeneity. After addressing both self-selection and endogeneity issues, the IV-Heckman estimates reveal that job seekers who use weak tie referrals experience a significant increase in their real starting wage, by approximately 11.2% ($p<0.01$), compared to those who do not use referrals (see column 3). In contrast, merely learning about a job through weak-tie connections does not result in a significant increase in starting wages (see column 4). This finding is consistent with studies by Bayer et al. (2008), Hellerstein et al. (2011), Dustmann et al. (2016), and Brown et al. (2016), which show that referrals can increase starting wages. However, our analysis distinguishes between the effects of the screening and information transmission mechanisms and identifies the wage increase as driven by the screening mechanism. Furthermore, our results align with Bentolila et al. (2010) and Kramarz and Skans (2014), who find that workers securing jobs primarily through the information mechanism or via strong-tie referrals do not experience any significant effect on starting wages.

Nonetheless, unlike Dustmann et al. (2016) and Brown et al. (2016), we argue that if a referral effectively screens a worker's productivity and leads to better matching quality, it can naturally result in higher current wages and even wage growth over time. To examine wage growth, we calculate the difference between the current wage and the starting wage, controlling for tenure. As shown in columns 1-2 of Appendix B, using a weak-tie referral has a persistent effect on both current wages and wage growth, with increases of 8.6% ($p<0.01$) and 6.9% ($p<0.05$), respectively. These results underscore the importance of using weak-tie referrals in not only securing initial employment but also driving long-term wage growth through more accurate screening and better job matching.

B. Screening mechanism

We have found that weak-tie referrals can improve job-finding probability through the information transmission mechanism and enhance matching quality and labor income through the screening mechanism. The next step is to investigate how the screening mechanism operates. To do this, we further divide weak-tie referrals into two types: co-worker (internal) referrals and employee (external) referrals. According to our definition, co-worker referrals include those "*referred by a former co-worker, supervisor, teacher, or business associate*". This type of referral tends to be more effective at screening a worker's unobserved productivity, as referrers often have direct experience working with the job seeker. However, these referrers typically face lower reputational costs, as they are no longer associated with the hiring firm. In contrast, employee referrals are defined as those "*referred by a current employee at the company*". While this type of referral may be less effective at screening the worker's unobserved productivity, it carries higher reputational costs for the referrer. Since current employees can be held accountable by the employer, they face greater risks if they recommend a low-productivity worker.

Intuitively, a co-worker referrer, having previously collaborated with the job seeker, is better positioned to screen the job seeker's true productivity compared to an employee referrer. However, when an employer receives a referral, they must evaluate not only the content of the information but also its credibility. Reputational cost plays a crucial role in determining the trustworthiness of the referral. Typically, the reputational cost for a co-worker referrer is lower than for an employee referrer. As a result, although an employee referrer may be less capable of accurately screening a job seeker's true productivity, employers are more likely to trust employee referrals due to the potential reputational consequences for the referrer. To examine this framework, we define the concept of "noisy signals" based on the education level of job seekers. Job seekers with higher education are assumed to have "high-noisy signals", while those with lower education have "low-noisy signals" (Spence, 1973; Gibbons and Lawrence, 1991). For low-noisy signal job seekers (i.e., those with lower education), employers are more likely to trust employee referrals. In this case, both employee and co-worker referrers may have similar screening abilities, but the higher reputational cost associated with employee referrals makes them more credible. Conversely, for high-noisy signal job seekers (i.e., those with higher education), employers may become more skeptical of employee referrals. This skepticism arises because it is more difficult for employee referrers to thoroughly screen high-noisy signal job seekers. Under these conditions, employers may place greater trust in co-worker referrals, even though they come with a lower reputational cost.

To examine above framework, we further divide the referral into three types $R_{itso}^{k} \in \{LR_{it}^{S}, LR_{it}^{C}, LR_{it}^{S}\}$ and the results are presented in Table 2.5 (also see the full sample in Appendix D). As column 1 displayed, our SSIV estimates show that low-noise signal job seekers who use employee referrals have a significantly higher probability of finding a job, with an increase of 22.8 percentage points (p<0.1). Interestingly, the use of co-worker referrals by low-noise signal job seekers does not have a significant effect on their job-finding probability. This finding is consistent with our hypothesis, suggesting that employers may place greater trust in employee referrals for low-noise signal job seekers. Additionally, our IV-Heckman estimates in column 3 indicate that low-noise signal job seekers who use employee referrals also experience higher matching quality than those who do not, with an increase of 6.3% (p<0.1). Given that low-noise signal job seekers are easier to screen, and that employee referrers have a better understanding of job requirements, employee referrals play a more critical role in improving matching quality for this group. In contrast, high-noise signal job seekers who use co-worker referrals see statistically significant increases in both job-finding probability and matching quality, by 12.3 percentage points (p<0.01) and 2.7% (p<0.05), respectively (see columns 2 and 4). However, we find no evidence that high-noise signal job seekers who use employee referrals experience a statistically significant improvement in matching quality. These results support our argument that co-worker referrers are often better positioned to screen the true productivity of job seekers than employee referrers. Therefore, employers tend to place more trust in co-worker referrals when dealing with high-noise signal job seekers, leading to higher job-finding probabilities.

In general, we found that the effect of co-worker (employee) referrals on job-finding probability and matching quality is significantly positive only for high-noise (low-noise) signal job seekers. According to learning theory, the wage premium is also contingent on these dynamics: high-noise signal job seekers benefit from co-worker referrals, while low-noise signal job seekers benefit from employee referrals. As shown in columns 5 and 6, the wage premium from employee referrals is significant for low-noise signal job seekers, amounting to a 25.7% increase ($p<0.01$). In contrast, high-noise signal job seekers see a 22% wage increase ($p<0.05$) when using co-worker referrals. These findings further support our hypothesis that the screening mechanism in the labor market is shaped by two key factors: reputational cost and information availability. When dealing with job seekers who provide low-noise signals, employers tend to trust referrals with lower screening accuracy but higher reputational cost, namely employee referrals. Conversely, when handling job seekers with high-noise signals, employers are more likely to trust referrers who provide stronger screening, despite their lower reputational cost, namely co-worker referrals. Nonetheless, job seekers with either low- or high-noise signals who rely on strong-tie referrals, such as those from relatives or social friends, do not experience higher job-finding probabilities and may even face lower starting wages. This is because these referrers are less capable of accurately assessing the job seeker's productivity, and the reputational cost associated with these referrals is relatively low, making it difficult for employers to place trust in such referrals.

Table 2.5: The effect of different types of referrals on labour outcomes by different education groups

	Job-finding		Matching quality		Starting wage	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Strong tie	-0.094	-0.031	0.021	0.009	-0.060	-0.012
	(0.081)	(0.043)	(0.023)	(0.011)	(0.068)	(0.035)
Coworker	0.124	0.123***	0.046	0.027**	0.094	0.220**

	(0.113)	(0.046)	(0.022)	(0.008)	(0.059)	(0.076)
Employee	0.228*	0.113**	0.063*	0.015	0.257***	0.042
	(0.131)	(0.056)	(0.023)	(0.019)	(0.029)	(0.066)
N	227	701	956	1,813	944	1,793
R^2	0.3290	0.1494	0.1374	0.1338	0.2892	0.3354
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
T & S & O FE	Yes	Yes	Yes	Yes	Yes	Yes
Other channels	Yes	Yes				
Matched	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns 1 and 2 present the SSIV estimated effects of strong tie, co-worker and employee referrals on job finding probability, separated by low and high education levels. Columns 3-6 display the SSIV-Heckman estimated effects of strong tie, co-worker and employee referrals on matching quality and real starting wage, separated by low and high education levels. The full set of controls described in equation (2) is included but not reported. "T & S & O FE" refers to time, state and occupation fixed effects. The use of SSIV results in the loss of one period of observation to construct the growth index. Standard errors clustered at the level of the wave and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

C. Robust and Placebo tests

The robustness of our results depends on the validity of our instrumental variable in both Heckman and IV methods. To substantiate its validity, we perform tests to ensure that it satisfies two principal assumptions: relevance and exogeneity. We only focus on the validation for the directly use referrals through the screening mechanism here, with results for search referrals and learned referrals available in Appendix C. In term of in the Heckman method, as discussed before, we use married status as the instrument. The relevance assumption is confirmed through a strong positive correlation between married status with employment probability, demonstrated by a coefficient of 0.143 ($p<0.05$) and 0.135 ($p<0.05$) on the Probit scale in matching quality and starting wage regression, respectively, as shown in columns 1 and 3 in Appendix B. Additionally, the F-test results, with values of 14.23 and 13.28, are above the critical threshold of 10, indicating no evidence of weak instruments. Moreover, our reduced-form equation for Heckman method shows that married status is not significantly correlated with matching quality and starting wage (i.e., $cov(z, y) = 0$), providing some support for the instrument's validity (see columns 2 and 4 in Appendix B).

After validating the instruments in the Heckman method, we now turn to the validation of the instruments in the SSIV method. As shown in columns 1-3 of Table

2.6, the strong positive correlation between our instrument variables (i.e., $ssLR_{tso}^{k} \in \{ssLR_{tso}^{S}, ssLR_{tso}^{E}, ssLR_{tso}^{C}\}$) and the endogeneity variables (i.e., $LR_{itso}^{k} \in \{LR_{itso}^{S}, LR_{itso}^{E}, LR_{itso}^{C}\}$ in the first-stage regressions confirms the relevance of the instruments. Additionally, F-test results exceeding the critical threshold of 10 indicate no issues with weak instruments. Although empirically validating exogeneity in the IV method is challenging, we follow Glitz (2017) by conducting a placebo test, using the shift-share instrument variables in last period as independent variables (i.e., $ssLR_{(t-1)so}^{k} \in \{ssLR_{(t-1)so}^{S}, ssLR_{(t-1)so}^{E}, ssLR_{(t-1)so}^{C}\}$). Our framework posits that the shift-share instrument behaves like an exogenous state-level shock, correlated with current outcomes but not with future outcomes. The results of the placebo test, shown in column 5, indicate that $ssLR_{(t-1)so}^{k}$ has no statistically significant effect on SR_{itso}^{k}, except in the case of social referrals. Nevertheless, these findings further confirm the validity of our instruments for co-worker and employee referrals.

Table 2.6: The SSIV estimates of first-stage and placebo test on starting wage analysis

	First-stage			Placebo: future referral usage		
	Social	Coworker	Employee	Social	Coworker	Employee
	(1)	(2)	(3)	(4)	(5)	(6)
SS: Social	**0.240****	-0.109	0.054	**0.228****	-0.088	0.027
	(0.102)	(0.116)	(0.108)	**(0.098)**	(0.105)	(0.119)
SS: Coworker	-0.220**	**0.157****	-0.050	0.096	**0.085**	-0.205
	(0.103)	**(0.075)**	(0.105)	(0.103)	**(0.085)**	(0.136)
SS: Employee	0.120	0.007	**0.279*****	0.052	-0.059	**0.062**
	(0.171)	(0.140)	**(0.095)**	(0.147)	(0.142)	**(0.132)**
N	2,752	2,752	2,752	1,927	1,927	1,927
R^2	0.514	0.459	0.426	0.473	0.437	0.380
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
T & S & O FE	Yes	Yes	Yes	Yes	Yes	Yes
F-test	34.29	44.21	27.44	19.58	22.23	12.94

Notes: Columns 1-3 reports the first-stage results for the SSIV specifications, displaying the estimated effects of the instrumental variable on endogenous variable, namely strong tie, co-worker and employee referrals. Columns 4-6 provide the placebo tests, showing the effects of the instrumental variables in the last period on the current endogenous variable. The full set of controls described in equation (2) is included but not reported. "T & S & O FE" refers to time, state and occupation fixed effects. The use of SSIV results in the loss of one period of observation to construct the growth index. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

2.6 Conclusion

Referral networks play a crucial role in the labor market, as workers benefit from both information transmission and screening mechanisms during the job search and matching process. Using the unique SCE dataset, this paper distinguishes between these two mechanisms. Employing an IV-Heckman approach to control for endogeneity and self-selection biases, we find that weak-tie referrals significantly enhance job-finding probability through the information transmission mechanism and improve matching quality through the screening mechanism. Interestingly, we do not find a significant effect of referrals on starting wages through the information transmission mechanism, which contrasts with previous theoretical research suggesting that referrals increase hiring probability, leading to more job offers and, ultimately, higher reservation wages (Pissarides, 2000). Instead, starting wage improvements are primarily driven by the screening mechanism, with weak-tie referrals having a persistent effect on wage growth due to better job matching.

To further investigate how the screening mechanism operates, we divide weak-tie referrals into co-worker referrals and employee referrals, based on differences in screening ability and reputational cost. Co-worker referrers exhibit higher screening ability but lower reputational cost, while employee referrers have the opposite characteristics. Moreover, we define “noisy signals” based on job seekers’ education levels to analyze how employers balance screening ability and reputational cost. For low-noise signal job seekers, employee referrals improve job-finding probability, matching quality, and starting wages. In contrast, co-worker referrals benefit high-noise signal job seekers. These findings suggest that employers are more likely to trust employee referrals for low-noise signal job seekers, while for high-noise signal job seekers, employers may rely more on co-worker referrals due to their stronger screening ability despite lower reputational cost

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2.8 Appendix

Appendix A: Job Search Methods and Variables

Job-finding regression:

The job-finding regression is based on detailed job search information collected through the Survey of Consumer Expectations (SCE). Respondents were asked two key questions regarding their job search activities. The first question (1) asks, "*What were all the things you have done to look for work during the last 4 weeks?*" to capture a comprehensive range of job search methods. Respondents could select multiple methods from the following list: (i) contacting an employer directly, either online or via email; (ii) contacting an employer in person or through other means; (iii) using an employment agency or career center, including those at schools or universities; (iv) contacting friends or relatives; (v) contacting former co-workers, supervisors, teachers, or business associates; (vi) contacting current employees at other companies; (vii) applying to job postings online; (viii) applying to job openings through other means, such as help wanted ads; (ix) checking union or professional registers; (x) viewing job postings online or elsewhere; and (xi) posting or updating a resume online or through other means.

For the job-finding probability regression, we focus on referrals from friends or relatives (social referrals), former co-workers (co-worker referrals), and current employees at other companies (employee referrals), as these referral types provide insights into informal job search channels. These variables were constructed as binary indicators representing whether the respondent used any of these referral methods during the search period.

Matching quality and starting wage regression:

For the matching quality and starting wage regressions, data are drawn from the Survey of Consumer Expectations (SCE), which provides detailed information on how workers learned about their current job. Specifically, question (2) asks, "*How did you learn about your current job?*" Respondents could select multiple methods from the following list: (i) found through the employer's website; (ii) inquired directly with the employer through other means, including in-person; (iii) found through an employment agency, including conversions from temporary to permanent positions; (iv) referred by a friend or relative; (v) referred by a former co-worker, supervisor, teacher, or business associate; (vi) referred by a current employee at the company; (vii) found through a school/university or government employment or career center; (viii) found through an online job search engine; (ix) found a job opening through other means, including help wanted ads; (x) found through union or professional registers; (xi) contacted by a potential employer, recruiter, or head-hunter; (xii) temporary or part-time job converted into a full-time job; (xiii) within-company promotion or transfer; (xiv) returned to a previous employer, including one where the respondent had a previous internship or similar experience; (xv) began work in the family business; (xvi) other; and (xvii) do not remember.

Similarly, we focus on specific referral types. Three key binary variables are constructed: (1) referrals from friends or relatives (social referrals), (2) referrals from former co-workers, supervisors, or business associates (co-worker referrals), and (3) referrals from current employees at the company (employee referrals). Moreover, to isolate the information transmission mechanism and focus on the signaling role of referrals, we leverage a specific and unique question from the Survey of Consumer Expectations (SCE). Respondents were asked to report how many job offers they received as a direct result of a referral (JH15c), followed by a question (JH15d) that asks for the type of referral: "*How many of these were a referral from (1) a friend or relative, (2) a former co-worker, supervisor, teacher, or business associate, and (3) a current employee at that company?*"

Appendix B: Current wage and wage growth

Table 2.B: The effect of different type referrals on current wage and wage growth

	(1)	(2)
	Current wage	Wage growth
Strong tie	-0.080	-0.065
	(0.057)	(0.060)
Weak tie	0.086***	0.069**
	(0.032)	(0.034)
N	2,755	2,740
R^2	0.5632	0.3809
Control variables	Yes	Yes
T & S & O FE	Yes	Yes
Matched	Yes	Yes

Notes: Columns 1 and 2 present the SSIV-Heckman estimated effects of strong and weak tie referral on current wage and real wage growth, respectively. The full set of controls described in equation (2) is included but not reported. "T & S & O FE" refers to time, state and occupation fixed effects. The use of SSIV results in the loss of one period of observation to construct the growth index. Standard errors are presented in parentheses below the point estimates. $***$ denotes significance at the 1% level, $**$ denotes significance at the 5% level, and $*$ denotes significance at the 10% level.

Appendix C: Robust check for Heckman

Table 2.C: The first and reduce form for Heckman method

	Matching quality		Starting wage	
	(1)	(2)	(3)	(4)
	First	Reduce	First	Reduce
Married status	0.143**	0.008	0.135**	0.015
	(0.059)	(0.007)	(0.059)	(0.010)
N	4,860	2,653	4,927	2,748
R^2	0.2225	0.1330	0.2230	0.3393
Control variables	Yes	Yes	Yes	Yes
T & S & O FE	Yes	Yes	Yes	Yes
F-test	14.23		13.28	

Notes: Columns 1-2 and 3-4 reports the first-stage results for the Heckman specifications, displaying the estimated effects of the instrumental variable on employment probability in matching quality and starting wage regressions, respectively. The full set of controls described in equation (2) is included but not reported. "T & S & O FE" refers to time, state and occupation fixed effects. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

Appendix D: Robust check for SSIV

Table 2.D: The SSIV estimates of first-stage of job finding probability and matching quality

	Job-finding			Matching quality		
	Social	Coworker	Employee	Social	Coworker	Employee
	(1)	(2)	(3)	(4)	(5)	(6)
SS: Social	**0.278****	0.080	-0.080	**0.086**	-0.135	0.034
	(0.116)	(0.107)	(0.097)	**(0.107)**	(0.108)	(0.111)
SS: Coworker	-0.208	**0.533*****	-0.128	-0.092	**0.176****	-0.042
	(0.188)	**(0.187)**	(0.177)	(0.115)	**(0.076)**	(0.110)
SS: Employee	-0.135	-0.048	**0.400*****	0.194	-0.038	**0.311*****
	(0.096)	(0.097)	**(0.093)**	(0.177)	(0.132)	**(0.103)**
N	978	978	978	2,522	2,522	2,522
R^2	0.523	0.332	0.413	0.581	0.501	0.483
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Time and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Other job channel	Yes	Yes	Yes	Yes	Yes	Yes
F-test	75.17	36.38	14.77	Yes	Yes	Yes

Notes: Columns 1-3 reports the first-stage results for the SSIV specifications, displaying the estimated effects of the instrumental variable on endogenous variable, namely strong tie, co-worker and employee referrals. The full set of controls described in equation (2) is included but not reported. “T & S & O FE” refers to time, state and occupation fixed effects. The use of SSIV results in the loss of one period of observation to construct the growth index. Standard errors clustered at the level of the time and industry-occupation are in parentheses. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix E: Effect of using referrals on labor market outcomes

Table 2.E: The effect of different types of referrals on labour outcomes in full sample

	Job-finding		Matching quality		Starting wage	
	PSM	SSIV	Heck	IV-Heck	Heck	IV-Heck
	(1)	(2)	(3)	(4)	(5)	(6)
Weak referral	-0.102**	-0.112**	0.025	0.016	-0.006	-0.035
	(0.049)	(0.049)	(0.021)	(0.023)	(0.073)	(0.077)
Coworker referral	0.127***	0.127***	0.027	0.039**	0.123*	0.152**
	(0.048)	(0.048)	(0.018)	(0.019)	(0.066)	(0.068)
Employee referral	0.143***	0.143***	0.044**	0.038*	0.085	0.082
	(0.053)	(0.053)	(0.020)	(0.022)	(0.073)	(0.076)
imr_Em			-0.097	-0.097	1.337**	1.323**
			(0.109)	(0.109)	(0.645)	(0.645)
imr_S		0.139		-0.039		-0.224
		(0.093)		(0.074)		(0.263)
imr_C		-0.084		0.131*		0.456*
		(0.108)		(0.074)		(0.267)
imr_E		0.020		-0.050		0.053
		(0.084)		(0.085)		(0.320)
N	932	932	2,522	2,522	2,522	2,522
R^2	0.1525	0.1551	0.1276	0.1291	0.1310	0.1325
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Time and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Other job channel	Yes	Yes				
Matched	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns 1 and 2 present the estimated effects of network size and quality on employment probability using linear and IV models, respectively. Columns 3 and 4 display the estimated effects of network size and quality on referral usage probability, also using linear and IV models, respectively. Columns 5 and 6 report the estimated effects of network size and quality on monthly starting wages, using the Heckman and IV-Heckman models, respectively. The instrument for network size is lagged short-term mental health, and for network quality, it is lagged residence proximity. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the wave and industry-occupation are in parentheses. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix F: Signaling game and reputation cost

Simple model:

Remember our model, job seeker with noisy signal s_i according to the following production function:

$$s_i = \theta_i + \varepsilon_i \tag{F.1}$$

where θ_i represents the true productivity, assumed to be normally distributed, with $\theta_i \sim N(\mu_\theta, \sigma_\theta^2)$; and ε_i is the noisy signal, assumed to be normally distributed, with $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$. A job seeker using referrals can reduce the noisy signal of their unobservable productivity through the referrer's screening ability, leading to a reduction in noise to $\frac{\sigma_{\varepsilon,i}^2}{a}$, where $a \in [1, \infty)$. Additionally, to capture the advantage of referrals, we further assume that the noisy signal observed directly by the employer is infinite, meaning $\sigma_\varepsilon^2 \to \infty$. Thus, we have two levels of noise for the job seeker: $s_{i,j} = \{s_{i,F}, s_{i,R}\}$, where $s_{i,F}$ represents the noisy signal without a referral, and $s_{i,R}$ represents the signal with a referral.

In the simple model, we first assume the referrer can take one of two actions. If $U_R \geq 0$, the referrer sends a referral, which includes the message $m = E[\theta | s_{i,R}]$. If $U_R < 0$, the referrer does not send a referral. The referrer's utility is represented as:

$$U_R = I - r \tag{F.2}$$

where I is the fixed payoff for sending the referral, and r represents the reputational cost. When the employer receives the referral, the utility is given by:

$$U_E(r) = y_i = \gamma(r)\big(1+\tau(a)\big)E\big[\theta|s_{i,R}\big] + \big(1-\gamma(r)\big)\mu_\theta \tag{F.3}$$

where $\gamma(r)$ is the weight the employer places on the referral due to reputational cost, and $\tau(a)$ captures the reduction in noise through the referral's screening ability. The first term reflects the employer's belief in the referrer's message, $m = E\big[\theta|s_{i,R}\big]$. If the employer does not trust the referral, they rely on their own observation, but since the noisy signal is infinite, $E\big[\theta|s_{i,F}\big] = \mu_\theta$ as $\sigma^2_{\varepsilon,F} \to \infty$.

Since $E\big[\theta|s_{i,R}\big] = \mu_\theta + \frac{\sigma_\theta^2}{\sigma_\theta^2 + \frac{\sigma_\varepsilon^2}{a}}(s-\mu_\theta)$, the equation (F.3) becomes:

$$y_i = \mu_\theta[1+\tau(a)\gamma(r)] + \big(1+\tau(a)\big)\gamma(r)\left[\frac{a\sigma_\theta^2}{a\sigma_\theta^2+\sigma_\varepsilon^2}(s-\mu_\theta)\right] \tag{F.4}$$

In the case where $s \geq \mu_\theta$, meaning the referrer only recommends job seekers with observed productivity equal to or higher than the average productivity, the firm's profit increases strictly with both the reputational cost and the referrer's screening ability. However, the employer must balance the trade-off between screening ability and reputational cost. If the reputational cost is set too high, $r > I$, the referrer will not recommend any job seeker, and the firm's profit will simply become $E\big[\theta|s_{i,F}\big] = \mu_\theta$ (i.e., job seekers find job only through the formal market, but employer has infinite noisy signal). Since $\tau(a) > 0$, $\gamma(r) > 0$ and $s > \mu_\theta$, it follows that $y_i > \mu_\theta$, leading to employer are more likely to believe the referral market. Moreover, the employer will set the reputational cost $r \in (0, I]$ to incentivize the referrer to recommend job seekers. But since firm's profit strictly increase with r, given by:

$$\frac{\partial y_{i,r}}{\partial r} = \mu_\theta\tau(a) + \big(1+\tau(a)\big)\left[\frac{a\sigma_\theta^2}{a\sigma_\theta^2+\sigma_\varepsilon^2}(s-\mu_\theta)\right] > 0 \tag{F.5}$$

the employer will set $r = I$, , maximizing the referral's credibility. In the case of $s < \mu_\theta$, the opposite occurs, and the employer will not trust or accept any referrals. Overall, in equilibrium, the referrer only recommends job seekers with observed productivity $s \geq \mu_\theta$, and the employer sets $r = I$. The referrer earns zero profit because the firm holds maximum bargaining power.

Reputation cost in Nash bargaining

We now explore the role of reputation cost in the Nash bargaining framework. As previously mentioned, if the referrer has no bargaining power, the employer will set the maximum reputation cost, $\bar{r} = I$, leaving the referrer with zero profit. To set up the Nash bargaining, we firstly need to determine the disagreement utilities are U_R^0 for the referrer and U_E^0 for the employer. Clearly, if the referrer does not recommend the job seeker, they earn zero profit, with $U_R^0 = 0$. On the other hand, if the employer does not receive the referral, their utility is $U_E^0 = \mu_\theta$, as the job seeker would find employment through the formal market. Then, the Nash Bargaining Solution seeks to maximize the product of the utilities' gains over their disagreement points, weighted by the bargaining power parameters:

$$\max_r \left(\gamma(r)\left(\left(1+\tau(a)\right)E\left[\theta|s_{i,R}\right] - \mu_\theta\right)\right)^\beta (I-r)^{1-\beta} \tag{F.6}$$

Differentiating with respect to r and set the optimal condition, we have:

$$r^* = I - \frac{1-\beta}{\beta}\frac{\gamma(r)}{\gamma'(r)} \tag{F.7}$$

The fraction $\frac{1-\beta}{\beta}$ adjusts the r^* based on the bargaining power between the employer and the referrer. If β (i.e., referrer's bargaining power) is high, the referrer will negotiate a lower reputation cost. That is, assuming a linear form for $\gamma(r)$, such that $\gamma(r) = Ar$, we can find $r^* = \beta I$. If $\beta = 1$, meaning the employer has maximum bargaining power, the optimal reputation cost reduces to the previously discussed in equation (F.4) where $r^* = I$.

Advance model: Screening ability and reputation cost

The previous model is simplistic, as it assumes that the referrer sends a fixed message. In reality, the referrer can choose the specific message level to maximize their utility. Additionally, the referrer must balance their screening ability against the reputation cost, leading to the following utility function (the following without subscript):

$$U_R(m, a) = \eta m - rE[(m - \theta)^2 | s_R] - kra \tag{F.8}$$

where $\eta m \geq 0$ represents the benefit the referrer gains from sending a message m to the employer; $r \geq 0$ represents the reputational cost parameter, and $E[(m - \theta)^2 | s_R]$ captures the penalty the referrer faces if the message is imprecise. a represents the screening ability, while kr is the effort required for screening. Higher reputational cost increases the overall effort cost of maintaining or improving screening ability. According to above the utility function, referrer send the optimal message m^*:

$$m^* = E[\theta | s_R] + \frac{\eta}{2r} \tag{F.9}$$

where $E[\theta | s_R] = \mu_\theta + \frac{a\sigma_\theta^2}{a\sigma_\theta^2 + \sigma_\varepsilon^2}(s - \mu_\theta)$. The referrer's message is partly based on the screening ability (a), which reduces the noise (uncertainty) in the employer's estimate of the worker's productivity. The more effective the referrer's screening, the closer the message will be to the job seeker's true productivity. The term $\frac{\eta}{2r}$ refers to how much the referrer can inflate the message. If the reputational cost r is high, the referrer will be more conservative in inflating the message. Conversely, if payoff of referral η is strong, they might overstate the job seeker's productivity.

Substituting the optimal message, m^*, into the utility function, and recognizing that

$E[(m-\theta)^2|s_R] = (m - E[\theta|s_R])^2 + Var(\theta|y)$, we have:

$$U_R(m,a) = \eta\left(E[\theta|s_R] + \frac{\eta}{2r}\right) - r\left(\left(\frac{\eta}{2r}\right)^2 + Var(\theta|y)\right) - kra \tag{F.10}$$

Then, the optimal screening ability can be determined by:

$$a^* = \frac{\sigma_\varepsilon}{\sigma_\theta}\sqrt{\frac{\eta(s-\mu_\theta) + r\sigma_\theta^2}{kr}} - \left(\frac{\sigma_\varepsilon}{\sigma_\theta}\right)^2 \tag{F.11}$$

Indeed, if the referrer receives a higher payoff η, they will invest more in improving their screening ability. Conversely, if the effort cost k of investing in screening ability increases, they will invest less. Interestingly, an increase in reputational cost r can either increase or decrease the optimal screening ability a^*. On one hand, if the penalty for making inaccurate referrals (due to high uncertainty) is severe, referrers will allocate more resources to ensure their referrals are as accurate as possible. However, as r increases, the effort cost k associated with screening also rises, which may discourage the referrer from investing further in screening ability. Overall, differentiating with respect to r, we have:

$$\frac{da^*}{dr} = -\frac{1}{2}\frac{\sigma_\varepsilon}{\sigma_\theta}(B)^{-\frac{1}{2}}\left[\frac{\eta(s-\mu_\theta)}{kr^2}\right] < 0 \tag{F.12}$$

where $B = \frac{\eta(s-\mu_\theta)+r\sigma_\theta^2}{kr}$. This shows that as reputational cost increases, the optimal screening ability tends to decrease, as the negative effect of r on the effort required for screening outweighs the benefits of improving accuracy. Overall, the referrer will send the referrals only in situation of $\eta m \geq rE[(m-\theta)^2|s_R] + kra$. In any case where $r < \bar{r} = \frac{\eta m^*}{E[(m^*-\theta)^2|s_R, a^*] + ka^*}$, the referrer will develop the optimal screening ability a^* and send the optimal message m^* to the employer. Similar to equation (F.3), when employer receive m^*, his utility function becomes:

$$U_E(r) = y_i = \gamma(r)\big(1 + \tau(a)\big)m^* + \big(1 - \gamma(r)\big)\mu_\theta \tag{F.13}$$

We still assume that the employer always trusts the referrer's message, as the employer can punish the referrer for sending an imprecise message. Additionally, we assume the employer only believes the referrer when the reputation cost is sufficiently high. After substituting the optimal message. After substituting the optimal message m^* from equation (F.9), the employer's utility function becomes:

$$y_i = \mu_\theta[1 + \tau(a^*)\gamma(r)] + \big(1 + \tau(a^*)\big)\gamma(r)\left[\frac{a^*\sigma_\theta^2}{a^*\sigma_\theta^2 + \sigma_\varepsilon^2}(s - \mu_\theta) + \frac{\eta}{2r}\right] \tag{F.14}$$

Unlike equation (F.4), equation (F.14) more fully captures the employer's need to balance the reputational cost and the referrer's screening ability. There are two reasons why the employer might set a higher reputational cost. First, increasing the reputational cost reduces the inflation of the optimal message m^*, as the term $\frac{\eta}{2r}$ becomes smaller. Second, a higher reputational cost increases the employer's trust in the referral, with $\gamma(r)$. However, setting a higher reputational cost also discourages the referrer from investing in screening ability, as shown in equation (F.12). As a result, the matching quality function $\tau(a^*)$ will decrease, and the term capture the noisiness of the signal, $\frac{a^*\sigma_\theta^2}{a^*\sigma_\theta^2+\sigma_\varepsilon^2}$, will also decline with reduced screening ability, leading the referrer to provide less accurate referrals. Calculating the exact value of the optimal r^* is complex even we assume $\tau(a) = 0$. After assuming $\tau(a) = 0$, above equation (F.14) becomes:

$$U_E(r) = \left[Ar\frac{a\sigma_\theta^2}{a\sigma_\theta^2 + \sigma_\varepsilon^2}(s - \mu_\theta) + Ar\frac{\eta}{2r}\right] + \mu_\theta \tag{F.15}$$

The optimal condition of r^* can be found in the following equation:

$$2\left(\eta(s-\mu_\theta)+r^*\sigma_\theta^2\right)^{\frac{3}{2}}=\frac{\sigma_\varepsilon}{\sigma_\theta}\left(3\eta(s-\mu_\theta)+2r^*\sigma_\theta^2\right)\sqrt{kr^*} \tag{F.16}$$

The optimal r^* can be found using numerical methods, but it falls outside the scope of this study.

Job seeker's strategy:

In the model, we do not impose any cost for the job seeker to find the job through the referrals. If have, assuming their wage equation to the matching productivity $w_i = y_i$ then their utility becomes:

$$U_J(m^*,y^*)=y^*-(1-\eta)m^* \tag{F.16}$$

Job seeker plays the cut-off strategy, using the referral or successful used referral when $y^* \geq (1-\eta)m^*$.

Chapter 3

The Role of Networks Size and Quality in Labor Market Outcomes

Abstract

Existing research on employed networks primarily focuses on information transmission, with limited attention to signaling mechanisms and the intrinsic value of network capital. In this paper, we address these gaps by investigating both the direct and indirect effects of network size and quality on starting wages. Using an IV-Heckman approach to account for endogeneity and self-selection biases, we find that an increase in network size significantly raises starting wages, both directly and indirectly through higher hiring probability. However, the indirect effect is more pronounced for low-ability job seekers, suggesting they rely more heavily on job information provided by their networks. Additionally, while network quality alone does not increase wages, its interaction with referrals results in significant increase in both starting wages and wage growth. This finding suggests that referrals signal high productivity, but mainly for high-ability job seekers. In occupations like sales and social work, where network capital is highly valued, we find that network capital has a particularly strong effect on starting wages, whereas the effect of human capital is comparatively weak.

3.1 Introduction

In the labor market, one of the most significant challenges is asymmetric information, which leads to search frictions and uncertain matching between job seekers and employers. This problem can be mitigated by "*referral networks*", where employed individuals refer job seekers for open positions. As reported by Holzer (1988), more than 85% of workers in the U.S. rely on informal contacts when searching for jobs. Similarly, a comparable percentage of employers use referral networks to hire their current employees (Miller and Rosenbaum, 1997). The literature identifies two primary mechanisms through which referral networks influence labor market outcomes: information transmission and signaling, affecting both the job search and matching processes. On the searching side, if information about job opportunities is passed from employed workers to job seekers, an increase in network size can directly enhance hiring probability, thereby indirectly boosting wage outcomes (Calvó-Armengol and Zenou, 2005; Ioannides and Loury, 2006; Schmutte, 2016). On the matching side, referral networks reduce uncertainty about job matching through signaling mechanisms. Montgomery (1991) shows that high-type job seekers tend to use referrals from high-type workers to signal their similar unobserved productivity, improving the quality of matches and leading to higher wage outcomes (also see Ekinci, 2016; Galenianos, 2014; Casella and Hanaki, 2008; Horvath, 2014).[45]

Despite the theoretical model of network effects and previous examinations of information transmission on labor market outcomes, there is limited research testing the signaling mechanism. For instance, Cingano and Rosolia (2012) study the displaced networks and provide persuasive evidence that a higher network employment rate significantly shortens unemployment duration, but it does not statistically influence the entry wages of reemployed workers. These results are corroborated by Glitz (2017), who further employs an instrumental variable (IV) method. Likewise, by examining the

[45] Another mechanism of referral is the potential for moral hazard on the referrer's side. Heath (2018) shows that referral providers may face wage penalties if the workers they refer underperform.

displaced specific networks by matched employee-employer dataset, Saygin et al. (2021) show that higher network employment rates increase job-finding rate; however, they also find that higher network employment rates can lead to higher starting wages within high-wage firms. By directly observing network linkages through the survey dataset, Cappellari and Tatsiramos (2015) also find that higher network employment rates increase job-finding rate, but only the employed non-relative linkages increase the re-employment wage of high-skilled workers. Although existing studies provide compelling evidence that employed network size can increase the job-finding or hiring probability, it remains unclear how and why it directly impacts starting wage. Moreover, previous research show that the network size may increase both hiring probability and wage outcome but does not quantify the share of direct and indirect effects.

Empirical studies examining signaling mechanisms often focus on referral usage strategies and the degree of homophily. One the one hand, research on referral usage typically relies on proxies from administrative data or direct observations from survey data. For example, using administrative data, Bayer et al. (2008) show that network members tend to cluster in the same residential and work locations, using this proximity as a proxy for referral usage, which significantly impacts job-finding rate and current earnings. However, these effects may vary by race, as demonstrated by Hellerstein et al. (2011). Dustmann et al. (2016) use ethnic distance as well as shared work histories as a proxy for referrals and find that workers hired through referrals receive higher starting wages, but the variance in wages between referred and non-referred workers decreases over time. These findings are further supported by Brown et al. (2016), who use actual observed referrals (also see Simon and Warner, 1992). Likewise, using observed referrals, Loury (2006) argues that only job seekers who find jobs through high-wage contacts tend to achieve both higher starting wages and wage growth, because of higher performance and lower turnover (Pallais and Sands, 2016). However, Bentolila et al. (2010) find a wage discount for workers who secure jobs through referrals, arguing that job seekers using referrals may prioritize easier job access over the signaling mechanism. Likewise, Kramarz and Skans (2014) focus on family-based networks as a proxy for referral hiring, finding that children of current employees are more likely to be hired, but receive lower starting wage compared to job seekers.

The existing empirical literature on the mechanism of homophily is limited. For instance, Hensvik and Skans (2016), using a matched employee-employer dataset, find that firms with high-ability incumbent workers are more likely to use referrals to hire, and referred workers benefit from higher starting wages, supporting Montgomery's (1991) model. However, focusing solely on homophily may overlook the accumulation of network quality for low-ability job seekers, as they may also be referred by high-ability incumbents.[46] The concept of network quality varies across studies depending on the research focus. Bayer et al. (2008) define network quality as individual's matches with other adults in her block, while Cappellari and Tatsiramos (2015) define it based on the share of friends who are employed. In our framework, we define network quality by the education level of a job seeker's friends, which more directly captures the signaling mechanism outlined by Montgomery (1991), as well as the insights from Casella and Hanaki (2008) and Horvath (2014).

In addition, unlike the extant studies, our analysis systematically focuses on both network size and quality in relation to hiring probability and starting wages, recognizing that low-ability and high-ability job seekers may have different network investment strategies. To explore this, we develop a simple theoretical framework that explains how network size, through information transmission, affects starting wages through hiring probability, while network quality interacts with referrals through signaling mechanisms to reveal high productivity. Additionally, prior research has typically examined referral networks through either information transmission or signaling mechanisms, often overlooking the intrinsic value of network capital. We argue that network capital itself can be a significant form of observed productivity, particularly in sales and social occupations where network capital may directly contribute to a firm's profits and may even surpass the importance of human capital (Podolny and Baron, 1997).

In the subsequent empirical analysis, we estimate the effects of lagged network size,

[46] Although Montgomery's (1991) theoretical model suggests that high-ability incumbents absent incentive to refer low-ability job seekers, it fails to account for potential reward mechanisms that may motivate referrals.

and network quality along with its interaction with referrals, on hiring probability, starting wage, and wage growth using longitudinal data from the UK Household Longitudinal Study (UKHLS). The empirical analysis has five main objectives: (i) to assess the impact of network size and quality on the referral used probability and on hiring probability; (ii) to examine the direct and indirect effects of network size on starting wages and calculate the share of the indirect effect through hiring probability; (iii) to investigate the effect of network quality and its interaction with referrals on starting wage and wage growth; (iv) to measure how these network effects differ by the job seeker's skill level; and (v) to explore the relative importance of network capital and human capital across different occupation types, such as sales and social occupations.

A key concern in the empirical analysis of referral networks is the endogeneity of network characteristics with respect to outcomes. If job seekers self-select into networks based on unobserved characteristics, a clear identification strategy is necessary to distinguish causal network effects from correlations among network members' outcomes. To address this, we define referral networks based on former coworkers. These networks are not primarily formed with the objective of sharing job information, which helps isolate peer effects (Cornelissen et al., 2017). Additionally, defining networks in this way helps mitigate reverse causality issues, as the network formation occurred prior to the job search process (Cappellari and Tatsiramos, 2015; Glitz, 2017). However, referral networks may still be shaped by employment histories and are thus not randomly generated with respect to labor market outcomes. For instance, more productive job seekers may be more likely to have stronger network characteristics, which could confer advantages in labor market outcomes. To account for this, we measure both job seekers' comprehensive abilities (such as memory, verbal skills, math, and reading) and key network characteristics, including gender homophily (Beaman et al., 2018; Saygin et al., 2021; Mengel, 2020), weak tie connections (Granovetter, 1973; Kramarz and Skans, 2014; Goel and Lang, 2019), and interaction frequency (Calvó-Armengol and Jackson, 2004).

Moreover, bias may arise from self-selection into employment. According to Calvó-

Armengol and Zenou (2005)'s model, the hiring probability depends both on formal market and employed network size. If job seekers with better networks are more likely to be hired, the wage regression analysis may be biased due to the analysis being conducted on a truncated sample. Additionally, potential bias arising from unobserved variables related to the network characteristics that may affect labor market outcomes, such as bargaining power (Ioannides and Loury, 2006), searching effort (Krueger and Mueller, 2011) and firm characteristics (Abowd et al., 1999). To address both self-selection and endogeneity issues, we employ IV-Heckman methods, using a set of instrumental variables, such as lagged short-term mental health and lagged residence proximity. To demonstrate the exogeneity of these instruments, we conduct a placebo test, showing that the lagged instrumental variables are correlated only with job starting status and not with future labor market outcomes.

First, our IV-Heckman estimates confirm previous findings that employed network members increase hiring probability (Cingano and Rosolia, 2012; Cappellari and Tatsiramos, 2015; Glitz, 2017; Saygin et al., 2021). On average, among UK job seekers, a one unit increases in the size of employed network members (referred to as network size hereafter) indirectly raises starting wages by 6.5% through its effect on hiring probability. However, unlike previous research, we find a stronger direct effect of network size on monthly starting wages, with a one-unit increase in network size resulting in a 5.4% increase in monthly starting wages. Assuming unemployed individuals earn zero wages, the indirect effect accounts for approximately 56.1% of the total effect on starting wages. This indirect effect, however, shows strong heterogeneity between low-ability and high-mid-ability (referred to as high-ability hereafter) job seekers. For low-ability job seekers, the indirect effect constitutes about 86.6% of the total impact, whereas for high-ability job seekers, it accounts for only 32.7%. This suggests that network size plays different roles: for low-ability job seekers, it primarily enhances access to job opportunities, while high-ability job seekers benefit more from direct effects. Interestingly, network quality might not affect or even negative affect on hiring probability, possibly because job seekers with higher-quality networks are more selective and target better opportunities that take longer to secure

(Pissarides, 2000).

In the subsequent empirical analysis, we then estimate the effect of network quality and referral on starting wage and wage growth. Consistent with previous research, we find that referrals alone do not increase starting wages (Bentolila et al., 2010; Kramarz and Skans, 2014), nor does higher network quality by itself. An increase in starting wages occurs only when job seekers with higher network quality also use referrals. In this case, they experience an increase of about 4.8% in their monthly starting wages compared to those with lower network quality. This finding supports the idea that referrals can reveal high productivity through signaling mechanism, but this effect is contingent on the quality of the network (Hensvik and Skans, 2016).[47] Moreover, the signaling mechanism also exhibits strong heterogeneity between low-ability and high-ability job seekers. For low-ability job seekers, the effect is only 0.4%, suggesting that when there is less variance in unobserved productivity, as is the case for low-ability job seekers, the signaling mechanism has a weaker impact. If low-ability job seekers benefit more from network size than from network quality, their optimal network investment is primarily focused on network size. Our data support this argument, showing that the mean difference in network quality between low and high skill job seekers is about 34%, while the mean difference in network size is only about 13%.

Furthermore, unlike previous research, which shows that the use of referrals generally results in lower wage growth over time (Dustmann et al., 2016; Brown et al., 2016; Simon and Warner, 1992) due to the gradual revelation of unobserved productivity, we find a positive and statistically significant interaction effect of network quality and referrals on wage growth, 2.6% increase in wage growth. There are two possible mechanisms for this persistent effect. First, high network quality may amplify peer effects. Second, the network itself may carry intrinsic value, providing a wage premium over time. While there is substantial existing research on peer effects (e.g., Cornelissen et al., 2017; Mas and Moretti, 2009; Falk and Ichino, 2006), we focus on

[47] Unlike to Hensvik and Skans (2016), we focus on the labour supply side, showing how the effect of job seekers with higher network quality also use referrals on the lobour outcomes.

the second mechanism—the premium value of the network itself. This focus is also supported by the observation that high-ability job seekers' starting wage benefit more from the direct effects of network size, a factor that remains underexplored in the literature. Specifically, we find that the effect of network capital (i.e., network size and quality) on starting wages is particularly strong in occupations where employers place a high value on network capital, such as sales and social occupations. In contrast, in manual labor occupations, the network capital effect disappears. Interestingly, we find that employers may balance observable abilities (measured by basic skills such as memory, verbal skills, math, and reading) with unobserved abilities signaled by high-quality referrals when determining starting wages. Excluding specific occupations such as sales, social, and manual labour, we further find that low-ability job seekers receive twice as much of a wage premium for observable abilities compared to high-ability job seekers, who must rely on high-quality referrals to compensate for this difference.

The remainder of the paper is organized as follows. The next section outlines a theoretical framework. Sections III and IV then describe our identification strategy and our data, respectively. Section V reports our results, and Section VI summarizes our findings.

3.2 Theoretical framework

Our model builds on the work of Calvó-Armengol and Zenou (2005), Ioannides and Loury (2006), and Schmutte (2016). Workers and firms populate the economy. A free entry condition determines the measure of firms. Each worker has a market characteristic and a social characteristic. The market characteristic determines the worker's interaction with firms, it is exogenous, and it depends on the worker's type. The social characteristic is the worker's referral network, and it is determined endogenously within the model. There are two distinct stages in the model. In the first stage, workers form a referral network. In the second stage, workers and firms search and produce in a frictional labour market. Further assume that that jobs are homogeneous and that firms are identical.

A. Network Formation

The formation of the referral network is modelled as a non-cooperative game with non-transferable utility. A worker's referral network consists of the measure of links that he has with Low type (L-type) and High type (H-type) co-workers. Let s_i^k denote the measure of links that worker i has with k-type co-workers, where $k \in \{L, H\}$. Define the network quality rate of a worker, ϕ_i:

$$\phi_i = \frac{s_i^H}{s_i^L + s_i^H} \tag{1}$$

Suppose that a worker produces individual output y_i according to the following production function:

$$y_i = a_i(s_i, \phi_i) + \tau\mu R(s_i, u, v)\zeta(\phi_i) \tag{2}$$

where $R(s_i, u, v)$ represents the probability that a job seeker finds a job through a referral. For instance, when a job seeker does not find a job through his network (i.e., $R(s_i, u, v) = 0$), his productivity y_i determined solely by their observed ability $a_i = \gamma\psi(s_i, \phi_i) + (1 - \gamma)p_i$, where observed ability is a combination of network capital ($\psi(s_i, \phi_i)$) and human capital (p_i).[48] Conversely, if the job seeker finds a job through a referral (i.e., $R(s_i, u, v) = 1$), his total productivity y_i depends on both observed ability a_i and unobserved ability $\zeta(\phi_i)$ (signalling by his network quality ϕ_i).[49] The parameter $\tau \in [0,1]$ determines the relative importance $\zeta(\phi_i)$ in contributing to the job seeker's total productive capacity.[50] Additionally, μ captures the tenure effect, reflecting the likelihood that employers perceive referred job seekers

[48] Some industry and occupation, like sales and social work activities, employer may pay them more on the social network capital.

[49] The higher network quality corresponds to higher unobserved ability, with $\frac{d\zeta(\phi_i)}{d\phi_i} > 0$

[50] Importantly, τ refers to the employer's ability to observe the worker's skills directly. In contexts where direct observation of skills is more feasible, such as in low-skill jobs, the employer may rely less on indirect signals like network quality. Several studies, such as Montgomery (1991) as well as Beaman and Magruder (2012), find evidence of τ, showing significant heterogeneity in its effects between low- and high-skill workers.

as having been closely observed by their referrers over an extended period.[51]

The hiring probability through the referral network $R(s_i, u, v)$, is determined by the joint event, including the following components: the probability that his co-worker is employed $1-u$; the probability that his co-worker receives information about a job opening information directly from an employer v; and the probability that the job opening information is transmitted to unemployed individuals ρ. In general, the expression for this joint event is given by:[52]

$$R(s_i, u, v) = \rho v(1-u)s_i + \kappa O \tag{3}$$

where κ is a constant that scales the contribution of other network factors, O, that influence the probability of using referrals. These factors include homophily (Beaman et al., 2018; Saygin et al., 2021; Mengel, 2020), weak tie connections (Granovetter, 1973; Kramarz and Skans, 2014; Goel and Lang, 2019), interaction frequency (Calvó-Armengol and Jackson, 2004), residence proximity (Bayer et al., 2008; Hellerstein et., 2011; Schmutte, 2015), and other network characteristics.

B. Searching and Matching Function

In each period, the hiring probability of job seeker i is given by the sum of three components: the probability of receiving a job offer from the formal market (v), the probability of receiving a referral through indirect effect of network size ($R(s, u, v)$), and the directly effect of network size (s).[53] For clarity and ease of exposition, we omit subscripts i in the following. Specifically, that is expressed as:

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[51] Loury (2006) suggests that longer tenure among workers who found their jobs through referrals may reduce uncertainty about the quality of the match between the worker and employer.

[52] Calvó-Armengol and Zenou (2005) derived the referral probability is $R(s, u, v) = 1 - \left[1 - \frac{\rho(s,u,v)}{s}\right]^s$ where $\rho(s, u, v) = v(1-u)\frac{1-(1-u)^s}{u}$. This formulation recognizes that the probability of receiving information initially increases as the s grows, reaching a unique global maximum at $\overline{s}$, after which it begins to decrease. However, incorporating this complex network structure into our model adds unnecessary complexity, especially as our focus is on both network size and network quality. Additionally, we argue that a simpler approach can effectively replicate the key insights of Calvó-Armengol and Zenou (2005) model for network sizes up to $\overline{s}$.

[53] The inclusion of ϑs reflects the idea that as a worker's network size s increases, they are more likely to be exposed to job opportunities in informal settings. A larger network means more connections across various platforms and events, increasing the chances of hearing about job openings through these channels, even if they are not part of a formal referral.

$$h(s,u,v) = v + (\pi R(s,u,v) + \vartheta s_{co}) \tag{4}$$

where s_{co} represents the employed network size, with $s_{co} = (1-u)s$; ϑ represents coefficient for the directly effect of s on the hiring probability, and π refers to coefficient for the indirectly effect of s on the hiring probability through referral channel. Moreover, there are u unemployed workers, and since each hiring probability is independent across individuals, the rate at which job matches occur in each period is just $uh(s,u,v)$. Consequently, the matching function can be expressed as:[54]

$$m(s,u,v) = u[v + (\pi R(s,u,v) + \vartheta s_{co})] \tag{5}$$

Finally, from Equation (5), we can derive the following expression for the probability $f(s,u,v)$ for firms to fill a vacancy:

$$f(s,u,v) = \frac{m(s,u,v)}{v} = u\left[1 + \frac{1}{v}(\pi R(s,u,v) + \vartheta s_{co})\right] \tag{6}$$

C. The employer's problem

In our economy, firms are identical and offer homogeneous jobs. Employed workers have the productivity equals $y > 0$, as indicated in Equation (2). The wage paid by firms to employed workers is denoted by w.[55] Unfilled positions generate no productive, and firm incur a search cost γ. Each period, there is a probability δ that a job is lost, and r is the discount factor. The job filling rate at the beginning of period t is $f(s,u_{t-1},v_t)$. Let $I_{F,t}$ and $I_{V,t}$ represent the intertemporal profit of a filled job and a vacancy, respectively, at the beginning of period t. Then, the Bellman equations for

[54] Similarly, given by $\frac{dh}{ds} = (1-v)\frac{dR}{ds}$, as $\frac{dR}{ds} > 0$ and $(1-v) > 0$, it follows $\frac{dh}{ds} > 0$.

[55] In the models proposed by Calvó-Armengol and Zenou (2005), as well as Ioannides and Loury (2006), workers undergo a probationary period of one period during which their productivity and wage are represented by y_0 and w_0, respectively, with the specific condition that $y_0 = w_0 = 0$.

these profits are as follows:

$$I_{F,t} = y - w + \frac{1}{1+r}\left[(1-\delta)I_{F,t+1} + \delta I_{V,t+1}\right] \tag{7}$$

$$\begin{aligned} I_{V,t} = -\gamma + \frac{1}{1+r}\Big[&\big(1 - f(s, u_{t-1}, v_t)\big)I_{V,t+1} \\ &+ f(s, u_{t-1}, v_t)\Big((1-\delta)I_{F,t+1} + \delta I_{V,t+1}\Big)\Big] \end{aligned} \tag{8}$$

At steady state, we have $I_{F,t} = I_{F,t+1} = I_F$, $I_{V,t} = I_{V,t+1} = I_V$, $u_{t-1} = u_t$ and $v_{t+1} = v_t$. Following Pissarides (2000), we assume that in equilibrium, the value to firms of posting an additional vacancy is zero. With $I_V = 0$ and Equation (8), we derive:

$$I_F = \frac{(y-w)(1+r)}{r+\delta} \tag{9}$$

Additionally, under the free-condition and using Equation (9), we have:

$$\frac{y-w}{r+\delta} = \frac{1}{1-\delta}\frac{\gamma}{f(s,u,v)} \tag{10}$$

D. The worker's problem

Let $I_{E,t}$ and $I_{U,t}$ denote the intertemporal gains of an employed and an unemployed worker, respectively, at the beginning of period t. When vacancies are posted at the beginning of t, with a hiring probability $h(s, u_{t-1}, v_t)$, the Bellman equations can be expressed as:

$$I_{E,t} = w + \frac{1}{1+r}\left[(1-\delta)I_{E,t+1} + \delta I_{U,t+1}\right] \tag{11}$$

$$I_{U,t} = \frac{1}{1+r}\Big[\big(1 - h(s, u_{t-1}, v_t)\big)I_{U,t+1} + h(s, u_{t-1}, v_t)\Big((1-\delta)I_{E,t+1} + \delta I_{U,t+1}\Big)\Big] \tag{12}$$

Similarly, at steady state, we have $I_{E,t} = I_{E,t+1} = I_E$, $I_{U,t} = I_{U,t+1} = I_U$. The worker's surplus can be obtained by subtracting Equation (12) from Equation (11):

$$I_E - I_U = \frac{1+r}{r + \delta + (1-\delta)h(s,u,v)}w \tag{13}$$

E. Equilibrium wage

Workers and firms bargain over the surplus associated with the match. The wage w is derived from a generalized Nash-bargaining process over the total intertemporal surplus:

$$w = \text{argmax}(I_E - I_U)^{\beta}(I_F - I_V)^{1-\beta} \tag{14}$$

where $0 \leq \beta \leq 1$ is the worker's bargaining power. Given the free-entry condition (i.e., $I_V = 0$), and using the Equations (13), (9), and (10), we obtain the final Equation:

$$w_i = \underbrace{\frac{\beta[r + \delta + (1-\delta)h(s_i)]}{r + \delta + \beta(1-\delta)h(s_i)}}_{\substack{\text{Wage responses to increased} \\ s \text{ enhancing hiring}}} \underbrace{[a_i(s_i, \phi_i) + \tau\mu R(s_i, u, v)\zeta(\phi_i)]}_{\substack{\text{Wage responses to increased } s \text{ enhancing} \\ \text{referral usage, couple with higher } \phi}} \tag{15}$$

The wage outcome reflects three primary mechanisms of network effects. The first mechanism captures the information transmission effect, where a worker's wage responds to the increased hiring probability $h(s)$ as their network size grows. The second mechanism reflects the signaling effect, where the use of referrals $R(s_i)$ signals higher unobserved abilities to employers through improved network quality $\zeta(\phi_i)$, further boosting wages. Additionally, as $a_i = \gamma\psi(s_i, \phi_i) + (1-\gamma)p_i$, the third mechanism captures the intrinsic value of network capital, where network capital directly influences wage outcomes, as employers may consider network-driven capital as part of a job seeker's overall observed productivity. Overall, both network size and quality positively affect the wage (i.e., $\frac{\partial w_i}{\partial s_i} > 0$ and $\frac{\partial w_i}{\partial \phi_i} > 0$; see Appendix A of the formal proof). However, the impact of these channels and mechanisms differs, and the following empirical section will further explore these differences. Nonetheless, in Appendix B, following the assumptions by Galenianos (2021), where the steady-state utility of a worker is $\Lambda_i = uI_{U,t} + (1-u)I_{E,t}$, we show that the optimal network investment for low ability job seekers primarily focuses on $s_{i(t-1)}$, while high ability job seekers must balance both $s_{i(t-1)}$ and $\phi_{i(t-1)}$ to maximize their expected utility.

3.3 Data

This paper mainly uses the UK Household Longitudinal Study (UKHLS, 2023), a panel survey focused on household dynamics, economic well-being, labour market changes, and family life. Wave 1 collected data from around 80,000 individuals in 40,000 households. We draw on data from waves 1-12, spanning the years 2010/2011 to 2020/2021. Among these waves, only waves 3, 6, 9 contain information pertaining to social network information. We supplement the UKHLS with information from the PASS-ADIAB and MCSUI (Multi-City Study of Urban Inequality) dataset.[56] PASS-ADIAB comprises annual surveys of around 12,000 individuals from 2007 to 2021, offering longitudinal information on employment histories, earnings, social benefits. MCSUI dataset is a comprehensive survey conducted in the early 1992-1994, designed to examine labor market disparities, social mobility, and urban poverty across four major U.S. cities. While we do not detail the variable quantification and empirical analysis from PASS-ADIAB and MCSUI in the main text, further details are provided in Appendix C. To the best of our knowledge, there are only three survey datasets that

[56] The PASS-ADIAB dataset refers to the "Panel Study Labour Market and Social Security (PASS)" linked with "Administrative Data of the Institute for Employment Research" (ADIAB).

collect information on network characteristics, specifically asking about the education level of friends. Overall, we constructed our sample by retaining both men and women aged between 16 and 60, who are not students and self-employed in all datasets.

A. Identify for starting wage

Workers' starting wages are identified under two conditions. First, starting wages include those who were unemployed in the previous period but are employed in the current period (i.e., unemployed to employed). Second, for those engaged in on-the-job searches (employed to employed), starting wages are identified when workers report changes in their employer, occupation, or industry. If any of these conditions differ from the previous wave, the wage is classified as a starting wage, indicating a job change. On average, between 2010 and 2020, the overall mean monthly starting wage across all workers is 2,323.8£ (see Table 3.1). However, there is significant variation by skill level. High-skill workers report a higher mean monthly starting wage of 2,728.6£, while low-skill workers have a much lower mean monthly starting wage of 1,340.2£.

B. Identify for network size and quality

To identify network size s_{it}, we utilize the following survey question from the UKHLS: "*How many close friends do you have?*" ($n_{i(t-1)}$). However, in our theoretical framework, $s_{i(t-1)}$ represents the employed network size. To calculate it, we use another survey question "*Proportion of friends who have a job*" ($c_{i(t-1)}$). That is, we derive $s_{i(t-1)}$ by multiplying the total number of close friends $n_{i(t-1)}$ by the proportion of those friends who are employed $c_{i(t-1)}$, resulting in $s_{i(t-1)} = n_{i(t-1)} \times c_{i(t-1)}$.[57] In terms of network quality $\phi_{i(t-1)}$, we rely on the survey question "*Proportion of friends with a similar level of education*" and the individual's own education level. First, we categorize the individual's education into two levels: low and high, depending on

[57] Cappellari and Tatsiramous (2015) have previously quantified network quality by using friends' employment status as a measure. In our framework, we argue that friends' employment status belongs to the category of network size, as also suggested by numerous studies in the literature (Calvó-Armengol and Zenou, 2005; Cingano and Rosolia, 2012; Ioannides and Soetevent, 2006). Furthermore, while Cappellari and Tatsiramos (2015) identify network quality based on the "*three closest unemployed/employed friends*", which corresponds to network size in our framework, this approach may not fully capture the broader scope of network size.

whether their education level is below or at least at the undergraduate level. Then, a higher network quality is assigned when a low (high) education individual has a lower (higher) proportion of friends with a similar education level. However, this method presents certain challenges, as our sample generally categorizes education into four levels: before Secondary, Secondary, College or Undergraduate, and Postgraduate. For example, a worker with only Secondary education who reports lower education homophily may have a network including individuals with qualifications below Secondary. To enhance the robustness of our approach, we cross-validate the worker's network quality using the PASS-ADIAB and MCSUI dataset, which provides a more precise measure by directly asking each of the worker's three friends about their education levels: "*Has this person completed a degree at a university or advanced technical college?*" (PASS-ADIAB) and "*What level of education has this person completed?*" (MCSUI).[58] For further details on how the network variables are identified, see Appendix C.

The results from three datasets are consistent, with network quality scores of 0.52 in the UKHLS, and 0.54 in the PASS-ADIAB, and 0.57 in the MCSUI (see Table 3.1 and Appendix C). Furthermore, in Appendix C, we examine the effect of network quality on hiring probability and starting wages using the PASS-ADIAB and MCSUI datasets, finding that the coefficient sizes are similar to those in the UKHLS. Therefore, we argue that our identification of network characteristics remains valid. Nonetheless, as previously discussed, the social network survey is conducted only every three years. Given the persistence of social networks, we assume that the network information for each worker remains relatively stable from one year to the next.[59]

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[58] The cut-off value for assigning high network quality is set at the Secondary level, as the MSSUI dataset covers the years 1992-1994. This approach is reasonable, given that educational attainment in the UKHLS is more skewed toward undergraduate education, while in the MSSUI dataset, Secondary education is more prevalent, as discussed in detail in Appendix C.

[59] This assumption is reasonable, as previous research, including Cingano and Rosolia (2012), Glitz (2017), and Saygin et al. (2021), typically assumes that network information remains constant over a period of up to five years by using the administration dataset. Moreover, including this assumption does not affect our results, as we do not impute network values prior to the given year. This is crucial, as imputing values from earlier years and using lags could introduce issues by potentially capturing the current effect of the network rather than its past influence. However, under our imputation method, the lagged network value still reflects its impact on the current job search process and next starting wage.

C. Identify for referral, instrument variables and other network variables

To identify the use of referrals, R_{it}, we initially rely on the variable "*looked for work in the last 4 weeks: asked friends or contacts*" in each wave. However, the limited number of respondents who answered this question necessitates the use of an additional proxy: the variable "*importance of friendly colleagues when looking for a job*".[60] Although this question is only asked in wave 4, we use it to infer referral use by assuming that the importance placed on having friendly colleagues when searching for a job remains consistent over time. This assumption allows us to extend the proxy's applicability across other waves, ensuring we have enough sample power to combine the network information for our empirical analysis. As Table 3.1 shown, the about 75% of job seeker find the job through the referral, which similar to Holzer (1988) finding that more than 85% of workers use informal contacts when searching for jobs in US.[61]

Moreover, as previously discussed, we use an instrumental variable ($G_{i(t-1)}$) in the Heckman model, and two additional instruments ($M_{i(t-1)}$ and $L_{i(t-1)}$) in the IV method. For $G_{i(t-1)}$ and $L_{i(t-1)}$, we use detailed information from the UKHLS, which asks each participant about three of their closest friends about "*sex of friend*" and "*how far they live from the participant*". Based on this information, we construct indices to represent gender homophily and residence proximity. As Table 3.1 displayed, on average, the gender homophily index is 0.857, indicating that job seekers tend to have a high degree of same-gender friendships. For residence proximity, the overall average is 0.505, reflecting moderate geographical closeness between participants and their closest friends. High-mid skill job seekers report higher averages for both gender homophily and residence proximity compared to their low skill job seekers, suggesting that high-mid skill job seekers tend to exhibit stronger preferences for homophily.

For $M_{i(t-1)}$, we use the SF-12 Mental Component Summary, a standardized measure

[60] The variable can be used as a proxy for referrals because it reflects the value individuals place on having supportive social connections in their job search process. Workers who consider friendly colleagues important are likely to utilize their social networks more actively. This behavior aligns with the concept of using referrals, as both rely on leveraging social relationships to facilitate employment opportunities (Granovetter, 1973).

[61] More than 40%-60% of all workers have found their job through their social network in US and EU market (Corcoran et al., 1980; Pellizzari, 2010).

that assesses overall mental health, including emotional well-being, psychological distress, and social functioning. Additionally, we also control for the SF-12 Physical Component Summary to better isolate the effects of mental health on the outcomes of interest. Moreover, as discussed before, other network characteristics $\boldsymbol{N}'_{i(t-1)}$, such as age homophily, weak tie connections, and interaction frequency, may also influence wage outcomes. These variables are identified using a similar approach to that employed for our IVs.

Table 3.1: Summary Statistics

	All		High-mid skill		Low skill		
2010-2020	Mean	SD	Mean	SD	Mean	SD	Diff.
Dependence							
Starting wage	2323.8	1575.0	2728.6	1622.3	1340.2	864.9	1388.4***
Employment prob.	.81	.38	.89	.31	.76	.42	0.13***
Referral used Prob.	.75	.43	.77	.41	.73	.44	0.04***
Main independence							
Network size	4.28	3.42	4.50	3.43	3.77	3.35	0.73***
Network quality	.520	.499	.568	.495	.40	.49	0.168***
Person Chara.							
Age	38.2	10.3	38.9	9.94	36.6	11.2	2.3***
Gender	.42	49	.41	.49	.44	.49	-0.03**
Married Status	.70	.45	.73	.43	.62	.48	0.11***
Num. Children	.82	1.02	.81	.99	.86	1.07	-0.05*
Family income	4976.6	2919.3	5469.3	2882.7	3779.7	2649.5	1689.6***
Working hours	38.2	12.4	39.6	11.4	34.6	14.0	5.0***
Other Network Chara.							
Age homophily (1-4)	2.05	.90	2.03	.87	2.11	.96	-0.08***
Gender homophily	.857	.211	.860	.208	.849	.218	0.011***
Strong tie connections	.106	.198	.101	.188	.119	.219	-0.018***
Interaction frequency	.715	.171	.696	.170	.761	.166	-0.065***
Residence proximity	.505	.160	.525	.156	.456	.161	0.069***
Health condition							
Mental health	48.5	9.2	48.7	9.0	48.2	9.7	0.5**
Physical health	55.2	6.1	55.6	5.8	54.1	6.7	1.5***

NOTE.—Data are sourced from the UKHLS, covering the period from 2010 to 2020. Starting wage refers to the monthly income from the primary job. Network size represents the number of employed individuals in the job seeker's network. Network quality is measured based on the education level of friends. Working hours are reported on a monthly basis, including overtime. Age homophily is coded as 1 if all friends are of similar age, and 4 if they are not. Residence proximity is higher when individuals live closer to their friends. Mental and physical health are assessed using the SF-12 Mental and Physical Component Summary scores. The total sample consists of approximately 7,558 observations in the starting wage analysis, with 5,354 from high-mid skill occupation and 2,204 from low skill occupation. The sample are separated according to occupation skill levels (NS-SEC).

3.4 Empirical Implementation

In our empirical analysis, we aim to estimate the average effect of network size $s_{i(t-1)}$ and network quality $\phi_{i(t-1)}$ on starting wages w_{it} as described in Equation (15) and illustrated in Figure 1. There are two main pathways through which these effects operate. First, network size $s_{i(t-1)}$ primarily influences w_{it} indirectly by increasing the hiring probability h_i. A larger $s_{i(t-1)}$ enhances the h_i through indirect effect of using of referrals R_{it} and directly effect, as outlined in Equation (4). Second, $s_{i(t-1)}$ influences the probability of using referrals R_{it}, which in conjunction with network quality $\phi_{i(t-1)}$, affects the w_{it}.

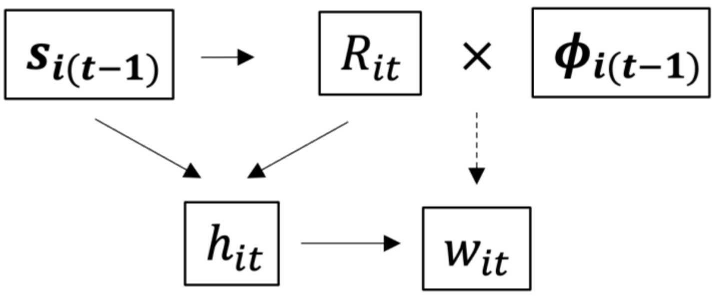

FIG. 3.1: Theoretical Framework for Network Effects on Wages

A. Baseline Specification and Identification

The analysis is conducted using a log-linear model, in which the natural logarithm of monthly starting wages is regressed to understand the causal effect of network size and network quality. The job seeker's network size and network quality at time $t-1$ are included in the model for three key reasons. First, when job seekers secure a job, the networks they leveraged during their job search were established at time $t-1$. Second, using network variables from period $t-1$ helps avoid capturing contemporaneous peer effects, such as peer ability and peer pressure, as discussed in

Cornelissen et al. (2017). Third, from a causal inference perspective, using lagged network size and network quality reduces the likelihood of reverse causality, where starting wages at time t are less likely be affected by network characteristics at time t. Overall, we estimate the following baseline wage:

$$\ln w_{itop} = \alpha_0 + \gamma s_{i(t-1)op} + \varsigma \phi_{i(t-1)op} + \boldsymbol{X}'_{itop}\eta + \varphi_t + \tau_o + \mu_p + \varepsilon_{itop} \tag{16}$$

where $\ln w_{itop}$ is the monthly starting wage of worker i at occupation o and industry p at time t; $s_{i(t-1)op}$ is a continuous variable representing the job seeker's employed network size at time $t-1$ (referred to as network size hereafter), and $\phi_{i(t-1)op}$ is a binary variable indicating whether the worker has high or low network quality at time $t-1$; $\boldsymbol{X}'_{itop}$ is a vector including a set of control variables, such as age, age square, gender, education, marital status, number of children, family income, and working hours at time t. We also include the occupation and industry fixed effect τ_o and μ_p to control the occupation and industry specific unobserved factors. For instance, employers might offer higher wages for sales positions due to the value placed on extensive networks in that occupation. The time fixed effects φ_t control for year variation in the outcome.[62]

Another result from our theoretical model indicates that the wage responses to increased $s_{i(t-1)}$ enhancing the referral used, couple with $\phi_{i(t-1)}$ (also see Equation (15) and Figure 1). To capture above effects, we incorporate the interaction term between network quality and referrals into the baseline regression (16):

$$\begin{aligned}\ln w_{itop} = \alpha_0 + \gamma s_{i(t-1)op} + \varsigma \phi_{i(t-1)op} + \chi R_{itop} \\ + \lambda\left(R_{itop} \times \phi_{i(t-1)op}\right) + \boldsymbol{X}'_{itop}\eta + \varphi_t + \tau_o + \mu_p + \varepsilon_{itop}\end{aligned} \tag{17}$$

[62] There are two reasons we do not use individual fixed effects. First, network information is only collected every three waves. Second, network variance within individuals over time is minimal, similar to preferences and social norms.

The effect of network quality is primarily captured by $\varsigma + \lambda$. However, since we argue that the impact of network quality operates only through referrals, we expect ς to become insignificant once the interaction term is included.

There are four principal concerns regarding the identification of the causal effect of referral networks on starting wages. First, bias may arise from differences in workers' productivity levels. For instance, more productive workers may be more likely to have stronger network characteristics, which could, in turn, lead to higher wage outcomes. Since the network variables are only collected in three waves in our sample (as explained in the following section), we lack individual fixed effects to control for time-invariant productivity. However, our sample includes measures of participants' abilities related to memory, verbal skills, math, and reading. To account for the multidimensional aspects of abilities and avoiding multicollinearity, we employ factor analysis to construct a composite ability factor that serves as a proxy for the worker's observable productivity levels. This factor is then included in $\boldsymbol{X}'_{itop}$ as a control variable to help isolate the network effect from productivity.[63] Second, other network characteristics may be related to both $s_{i(t-1)op}$ and $\phi_{i(t-1)op}$, potentially affecting wage outcomes. These characteristics include age homophily, weak tie connections, and interaction frequency, capturing by O in Equation (3). Similarly, we construct a vector $\boldsymbol{N}'_{i(t-1)op}$, and add it as a set of the control variables.

B. Heckman correction and mediation effect

Third, bias may arise from self-selection into employment. As shown in Equation (4), the hiring probability depends both on formal market and $s_{i(t-1)}$. If job seekers with better networks are more likely to be hired, the wage regression analysis may be biased due to the analysis being conducted on a truncated sample. To rectify this self-selection,

[63] The ability questions are only collected in wave 3; however, similar to the common assumption that productivity is likely to remain constant over time, we can reasonably assume that these ability measures representative of the worker's underlying productivity across the observed periods. Despite this, using this approach still leaves 11.7% of observations without any information, as these participants did not participate in wave 3. To address this issue, we follow the method used by Carruthers and Wanamaker (2017), specifying that the productivity levels for the other 88.3% of participants can be modeled as a function of a 5th-order polynomial. We then use the parameter estimates from this model to impute the missing values.

we use a Heckman correction (Heckman et al., 1997), with "*lagged value of Gender homophily*" ($G_{i(t-1)}$), as the relevant and exclusion restriction based on its impact on labor force participation but not on starting wage. More specifically, the variable captures the degree of gender similarity within a job seeker's network. Connections within similar groups often share relevant job information and facilitate job searching. However, homophily in a prior period ($t-1$) is unlikely to directly affect wage outcomes in period t, because current starting wages are primarily determined by current productivity. Most previous studies also show that homophily tends to increase referral usage and hiring probability (McPherson et al., 2001; Mengel, 2020; Beaman et al., 2018), which in turn leads to more job offers and, consequently, a higher reservation wage, resulting in better wage outcomes (Pissarides, 2000). Besides, to confirm our instrument variable validity, we use a reduced-form approach to directly test the exclusion restrictions on $G_{i(t-1)}$, and we do not expect them to have any statistically significant direct effect on w_{it} (see Appendix F).[64]

Applying the Heckman model typically involves the following two stages. In the first stage, a Probit model predicts the likelihood of employment ($employ_{it}$) for individual i at time t, formulated as:

$$\begin{aligned} Em_{it} = \alpha_0 + \vartheta s_{i(t-1)} + \theta\phi_{i(t-1)} + \pi R_{it} + \psi\big(R_{itop} \times \phi_{i(t-1)}\big) \\ +\zeta G_{i(t-1)} + \boldsymbol{X}'_{it}\eta + \boldsymbol{N}'_{i(t-1)}\upsilon + \varphi_t + \varepsilon_{it} \end{aligned} \tag{18}$$

where $G_{i(t-1)}$ indicate the gender homophily at time $t-1$. Following this, we calculate the inverse Mills ratios $imrE_{it}$ and incorporate them into the baseline regression model (17) to address self-selection problem. Additionally, the first-stage regression allows us to examine the effect of $s_{i(t-1)}$ and $\phi_{i(t-1)}$ on hiring or

[64] The reduced form in the Heckman model differs from that of the instrumental variable (IV) method. In the IV model, we expect the instrument z to be correlated with the outcome y through the endogenous variable, with $cov(y,z) \neq 0$, but z must be uncorrelated with the error term, with $cov(\varepsilon,z) = 0$. In contrast, the Heckman model addresses selection bias by assuming the instrument z is unrelated to the outcome but influences selection into the sample. This leads to the assumption $cov(y,z) = 0$ in the reduced form for the Heckman model, which simplifies the process of checking instrument validity.

employment probability (referred to as hiring probability hereafter). As displayed in Figure 1 (also discussed in Equations 4 and 15), it is evident the Em_{it} responses to increased $s_{i(t-1)}$ by enhancing the R_{it}. Therefore, it is necessary to examine the effect of $s_{i(t-1)}$ on R_{it} by a Probit model:

$$R_{it} = \alpha_0 + \rho s_{i(t-1)} + \xi \phi_{i(t-1)} + \boldsymbol{X}'_{it}\eta + \boldsymbol{N}'_{i(t-1)}\upsilon + \varphi_t + \varepsilon_{it} \tag{19}$$

Then, the total effect of $s_{i(t-1)}$ on Em_{it} is $\vartheta + \pi\rho$. Moreover, Figure 1 also indicates that $s_{i(t-1)}$ may influence $\ln w_{it}$ through hiring probability, suggesting that Em_{it} acts as a potential mediator. However, applying the traditional mediation model is challenging in our context since our mediator (Em_{it}) is conditionally observed (i.e., wage outcomes are only recorded when $Em_{it} = 1$). To identify the mediation effect, we can assume that unemployed workers earn zero. Then, the wage difference between employed and unemployed workers is essentially the mean wage of those employed, denoted as $\overline{w}$. Consequently, the indirect effect simplifies to $(\vartheta + \pi\rho)\overline{w}$, while the direct effect on wage outcome is simply captured by γ. Then, the shared of indirect effect of $s_{i(t-1)}$ on starting wage becomes $S = \frac{(\vartheta+\pi\rho)\overline{w}}{\gamma+(\vartheta+\pi\rho)\overline{w}}$. A formal proof is provided in Appendix D.

Nonetheless, as Figure 1 and Equation (15) shown, we are also interested in the indirect effect of $s_{i(t-1)}$ on the interaction term between R_{it} and $\phi_{i(t-1)}$ on w_{it}, which can be quantified as $\text{indirect} = \rho \times \lambda$.

C. Instrument variable

So far, we have discussed the identification strategy for examining the effect of network size and network quality on referral used probability, employment probability, and monthly starting wage. However, to ensure the robustness of our results, we have to address one more issue: the potential bias arising from unobserved variables related to the $s_{i(t-1)}$ and $\phi_{i(t-1)}$ that may affect labor outcomes, such as bargaining power (Calvó-Armengol and Zenou, 2005), searching effort (Krueger and Mueller, 2011) and firm characteristics (Abowd et al., 1999). For example, our model shows that the wage response to a job seeker's $s_{i(t-1)}$ and $\phi_{i(t-1)}$ may further increase when job seekers have greater bargaining power (see Equation 15). Additionally, the intensity and strategy of a job seeker's effort, such as how actively they indirectly seek referrals or directly engage with potential employers, may go unmeasured but can influence our analysis. Furthermore, firms with strong referral systems or a collaborative work culture may offer higher starting wages to job seekers with extensive networks. To address these possible endogeneity issues, we use the instrumental variable (IV) method, specifically a control function (CF) approach.[65]

The instrument variables used for $s_{i(t-1)}$ are the "*lagged value of short-term mental health*" ($M_{i(t-1)}$). Specifically, job seekers with better mental health may be more socially active, able to sustain stronger relationships, and participate in broader social networks, leading to a larger network size $s_{i(t-1)}$ in the contemporaneous period. However, short-term mental health, especially when measured in a prior period $t-1$, is not directly related to the referral used, employment probability, and wage outcome in period t. Although some studies suggest that workers with better mental health may actively seek jobs or have longer employment durations (Butterworth et al., 2011), as well as experience higher rates of absenteeism and presenteeism (Bubonya et al., 2017), these effects primarily reflect the contemporaneous period. It is unlikely that short-term mental health from the previous year would significantly influence the decision to find or quit a job, or the use of referrals in the current year. Moreover, starting wages are typically determined by current productivity, skills, and labor market conditions, rather than past short-term mental health. Moreover, to further ensure that our instrument variable is valid, we control the physical health, past physical health variables and drop those job seekers with long-term health conditions, as these conditions in period $t-1$ may influence wage outcomes in period t.

[65] Give that one of our potential endogenous variables, $\phi_{i(t-1)}$, are binary, applying a linear IV model may result in what is known "forbidden regression". Appendix E demonstrates that using a linear IV model significantly overestimates the effect size when $\phi_{i(t-1)}$ is treated as an endogenous variable. In contrast, when the endogenous variable is continuous, such as $s_{i(t-1)}$), the linear IV model produces results comparable to those obtained using the CF method. This highlights the particular advantage of the CF method in context of dummy variables, as it allows for the use of Probit models in the first stage (see Wooldridge, 2015, for more details).

Another instrument variables for $\phi_{i(t-1)}$ is "*lagged value of residential proximity*" ($L_{i(t-1)}$), which measures the physical distance between a job seeker's residence and that of their friends within their network. Bayer et al. (2008) provide partial support for our instrument $L_{i(t-1)}$, demonstrating that residing on the same block increases the probability of working together, potentially facilitating the use of high-quality referral mechanisms. Building on this finding, they further construct a measure of matching quality, or referral quality, to examine its impact on labor market outcomes (also see Hellerstein et al., 2011). Their results suggest that $L_{i(t-1)}$ may influence $\phi_{i(t-1)}$ but does not directly affect w_{it} ; instead, its effect on wages is fully mediated through $\phi_{i(t-1)}$. However, according to Bayer et al. (2008), $L_{i(t-1)}$ may have a direct effect on R_{it}. Although they do not discuss the case of lagged values, we still refrain from considering it a valid instrument for $s_{i(t-1)}$, as its potential direct influence on the referral process undermines its exogeneity in this context. Additionally, in our theoretical framework, we do not expect $\phi_{i(t-1)}$ to have any effect on either R_{it} or Em_{it}.

Overall, in the context of our study, we assume that the $M_{i(t-1)}$ remains a valid instrument for effect of $s_{i(t-1)}$ on R_{it}, Em_{it} and w_{it}, while $L_{i(t-1)}$ remains a valid instrument for effect of $\phi_{i(t-1)}$ only on w_{it}. We also conduct a series of tests, including first-stage, reduce form, and placebo tests, to demonstrate that they serve as a valid instrument variable in the subsequent analyses (see Tables 3.4 and Appendix F). Overall, in the first stage, a linear OLS model is used to predict $s_{i(t-1)}$, while a Logit model predicts $\phi_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$, respectively, formulated as:[66]

[66] Here, we assume R_{it} it is exogenous for two main reasons. First, we argue that the arrival of referrals is exogenous after controlling for network characteristics, as job seekers use their network characteristics to request referrals, and the arrival of referrals then depends on their co-workers, which is beyond the job seekers' control and akin to a random shock. Second, since both $s_{i(t-1)}$ and $\phi_{i(t-1)}$ may affect R_{it}, and R_{it} may act as a mediator interacting with $\phi_{i(t-1)}$ to influence the starting wage, we face a challenge in addressing this issue. Although Dippel and Ferrara (2020) have developed similar causal mediation models, we still lack a comprehensive literature review to address the interaction of mediators with treatment variables. However, exploring this further is beyond the scope of our study.

$$
\begin{aligned}
y^j_{i(t-1)op} = {} & \alpha_0 + \alpha_1 M_{i(t-1)op} + \alpha_2 L_{i(t-1)op} + \alpha_3 R_{itop} \\
& + \alpha_4 \left(L_{i(t-1)op} \times R_{itop} \right) + \boldsymbol{X}'_{itop} \eta + \boldsymbol{N}'_{i(t-1)op} \upsilon + \varphi_t + \tau_o \\
& + \mu_p + \varepsilon_{itop}
\end{aligned} \tag{20}
$$

where $y^j_{i(t-1)op}$ is either $s_{i(t-1)}$, $\phi_{i(t-1)}$ or $\phi_{i(t-1)} \times R_{it}$. Following this, we calculate a residuals *resi_s*, as well as two Inverse Mills ratio *imr_*ϕ and *resi_*ϕR, and incorporate them into the Equation (17) to address endogeneity problem for the wage outcome regression.

3.5 Empirical Results

This section examines the effect of network size and quality on labour outcomes, proceeding in six steps. First, a naïve baseline model is used to explore the association between network size, quality, and monthly starting wages. Second, we address self-selection and employment issues by applying the IV-Heckman. Also, we calculate the indirect effect through the mediation variable, employment probability. In the third step, we conduct a series of robustness checks, including first-stage, reduced-form, and placebo tests, to further validate our instrumental variables. The fourth step investigates the potential mechanisms, specifically the relative importance of observed versus unobserved productivity. Additionally, we provide evidence that employers in certain occupations may place greater emphasis on social capital rather than human capital. Lastly, we conclude with a heterogeneity analysis.

A. Baseline results

Table 3.2 presents the results from an OLS analysis examining the association of lagged network size $s_{i(t-1)}$ and network quality $\phi_{i(t-1)}$ on the natural logarithm of monthly starting wages (w_{it}). As shown in column 1, both $s_{i(t-1)}$ and $\phi_{i(t-1)}$ have statistically and economically significant effects on the w_{it}. The coefficient for $s_{i(t-1)}$ is 0.005 log points (p<0.05), and $\phi_{i(t-1)}$ is 0.019 log points (p<0.1) after controlling for basic variables, as well as year, industry, and occupation fixed effects. This suggests that a one-unit increase in network size at time $t-1$ is associated with an approximately 0.5% increase in the monthly starting wage at time t. Similarly, job seekers with a higher quality network in the previous period experience a 1.9% increase in their starting wage (p<0.1) in the current period, compared to those with a lower-quality network. When job seekers' $\boldsymbol{N}'_{i(t-1)}$ and productivity levels are taken into account, the positive effect of $s_{i(t-1)}$ on w_{it} remains consistent (see columns 2 and 3). In contrast, the effect of $\phi_{i(t-1)}$ on w_{it} becomes statistically insignificant.

Nonetheless, in our theoretical model and farmwork (see Equation 15 and Figure 1), we expect the network quality only affect the starting wage through the referral channel. This means that employers may infer higher unobserved productivity from job seekers referred through higher-quality networks. To test this assumption, we include an interaction term, $\phi_{i(t-1)} \times R_{it}$, which represents job seekers with higher network quality in the previous period and who used referrals in the current period, to examine whether this combination affects the starting wage in the current period (also see Equation 17). As shown in column 4, the coefficient for the interaction term is 0.047 log points (p<0.01), indicating that job seekers with higher $\phi_{i(t-1)}$ also use referrals experience a 4.7% increase in their starting wage, compared to those with lower $\phi_{i(t-1)}$.

As previously discussed, the identification of network quality may be subject to bias due to challenges in categorizing education levels. To further validate the robustness of our findings, we leverage the PASS-ADIAB dataset to re-estimate the effect of network quality on starting wages, with results presented in Appendix C. As shown in columns 3 and 4 of Appendix Table C, using the PASS-ADIAB sample, we find that the effects of network size and quality on starting wages are approximately 0.7% (p<0.05) and 5.3% (p<0.01), respectively. These coefficient sizes are consistent with those obtained from our baseline model using UKHLS data, providing additional support for the robustness of our identification strategy in estimating the effects of network size and quality.

Table 3.2: The effect of network size and quality on the monthly starting wage

	(1) Person & occupation, industry and time FE	(2) Plus lag of other network factors	(3) Plus worker's productivity factor	(4) Plus interaction term
$s_{i(t-1)}$	0.005**	0.005**	0.005**	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
$\phi_{i(t-1)}$	0.019*	0.017	0.017	-0.020
	(0.010)	(0.010)	(0.011)	(0.017)
$\phi_{i(t-1)} \times R_{it}$				0.047***
				(0.009)
N	7,558	7,558	7,558	7,558
R^2	0.7007	0.7017	0.7023	0.7025
Basic controls	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	-	Yes	Yes	Yes
Productivity	-	-	Yes	Yes

Notes: Table shows association of network size and quality on monthly starting wage by using linear OLS regression. Columns 1 show a simple OLS regression on the endogenous variables with basic controls (e.g., age, age square, gender, education, and marital status, number of children, family income, regular working hours, and overtime working hours), along with occupation, industry and time FE. Columns 2 and 3 controls the other network characteristics $\boldsymbol{N}'_{i(t-1)}$ (e.g., weak tie connections, interaction frequency, and income homophily) and productivity levels (e.g., measuring by the multiply ability skills). Column 4 includes an interaction term between network quality and referral. The referral variable is added but is not reported. Standard errors clustered at the level of the time and industry-occupation are in parentheses. $***$ denotes significance at the 1% level, $**$ denotes significance at the 5% level, and $*$ denotes significance at the 10% level.

B. Heckman-IV estimation

This section has three main objectives: to address the self-selection issue, endogeneity issue, and to calculate the mediation effect. We begin to examine whether the $s_{i(t-1)}$ have indirectly effect on starting wage through its effect hiring probability or employment probability (Em_{it}). As shown in column 1 of Table 3.3, by examining in the OLS model, a one-unit increase in $s_{i(t-1)}$ statistically significantly increases the Em_{it} by about 0.2 percentage points (p<0.01), after controlling for all covariates and using the fixed effects. This result may underestimate because of endogeneity issue. For examine the casual effect of $s_{i(t-1)}$ on the hiring probability, we use the lag value of short-term mental health as the instrument variables, and the result shown in column 2.

Specifically, our IV estimate show that a one-unit increase in $s_{i(t-1)}$ statistically significantly increases the hiring probability by about 6.2 percentage points (p<0.01). This finding aligns with previous empirical research, as a larger employed network size has been shown to increase employment probability (Cingano and Rosolia, 2012; Cappellari and Tatsiramos, 2015; Glitz, 2017; Saygin et al., 2021). However, contrary to our theoretical model's prediction that $\phi_{i(t-1)}$ might not affect hiring probability, job seekers with higher $\phi_{i(t-1)}$ experience a 1.9 percentage points (p<0.01) and 3.1 percentage points decrease (p<0.01) in hiring probability compared to those with lower network quality, using OLS and IV estimates. One potential reason for this unexpected result may be that job seekers with higher network quality are more selective in their job search, targeting higher-quality or better-matched opportunities that may take longer to secure (Pissarides, 2000).

Moreover, our IV estimate shows that using referrals (R_{it}) also positively affects hiring probability, increasing it by 4.8 percentage points (p<0.01) compared to those who without using referrals, as column 2 shown (also see Galenianos, 2014; Brown et al., 2016; Schmutte, 2016). This effect aligns with our theoretical model, which suggests that $s_{i(t-1)}$ may influence hiring probability both directly and indirectly through the use of referrals. To support this hypothesis, we further examine the effect of $s_{i(t-1)}$ and R_{it}, finding that a one-unit increase in $s_{i(t-1)}$ statistically significantly increases the probability of using referral by about 0.5 percentage points (p<0.01) in OLS estimate and 5.1 percentage points (p<0.01) in IV estimate (see columns 3 and 4). Importantly, the significance of the residual ($resi\_s$) from the first-stage suggests the presence of an endogeneity problem in both our Em_{it} and R_{it} analysis (for further robustness, see the next section). Overall, these results confirm that job seekers with a larger $s_{i(t-1)}$ have higher hiring probability both directly and indirectly through the R_{it}. However, the indirect effect on hiring probability is small, with only a 0.25 percentage points increase per one-unit rise in $s_{i(t-1)}$, potentially due to the relatively low joint probability $\pi\eta v$ (see Equations 3 and 4).[67] These findings differ slightly from Calvó-

[67] According to Equations 3 and 4, we have $h(s, u, v) = \pi\rho v s_{co} + \vartheta s_{co} + \pi\kappa O + v$. The first term captures the indirect effect of network size on hiring probability through referrals, the second term captures the direct effect of

Armengol and Zenou (2005), Ioannides and Loury (2006), and Schmutte (2016), who suggest that network size primarily influences referrals, which in turn impacts hiring probability. Interestingly, we find no evidence that $\phi_{i(t-1)}$ increases the use of referrals, which may be due to not distinguishing between low-type and high-type job seekers (see the following section).[68]

Furthermore, as suggested by Equation 15 and supported by prior research, a higher hiring probability may increase job seeker's reservation wage, which in turn raises their wage outcomes (Calvó-Armengol and Zenou, 2005; Ioannides and Loury, 2006; and Schmutte, 2016). This means that higher $s_{i(t-1)}$ can indirectly affect the starting wage through its effect on the Em_{it}. As discussed above, the total indirect effect of $s_{i(t-1)}$ on starting wages through Em_{it} can be calculated using $\vartheta + \pi\rho$, where we have known $\vartheta + \pi\rho = 0.066$.[69] The next step is to examine the direct effects of $s_{i(t-1)}$ and $\phi_{i(t-1)}$ on starting wages. The outcomes presented in columns 5 and 6 indicate that, after correcting for self-selection and endogeneity issues, the coefficient for interaction term ($\phi_{i(t-1)} \times R_{it}$) remains largely unchanged, while effect of $s_{i(t-1)}$ on monthly starting wage increase to 0.054 log points (p<0.01). Similarly, the significance of the inverse Mills ratio (*imr_E*) and residuals (*resi_s*) confirm the presence of self-selection and endogeneity issues in our wage analysis. In contrast, the insignificance of the inverse Mills ratio for $imr\_\phi R$ suggests that the interaction $\phi_{i(t-1)} \times R_{it}$ is exogenous, with a coefficient similar to that estimated in the OLS and Heckman model. Overall, the IV-Heckman estimates show that a one-unit increase in $s_{i(t-1)}$ leads to an approximately 5.4% increase in the monthly starting wage. This finding aligns with Saygin et al. (2021), who demonstrate that a one-percentage-point increase in the share of former co-worker networks leads to a 0.5% increase in wage growth between the last and new job, consistent with our baseline OLS results. Similarly, Cappellari and Tatsiramos (2015) find that high-skilled workers with more non-familial employed

network size, and the third term captures the indirect effect of other factors that influence the use of referrals, which in turn affect hiring probability.

[68] Montgomery (1991) predicted in his theoretical model that high-type (H-type) job seekers are more likely to be referred by H-type co-workers, an empirical result similarly found by Hensvik and Skans (2016).

[69] The direct effect of $s_{i(t-1)}$ on Em_{it} is about 6.6 percentage points, and the indirect effect of $s_{i(t-1)}$ on Em_{it} through R_{it} is about 0.25 percentage points.

friends experience a 6.1% increase in their starting wages. Previous research has discussed how network size can directly impact starting wages through higher hiring probability and, consequently, higher reservation wages. However, in the following, we propose an additional mechanism: network capital itself can be a significant form of observed productivity, particularly in sales and social occupations. Overall, according to the mediation analysis in Appendix D, the indirect effect $s_{i(t-1)}$ constitutes approximately 54.3% of the total effect on starting wages.[70] This substantial indirect effect aligns with our theoretical model, suggesting that network size primarily affects starting wages through its impact on hiring probability.

Additionally, job seekers have higher $\phi_{i(t-1)}$ who use referrals see an increase their monthly starting wages of about 4.8% (p<0.01) compared to those with lower network quality. As discussed in our theoretical framework, higher network quality acts as a signal of greater unobserved productivity and indicates a higher-performing individual. If this holds true, we expect its effect to persist, influencing not only starting wages but also current wages over time. Since using high-quality referrals can signal high productivity, we anticipate that the interaction term ($\phi_{i(t-1)} \times R_{it}$) will have an even greater effect on current wages. To test this argument, we match the information on $\phi_{i(t-1)}$ and R_{it} from the job search period to the working period. We find that the effect of $\phi_{i(t-1)} \times R_{it}$ on the current wage is nearly two times greater than its effect on the starting wage, and 2.6% increase in wage growth (see Appendix G), confirming that the mechanism by which $\phi_{i(t-1)}$ increases starting wages is through signaling unobserved productivity.[71] This result slightly differs from previous studies (Dustmann et al., 2016; Brown et al., 2016; Simon and Warner, 1992), which show that referrals generally lead to lower wage growth over time due to the gradual revelation of unobserved productivity. Our findings highlight the combined effect of referrals and high-quality networks, suggesting that employers may infer higher unobserved

[70] As mentioned earlier, we have $\vartheta + \pi\rho = 0.066$, and mean wage in our sample is 2323.8 (see Table 3.1), and the directly effect of $s_{i(t-1)}$ on starting wage is 129.33 (p<0.05) as shown in Appendix D. Using the formula, $S = \frac{(\vartheta+\pi\rho)\overline{w}}{\gamma+(\vartheta+\pi\rho)\overline{w}}$, the share of indirect effect is calculated to be 54.3%.

[71] Examining the persistent effect of $s_{i(t-1)}$ on current wages is challenging due to endogeneity issues with wage outcomes. Using the same instrument variable poses risks, and we will explain in the following placebo test part.

productivity in job seekers also with high-quality networks. There are three possible mechanisms for this persistent effect. First, high-quality referrals tend to be used by high-productivity workers, whose unobserved productivity becomes more apparent as they continue working (Montgomery, 1991; Hensvik and Skans, 2016). Second, high network quality may amplify peer effects (Cornelissen et al., 2017; Mas and Moretti, 2009; Falk and Ichino, 2006). Third, the network itself may carry intrinsic value, providing a wage premium over time, a concept we will explore in the following sections.

Nonetheless, our theoretical model predicts that $s_{i(t-1)}$ may affect the monthly starting wage through referrals that interact with $\phi_{i(t-1)}$. However, as shown in Table 3.3, this effect is minimal because the effect of $s_{i(t-1)}$ on referral usage is only about 5.1%. There may be many other factors influencing the decision to use referrals, as indicated by Equation (3) and the low R^2 value of 1.8%. Although this effect is small, it remains a potential channel, especially since we do not find a significant direct interaction effect between $s_{i(t-1)}$ and $\phi_{i(t-1)}$ on starting wages (see Appendix I).

Table 3.3: The IV estimates of network size and quality on labour outcomes

	Employment		Referral		Starting wage	
	Linear	IV-2SLS	Linear	IV-2SLS	Heckman	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
main results						
$s_{i(t-1)}$	0.003***	0.062***	0.005***	0.051***	0.005**	0.054***
	(0.000)	(0.007)	(0.001)	(0.011)	(0.002)	(0.012)
$\phi_{i(t-1)}$	-0.019***	-0.031***	-0.003	-0.011**	-0.021	-0.024
	(0.003)	(0.003)	(0.005)	(0.005)	(0.018)	(0.018)
R_{it}	0.071***	0.048***			-0.032*	0.013
	(0.003)	(0.004)			(0.015)	(0.037)
$\phi_{i(t-1)} \times R_{it}$					0.045***	0.048***
					(0.009)	(0.013)
imr_E					0.072	0.122*
					(0.054)	(0.059)
resi_s		0.060***		0.046***		-0.049***
		(0.007)		(0.011)		(0.012)
*imr_*ϕ						0.146
						(0.090)
*imr_*ϕR						0.008

						(0.005)
N	37,109	37,109	37,109	37,109	7,554	7,554
R^2	0.1749	0.1789	0.0159	0.0164	0.7024	0.7031
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
O & I & T FE					Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns 1 and 2 present the estimated effects of network size and quality on employment probability using linear and IV models, respectively. Columns 3 and 4 display the estimated effects of network size and quality on referral usage probability, also using linear and IV models, respectively. Columns 5 and 6 report the estimated effects of network size and quality on monthly starting wages, using the Heckman and IV-Heckman models, respectively. The instrument for network size is lagged short-term mental health, and for network quality, it is lagged residence proximity. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

C. Robust and Placebo tests

The robustness of our results depends on the validity of our instrumental variable in both Heckman and IV methods. To substantiate its validity, we perform tests to ensure that it satisfies two principal assumptions: relevance and exogeneity. In term of in the Heckman method, as discussed before, we use lagged value of gender homophily as the instrument ($G_{i(t-1)}$). The relevance assumption is confirmed through a strong positive correlation between $G_{i(t-1)}$ with Em_{it}, demonstrated by a coefficient of 0.157 (p<0.01) on the Probit scale, as shown in column 5 in Appendix F. Additionally, the F-test results, with values of 269.66, are well above the critical threshold of 10, indicating no evidence of weak instruments. Moreover, our reduced-form equation for Heckman method shows that $G_{i(t-1)}$ are not significantly correlated with starting wage (i.e., $cov(z, y) = 0$), providing some support for the instrument's validity (see column 5 in Appendix F). The significance of imr_E further reinforces the conclusion that our wage regression is affected by self-selection issues (see columns 5 and 6 in Table 3.3).

After validating the instruments in the Heckman method, we now turn to the validation of the instruments in the IV method, specifically the lagged values of short-term mental health ($M_{i(t-1)}$) and residence proximity ($L_{i(t-1)}$). First, we assess the validity of $M_{i(t-1)}$ as an instrument for $s_{i(t-1)}$ in the R_{it} and Em_{it} regressions. As shown in columns 2 and 4 of Appendix F, the strong positive correlation between $M_{i(t-1)}$ and $s_{i(t-1)}$ in the first-stage regressions confirms the instrument's relevance. Furthermore, F-test results that exceed the critical threshold of 10 indicate no issues of weak instruments. As discussed earlier, the reduce-form regression in IV method differs slightly from Heckman method, with $cov(z, y) \neq 0$. As shown in columns 1 and 3 of Appendix F, $M_{i(t-1)}$ are significantly correlated with starting wage.

Unlikely the instruments used in R_{it} and Em_{it} regressions, in the w_{it} regression we introduce additional instrument $L_{i(t-1)}$ for $\phi_{i(t-1)}$, and its interaction term with R_{it} ($L_{i(t-1)} \times R_{it}$) as an instrument for $\phi_{i(t-1)} \times R_{it}$. According to our theoretical framework, we expect $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$ can have effect on the w_{it}. To validate the relevance of the instruments used in these regressions, we examine the correlations in the first-stage regressions. Specifically, the positive correlation between $M_{i(t-1)}$ and $s_{i(t-1)}$, as well as between $L_{i(t-1)} \times R_{it}$ and $\phi_{i(t-1)} \times R_{it}$ confirm the validity and relevance of these instruments. Additionally, the reduce form show $s_{i(t-1)}$ and $L_{i(t-1)} \times R_{it}$ are significantly correlated with starting wage. F-test results in each first-stage regressions that exceed the critical threshold of 10 indicate no issues of weak instruments. Nonetheless, the p-value of the under-identification test is 0.0000, confirming the instruments are sufficiently correlated with the endogenous explanatory variables.

Although empirically validating exogeneity is challenging in the IV method, we follow Glitz (2017) by conducting placebo test, using wage growth as outcome variables. Our theoretical framework posits that short-term mental health ($M_{i(t-1)}$) and residence proximity ($L_{i(t-1)} \times R_{it}$) during the job search period influence starting wages only through $s_{i(t-1)}$, by providing an information premium, and only through $\phi_{i(t-1)} \times R_{it}$, by signaling high unobserved productivity. Given this, we do not expect only $L_{i(t-1)} \times R_{it}$ to have a persistent effect on wage growth, as wage growth is driven by peer effects and other labor market dynamics (also see Appendix G). Additionally, since network size primarily provides an initial information premium, we do not expect its instrument ($M_{i(t-1)}$) to have any effect on subsequent wage outcomes, such as wage growth. Therefore, a significant relationship between the IVs and these later outcomes would suggest a violation of the exclusion restriction. The results of the placebo test, shown in columns 5, indicate that $M_{i(t-1)}$ and $L_{i(t-1)} \times R_{it}$ during the job search period has no statistically significant effect wage growth. These results further confirm the validity of our instruments.

Table 3.4: The IV estimates of first-stage, reduce-form and placebo test

	First stage			Reduce & Placebo test	
	$s_{i(t-1)}$	$\phi_{i(t-1)}$	$\phi_{i(t-1)}R_{it}$	Baseline	Growth
	(1)	(2)	(3)	(4)	(5)
$M_{i(t-1)}$	**0.791*****	-0.005	-0.033	**0.037****	**0.003**
	(0.171)	(0.023)	(0.021)	**(0.013)**	**(0.005)**
$L_{i(t-1)}$	-0.088	0.109	-0.235***	0.023	-0.019
	(0.523)	(0.072)	(0.064)	(0.025)	(0.016)
$L_{i(t-1)} \times R_{it}$	0.182	-0.037	**0.378*****	**0.073****	**0.000**
	(0.572)	(0.078)	**(0.070)**	**(0.028)**	**(0.018)**
N	7,554	7,554	7,554	7,554	25,512
R^2	0.0445	0.1465	0.2975	0.7025	0.0641
Person controls	Yes	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes	Yes
F-test	11.36	26.67	64.26		
Under identification test	Chi-sq(1) P-val = 0.0000				

Notes: Columns 1-3 reports the first-stage results for the IV specifications, displaying the estimated effects of the instrumental variable on endogenous variable, namely network size, network quality and its interaction with referral. Column 4 reports the reduce-form of IV specifications, examining the effect of our IVs on the monthly starting wage. Columns 5 and 6 provide the placebo tests, showing the effects of the instrumental variables on the current wage and wage growth. The current wage refers to the wage earned in the second and third years of employment, while wage growth is calculated as the difference between the current wage and the starting wage within the same employment spell. The instrument for network size is lagged short-term mental health, and for network quality, it is lagged residence proximity. The full set of controls described in equation (16) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

D. The role of τ

In our theoretical model, the parameter τ determines the relative importance of observed versus unobserved productivity as signaled by $\phi_{i(t-1)}$. Then, τ may vary by job seeker's ability level, as high-ability job seeker is more challenging to observe compared to low-ability job seeker. To examine this, we split the sample based on occupation and education levels.[72] We conduct separate regressions for $\ln w_{it}$ according to high-mid and low skill occupation levels, and for Em_{it} and R_{it}, we divide the sample based on high and low education levels.[73]

As shown in columns 3 and 6 in panel A of Table 3.5, the Heckman-IV estimates indicate that the coefficient of the interaction term $\phi_{i(t-1)} \times R_{it}$ is significant only for the high-mid skill group, with about 6.1% (p<0.05). In contrast, for the low skill group, the effect is only 0.4% (p>0.1) and statistically insignificant. Interestingly, unlike to the effect of $\phi_{i(t-1)}$, the effect of $s_{i(t-1)}$ on monthly starting wages through Em_{it} is more substantial for the low skill group. According to columns 4 and 5, we calculate that the total indirect effect of $s_{i(t-1)}$ through Em_{it} on starting wages is about 8.5% for the low skill group (comprising an 8.3% direct effect and a 0.2% indirect effect through referrals). Similarly, for the high-mid skill group, as shown in columns 1 and 2, the total indirect effect of $s_{i(t-1)}$ through Em_{it} on starting wages is about 3.5%, which is nearly three times lower than that for low skill job seekers. This suggests that low skill job seekers benefit more from the effect of $s_{i(t-1)}$ than from $\phi_{i(t-1)}$, leading them to invest more effort in expanding their network size. As shown in Appendix B, following the additional assumptions by Galenianos (2021), the optimal network investment for low skill job seekers is primarily focused on $s_{i(t-1)}$. Additionally, our data further support this argument, showing that the mean difference in $\phi_{i(t-1)}$ between low and high-mid skill workers is about 34%, while the mean difference in $s_{i(t-1)}$ is only about 13%.[74]

Moreover, while the indirect effect of $s_{i(t-1)}$ on starting wages is more pronounced for low skill job seekers, the direct effect of $s_{i(t-1)}$ on starting wages for high-mid skill job seekers is nearly six times larger, at 6.6% (p<0.001). One potential reason for the higher directly effect of $s_{i(t-1)}$ on starting wages for high-mid skill job seekers is the high distribution of occupations in sales, financial, human health and social work

[72] To define high-mid and low-skill occupation levels, we use the NS-SEC code. The "*management & professional*" and "*intermediate*" categories are classified as the high-mid skill group, while the "*routine*" category is classified as the low-skill occupation group. Similarly, as discussed earlier, we define job seekers with education at or above the undergraduate level as the high education group, and those with lower education levels as the low education group.

[73] In the Em_{it} and R_{it} regressions, some job seekers are unemployed, making it impossible to observe their occupation skill levels directly. However, using education level as a proxy is appropriate, as individuals with higher education are more likely to engage in high-skill occupations.

[74] High-skill job seekers tend to have both larger and higher-quality networks because their higher wages allow them to invest more in expanding and maintaining their networks compared to low-skill job seekers.

activities, which is almost 30%. Indeed, if the employer assesses observed productivity, which includes both network resource ($\psi(s_i, \phi_i)$) and other observable skills (p_i), then the total observed productivity could be represented as $a_i = \gamma\psi(s_i, \phi_i) + (1-\gamma)p_i$, as Equation (2) shown. Then, in Equation (16), we can see network size can have a direct effect on starting wage outcomes. Overall, for low skill job seekers, the share of the indirect effect of $s_{i(t-1)}$ through Em_{it} on starting wages is about 66.3%, indicating they primarily rely on $s_{i(t-1)}$ to increase their job information. In contrast, for high-mid skill job seekers, the indirect effect is only 30.5%, suggesting that high-mid skill job seekers benefit more from observed productivity derived from their network resources. The next section provides a detailed discussion on network resources

Nonetheless, as discussed in Equation (2), there is an additional parameter μ that determines the effect of previous tenure on $\phi_{i(t-1)}$. Specifically, employers are more likely to perceive that referred job seekers have been closely observed by their referrers over an extended period, allowing for a more accurate assessment of their unobserved productivity. To examine this hypothesis, we identify each job seeker's tenure at their previous job and match this information with their current job data.[75] As shown in Appendix H, we find that the effect of $\phi_{i(t-1)} \times R_{it}$ on monthly starting wages increases as past job tenure increases. Specifically, for job seekers with less than one year of prior tenure, using a high-quality referral may even reduce their starting wage, as employers are less likely to trust such referrals, leading to $\mu = 0$ and resulting in total productivity of $y_i = \tau a_i$. However, as a job seeker's past tenure increases, the effect of $\phi_{i(t-1)} \times R_{it}$ on monthly starting wages rises significantly, with a 6.1% increase ($p<0.05$) for those with at least two years of prior job experience.

[75] If job seeker is unemployed all the time when we observe, we set their previous tenure equal to zero.

Table 3.5: The IV estimates of network size and quality on the labour market outcomes by occupation skill and age levels

	High-mid skill			Low skill		
	R_{it}	Em_{it}	w_{it}	R_{it}	Em_{it}	w_{it}
	(1)	(2)	(3)	(4)	(5)	(6)
$s_{i(t-1)}$	0.040***	0.032***	0.066***	0.056***	0.083***	0.010
	(0.014)	(0.008)	(0.013)	(0.016)	(0.011)	(0.033)
$\phi_{i(t-1)}$	0.014**	-0.013***	-0.033	-0.043***	-0.038***	0.000
	(0.007)	(0.004)	(0.023)	(0.008)	(0.006)	(0.051)
R_{it}		0.056***	-0.068		0.044***	0.035
		(0.005)	(0.080)		(0.007)	(0.037)
$\phi_{i(t-1)} \times R_{it}$			0.061**			0.004
			(0.022)			(0.068)
N	20,270	20,270	5,351	16,982	16,982	2,203
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
O & I & T FE			Yes			Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows IV estimates for effects of network size and quality on referral used probability, employment probability, segmented by job skill. Also, the table shows IV-Heckman estimates for effects of network size, network quality and its interaction term with referral used on monthly starting wage, segmented by job skill). Notably, the wage and referral regressions are separated according to occupation skill levels (NS-SEC), while the employment probability regression is separated based on education level. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

E. Network capital vs. Human capital

As discussed above, $s_{i(t-1)}$) may influence starting wages through two channels: (i) by increasing hiring probability, and (ii) by serving as a measure of observed productivity. In cases where network size serves as observed productivity, employers in occupations with high levels of social interaction may prioritize network capital over human capital, offering higher starting wages based on network capital, as reflected in Equations 2 and 15, where $a_i = \gamma s_i + (1-\gamma)p_i$. To examine this effect, we identify job seekers employed in occupations that inherently involve high levels of social interaction, such as sales, marketing, and social work. Additionally, we analyze job seekers in occupations with limited social interaction, such as manual labor, to compare their outcomes with those in more social occupations. To capture the observed skills component, we also report results based on abilities measured in areas such as memory, verbal skills, math, and reading.

As shown in column 1 of Table 3.6, the Heckman-IV estimates indicate that both the coefficients of $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$ are significant and larger compared to our baseline model in sales and social occupations, with effects of 10.5% ($p<0.05$) and 9.7% ($p<0.05$), respectively. This suggests that employers offer a wage premium to job seekers with extensive and higher-quality networks. In contrast, the observed skills do not play a significant role in determining starting wages, with coefficients close to zero ($p>0.1$). This finding supports our hypothesis that, in high levels of social interaction occupations, employers place more value on network capital than on human capital. On other hand, in manual labor occupations (see column 2), employers do not offer a wage premium for network capital, and even using network capital to find a job has a negative impact on starting wages. One potential explanation is that relying on network capital in these occupations may send a negative signal, which gives employers greater bargaining power and leads to wage cuts (Bentolila et al., 2010; Kramarz and Skans, 2014). Interestingly, in manual labor occupations, observed skills are also not significant, possibly because the skills measured (e.g., memory, verbal skills, math, and reading) are less relevant to the tasks typically required in these occupations.

The remaining occupations are grouped as “other occupations” and further categorized into high-mid and low skill occupations. In the high-mid skill group (see column 3), unlike in sales and social occupations, we find that both network capital and human capital play important roles, though network capital is less influential compared to the sales and social sectors, with 3.9% for $s_{i(t-1)}$ and 4.7% ($p<0.1$) for $\phi_{i(t-1)} \times R_{it}$. In the low-skill group, employers primarily value observed skill levels, with a percentage points increase in skill level associated with a 0.075% increase in starting wages, while network capital appears to have little impact. These findings highlight network capital proves most valuable in occupations that rely heavily on social interaction, while human capital plays a greater role in low-skill occupations where social interaction is less critical.

Table 3.6: The effect of network size and quality on the monthly starting wage by occupation types

	Sales & Social	Manual Labor	Other occupation	
			High-mid	Low
	(1)	(2)	(3)	(4)
$s_{i(t-1)}$	0.105^{**}	-0.010	0.039^{**}	-0.015
	(0.049)	(0.091)	(0.017)	(0.069)
$\phi_{i(t-1)} \times R_{it}$	0.097^{**}	-0.092	0.047^{*}	-0.115
	(0.042)	(0.077)	(0.025)	(0.070)
P_{it}	-0.003	0.019	0.034^{**}	0.075^{**}
	(0.024)	(0.047)	(0.014)	(0.038)
N	1,899	732	3,992	944
R^2	0.7571	0.7569	0.6645	0.6290
Basic controls	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes
$\boldsymbol{N'}_{i(t-1)}$	Yes	Yes	Yes	Yes

Notes: The table shows IV-Heckman estimates for effects of network size, network quality and its interaction term with referral used on monthly starting wage. The "Sales & Social" category represents occupations that inherently involve higher levels of social interaction, such as sales, marketing, and social work. The "Manual Labor" category includes jobs that involve less reliance on social networks, such as cleaning, agriculture, and machine operative. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects. Standard errors clustered at the level of the time and industry-occupation are in parentheses. $*/**/***$ denote statistical significance at the 10%/5%/1% level, respectively.

F. Heterogeneity effect

We now provide additional evidence on the effects of network size and network quality on starting wages by investigating heterogeneity in working status (part-time vs. full-time) and firm size (less than 50 vs. 50 or more employees). Table 3.7 presents IV estimates of $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$ on monthly starting wages. For job seekers who find part-time employment, neither $s_{i(t-1)}$ nor $\phi_{i(t-1)} \times R_{it}$ leads to significant changes in starting wages, as indicated by coefficients of -0.014 and -0.028 log points (p>0.1), respectively. Conversely, for job seekers who secure full-time positions, there is a significant positive effect for both $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$, with coefficients of 0.083 log points (p<0.01) and 0.044 log points (p<0.05), respectively. These results partially support our theoretical framework, showing that part-time roles are often associated with lower skill requirements and shorter job tenure, reducing the importance of network size and quality in wage determination. When examining firm size, we observe further heterogeneity. For smaller firms (fewer than 50 employees), both $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$ show no significant effect on starting wages. In contrast, for larger firms (50 or more employees), the positive impact of network size and referral quality becomes more pronounced. Full-time employees at larger firms see a larger wage premium associated with $s_{i(t-1)}$ and $\phi_{i(t-1)} \times R_{it}$, with coefficients of 0.100 log points (p<0.01) and 0.092 log points (p<0.01), respectively.

Table 3.7: The heterogeneity IV estimates of network size and quality on the monthly starting wage

	Working status		Firm size	
	Part-time	Full-time	< 50	>= 50
	(1)	(2)	(3)	(4)
$s_{i(t-1)}$	-0.014	0.083***	-0.020	0.100***
	(0.112)	(0.024)	(0.040)	(0.033)
$\phi_{i(t-1)} \times R_{it}$	-0.028	0.044**	-0.004	0.092***
	(0.090)	(0.021)	(0.037)	(0.028)
N	953	6,601	3,049	4,505
R^2	0.4963	0.6467	0.7005	0.6788
Basic controls	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes

Notes: The table shows IV-Heckman estimates for effects of network size and quality on monthly starting wage, segmented by working status and firm size. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

3.6 Conclusion

Although network effects on labor market outcomes have been extensively studied, empirical evidence on the role of network quality in starting wages and wage growth remains limited. Also, previous research has primarily focused on referral networks as channels for information transmission, with little emphasis on signaling mechanisms and the intrinsic value of network capital. Our study addresses these gaps by conducting a comprehensive analysis of the direct and indirect effects of both network size and quality on labor market outcomes, using an IV-Heckman approach to control for endogeneity and self-selection biases.

We find that an increase in network size significantly raises starting wages, both directly and indirectly through higher hiring probability. The indirect effect is especially pronounced for low-ability job seekers, indicating that they rely more heavily on job information from their networks to secure employment. Additionally, job seekers with high network quality who also use referrals experience significant gains in both starting wages and wage growth. This suggests that referrals, when combined with high-quality networks, serve as a strong signal of high productivity among job seekers. High-quality referrals particularly benefit high-ability job seekers, as their unobserved productivity exhibits greater variance, making the signaling effect more impactful. In occupations where social connections and interpersonal skills are highly valued, such as sales and social work, network capital, namely network size and quality, exerts a particularly strong influence on starting wages. Conversely, human capital plays a more prominent role in low-skill occupations where social interaction is less critical, highlighting that the value of network capital is most pronounced in roles that rely heavily on interpersonal relationships and social networks.

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3.8 Appendix

Appendix A: the effect of network size and quality on wage

Proof $\frac{\partial w}{\partial \phi} > 0$:

According to the Equation (15), we have:

$$w = \frac{\beta[r + \delta + (1 - \delta)h(s)]}{r + \delta + \beta(1 - \delta)h(s)}[a(s, \phi) + R(s)\mu\tau\zeta(\phi)] \tag{A.1}$$

Let $A = \frac{\beta[r+\delta+(1-\delta)h(s)]}{r+\delta+\beta(1-\delta)h(s)}$, the wage equation becomes:

$$w = \mathrm{A}[a(s, \phi) + R(s)\mu\tau\zeta(\phi)] \tag{A.2}$$

Differentiating the w with ϕ, we have:

$$\frac{\partial w}{\partial \phi} = \mathrm{A}\left(\frac{\partial a(s, \phi)}{\partial \phi} + R(s)\tau\frac{d\zeta(\phi)}{d\phi}\right) \tag{A.3}$$

Since $\mathrm{A} > 0$, $\frac{\partial a(s,\phi)}{\partial \phi} > 0$ and $\frac{d\zeta(\phi)}{d\phi} > 0$, then $\frac{\partial w}{\partial \phi} > 0$.

Proof $\frac{\partial w}{\partial s} > 0$:

Let the wage equation becomes:

$$w = \frac{B(s)}{C(s)}D(s) \tag{A.3}$$

where $B(s) = \beta[r + \delta + (1 - \delta)h(s)]$, $C(s) = r + \delta + \beta(1 - \delta)h(s)$, and $D(s) = \big(1 - R(s)\big)a(s, \phi) + R(s)\big(\tau a(s, \phi) + (1 - \tau)\zeta(\phi)\big)$.

Now, differentiate with respect to s:

$$\frac{dB}{ds} = \frac{dC}{ds} = \beta(1 - \delta)h'(s)$$
$$\frac{dD}{ds} = \frac{\partial a(s, \phi)}{\partial s} + R'(s)\mu\tau\zeta(\phi)$$

Given these, differentiate of w with respect to s:

$$\frac{dw}{ds} = \frac{\frac{dB}{ds}C(S) - \frac{dC}{ds}B(s)}{\left(C(s)\right)^2} D(s) + \frac{B(s)}{C(s)}\frac{dD}{ds} \tag{A.4}$$

Substituting, we have:

$$\frac{dw}{ds} = \frac{\beta(1-\delta)(1-\beta)(r+\delta)h'(s)}{\left(r+\delta+\beta(1-\delta)h(s)\right)^2} D(s) + \frac{B(s)}{C(s)}\frac{dD}{ds}$$

Since $\frac{\beta(1-\delta)(1-\beta)(r+\delta)h'(s)}{\left(r+\delta+\beta(1-\delta)h(s)\right)^2} > 0$, $D(s) > 0$, and $\frac{B(s)}{C(s)}\frac{dD}{ds} > 0$, it follows that $\frac{dw}{ds} > 0$.

Appendix B: optimal network

To obtain the optimal network structure, we follow Galenianos (2021), assuming that the total utility of a job seeker when unemployed and employed is given by:

$$\Lambda_i = uI_U + (1-u)I_E$$

or equivalently

$$\Lambda_i = I_E - u[I_E - I_U]$$

According to Equation (13), we have $I_E - I_U = w\frac{1+r}{r+\delta+(1-\delta)h(s,u,v)}$, and rearrange Equation (11), we have $I_E = w\frac{1+r}{r}\frac{r+(1-\delta)h(s)}{r+\delta+(1-\delta)h(s)}$. Thus, we obtain:

$$\Lambda = w\frac{(1+r)[r(1-u)+(1-\delta)h(s)]}{r[r+\delta+(1-\delta)h(s)]}$$

where $w = \frac{\beta[r+\delta+(1-\delta)h(s)]}{r+\delta+\beta(1-\delta)h(s)}[a(s,\phi)+R(s)\mu\tau\zeta(\phi)]$.

Next, we assume that the total effort required to maintain the network is given by $C = c_1 s + c_2\phi$, where c_1 and c_2 are the marginal costs of network size and network quality, respectively. Therefore, the total utility after accounting for the cost of maintaining the network becomes:

$$\mathcal{L}_i = \Lambda_i - C = \frac{(1+r)[r(1-u)+(1-\delta)h(s_i)]}{r[r+\delta+(1-\delta)h(s_i)]}w_i - c_1 s_i - c_2\phi_i$$

Correction:

Our empirical results show that the effect of s on $R(s,u,v)$ is small (i.e., other factors play a more significant role in determining referral usage, as indicated by the low R^2 value of 1.8%; see Table 3.3). Additionally, we can directly observe whether a job seeker uses a referral or not. Therefore, to simplify the model, we assume that $R(s,u,v)$ does not depend on s and treat R_i as a binary variable, where $R_i = \{0,1\}$. Additionally, without loss of generality, we assume job seeker has full bargaining power, with $\beta = 1$, then $w = a(s, \phi) + R(s)\mu\tau\zeta(\phi)$.

Low-skill job seeker:

As displayed in Table 3.5, since the effect of ϕ_i is minimal in both $a_i(s_i, \phi_i)$ and $\zeta(\phi_i)$, capturing by ς_L and λ_L in Equation (17), we assume without loss of generality that $a_i(s_i, \phi_i) = a_i(s_i)$ and $\lambda_L = 0$. Assuming that $\zeta(\phi_i) = \lambda_L \phi_i b_i$, then their overall utility becomes:

$$\mathcal{L}_i = \mathrm{A} a_i(s_i) - c_1 s_i - c_2 \phi_i$$

where $\mathrm{A} = \frac{(1+r)[r(1-u)+(1-\delta)h(s_i)]}{r[r+\delta+(1-\delta)h(s_i)]}$. In this scenario, low-skilled job seekers do not invest in network quality, as it would decrease their overall utility, since $\frac{\partial \mathcal{L}_i}{\partial \phi_i} = -c_2 < 0$. However, investing in network size may provide profits, given that $a_i = \gamma s_i + (1-\gamma)p_i$ and setting $\gamma = 0$ to let $a_i = p_i$ to only focus on the job information and signilling mechanism first since $\gamma \neq 0$ only increase the profit and setting to zero is more directly to get the result of optimal s_i^*. In general, job seeker improve his network size and get more total expected utility when $\frac{(1+r)r(1-\delta)(\delta+ru)\vartheta}{(r+\delta+(1-\delta)h(s_i))^2} p_i > c_1$ as $\frac{\partial \mathcal{L}_i}{\partial s_i} = \frac{(1+r)r(1-\delta)(\delta+ru)\vartheta}{(r[r+\delta+(1-\delta)h(s_i)])^2} p_i - c_1$. In this case, the optimal network size:

$$s_i^* = \frac{1}{r(1-\delta)} \left(\sqrt{\frac{(1+r)r(1-\delta)(\delta+ru)\vartheta}{c_1}} p_i - r[r+\delta+(1-\delta)(v+\pi)] \right)$$

This suggests that low-skilled job seekers should focus on expanding their network size to enhance their hiring probability, but avoid investing in network quality, as it does not yield positive returns to their overall utility in this context.

High-skill job seeker:

For high-skill job seekers, the situation becomes more complex, since $\lambda_H \neq 0$ (but similar $a_i(s_i, \phi_i) = a_i(s_i)$; see Table 3.5), they must balance the benefits and costs of investing in ϕ_i, with

$$\frac{\partial \mathcal{L}_i}{\partial \phi_i} = \mathrm{A}R(s_i)\mu\tau\lambda_H b_i - c_2$$

In this case, if the high-skill job seeker has sufficiently high unobserved productivity, with $b_i \geq \frac{c_2}{\mathrm{A}\mu\tau\lambda_H}$ in the case of $R_i = 1$, it becomes beneficial for them to invest in network quality (ϕ_i) to signal their productivity and maximize their utility.

1) In the case of $b_i < \frac{c_2}{\mathrm{A}\mu\tau\lambda_H}$:

   Since the network quality cost is high, then job seeker does not invest network quality such that $\phi_i = 0$. Then, the overall expected utility becomes:

$$\mathcal{L}_i = \mathrm{A}a_i(s_i) - c_1 s_i - c_2\phi_i$$

   Similarly to the case of low-skill job seeker to set $R_i = 0$, and the optimal network is to invest to network size, with s_i^*.

2) In the case of $b_i \geq \frac{c_2}{\mathrm{A}\mu\tau\lambda_H}$:

   Since the network quality cost is low, then job seeker invests network quality such that $\phi_i = 1$. Then, the overall expected utility becomes in the case of $R_i = 1$:

$$\mathcal{L}_i = \frac{(1+r)[r(1-u) + (1-\delta)h(s_i)]}{r[r+\delta+(1-\delta)h(s_i)]}(p_i + \mu\tau\lambda_H b_i) - c_1 s_i - c_2$$

   In this case, the optimal network size:

$$s_i^* = \frac{1}{r(1-\delta)}\left(\sqrt{\frac{(1+r)r(1-\delta)(\delta+ru)\vartheta}{c_1}\mathrm{B}_i} - r[r+\delta+(1-\delta)(v+\pi)]\right)$$

   where $\mathrm{B}_i = p_i + \mu\tau\lambda_H b_i$.

Appendix C: PASS-ADIAB and MCSUI

The PASS-ADIAB dataset is a unique linked data resource that combines information from the Panel Study Labour Market and Social Security (PASS) with administrative data from the Institute for Employment Research (IAB) in Germany. The PASS survey, initiated by the IAB in 2006, primarily aims to study the dynamics of unemployment and the effectiveness of social policy in Germany. The PASS-ADIAB survey dataset is exceptionally useful for quantifying social networks, incorporating questions that facilitate the assessment of both employed network size and quality. To assess network size, the survey asks, "*How many close friends or family members, with whom you maintain a close relationship, do you have outside your household?*" and "*Is this person unemployed?*" (for each of the participant's three closest friends; calculating the proportion of employed friends). To evaluate network quality, participants were asked about three of their closest friends, specifically whether each friend had "*Has this person completed a degree at a university or advanced technical college?*". Since this variable is binary, network quality can be directly calculated as the average education level of a respondent's friends. For identifying starting wages, the dataset utilizes administrative records, enabling the observation of employment spells, including transitions from unemployment to employment and job-to-job changes. This detailed tracking of employment spells allows us to accurately pinpoint the starting wage at the beginning of each new employment period.

The Multi-City Study of Urban Inequality (MCSUI) is a survey conducted between 1992 and 1994 across four major U.S. cities. To assess network size, we use responses to the questions, "*Number of people in network (up to three)*" and "*Does this person have a steady job?*" for each of the participant's three closest friends, calculating the proportion of employed friends. To evaluate network quality, participants were asked, "*What level of education has the person completed?*" for each of their three closest friends. Unlike the UKHLS and PASS-ADIAB, where the cut-off for high education is set at the college or undergraduate level, we define high education in MCSUI as completion of Secondary education (high school). This adjustment based on the higher proportion of individuals with a high school education in MCSUI (38.0%), which is comparable to the proportion of individuals with a college or undergraduate education in the UKHLS (37.7%). Next, we construct a dummy variable to indicate whether a participant's friends have a high education level, and network quality is calculated as the average proportion of friends with a high education level. Additionally, MCSUI does not capture starting wages, as the survey only collects information on current wages. To ensure robustness, we compare the network quality effect on current wages in MCSUI with the corresponding effect on current wages in the UKHLS. The results are presented in Table C1.

Table 3.C1: Statistical Description of PASS-ADIAB and MCSUI

	PASS-ADIAB			MCSUI	
	Mean	SD		Mean	SD
Network size	7.532	7.016		1.728	1.277
Network quality	0.545	0.169		0.577	0.465

Notes: The total sample consists of approximately 1,935 observations in PASS-ADIAB and 2,643 observations in MCSUI in the wage regression.

Table 3.C2: The effect of network size and quality on labour outcomes by different datasets

	UKHLS		PASS-ADIAB		MCSUI	
	Em_{it}	**Starting**	Em_{it}	**Starting**	Em_{it}	Current
	(1)	(2)	(3)	(4)	(5)	(6)
$s_{i(t-1)}$	0.003***	0.005**	0.005***	0.007**	0.127*	0.107***
	(0.000)	(0.002)	(0.001)	(0.031)	(0.073)	(0.012)
$\phi_{i(t-1)} \times R_{it}$		0.047***		0.053***		0.087**
		(0.009)		(0.017)		(0.039)
N	37,109	7,558	2,068	1,935	3,328	2,643
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes		

Notes: Notes: The table presents the estimated effects of network size and quality on employment probability and starting wage, using linear OLS models with data from the UKHLS, PASS-ADIAB, and MCSUI. Notably, the dependent variable in Column 6 is the current wage. The full set of controls described in equation (16) is included but not reported here. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills in Columns 1 and 2, and by previous wage in Columns 3 and 4. $*/**/***$ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix D: Mediation effect

In this section, we aim to calculate both the direct and indirect effects of network size $s_{i(t-1)}$ on starting wages w_{it}, with the indirect effect operating through the hiring (employment) probability Em_{it}. According to Equation (15), the wage equation can be represented by the following regression model:

$$w_{itop} = \alpha_0 + \gamma s_{i(t-1)op} + \delta\phi_{i(t-1)op} + \boldsymbol{X}'_{itop}\eta + \varphi_t + \tau_o + \mu_p + \varepsilon_{itop} \tag{D.1}$$

where w_{it} represents the wage of individual i at time t, $s_{i(t-1)}$ represents job seeker's employed network size at time $t-1$; γ captures the **direct effect** of $s_{i(t-1)}$ on wages, expressed in absolute wage units.

Next, by incorporating the hiring probability Em_{it} into the wage regression, we obtain:

$$\begin{aligned} w_{itop} = \alpha_0 + \gamma s_{i(t-1)op} + \delta\phi_{i(t-1)op} + \rho Em_{it} + \boldsymbol{X}'_{itop}\eta \\ +\varphi_t + \tau_o + \mu_p + \varepsilon_{itop} \end{aligned} \tag{D.2}$$

where Em_{it} denotes the employment status of individual i at time t, and the term δ captures the effect of employment status on wages. In addition, the employment probability is modeled by the following regression:

$$Em_{it} = \alpha_0 + \vartheta s_{i(t-1)} + \delta\phi_{i(t-1)} + \boldsymbol{X}'_{it}\eta + \varepsilon_{it} \tag{D.3}$$

where ϑ represents the impact of $s_{i(t-1)}$ on the likelihood of employment. According to traditional medication model, the indirect effect of $s_{i(t-1)}$ on wage is simply equals to $\rho\vartheta$. However, since our mediator (Em_{it}) in Equation (D.2) is conditionally observed (i.e., wage outcomes are only recorded when $Em_{it} = 1$), applying the traditional mediation model is challenging.

Assuming that unemployed individuals earn zero wages, the wage difference between employed and unemployed individuals is equal to the average wage of the employed, denoted as $\overline{w}$. Thus, the effect of employment on wages becomes the average wage of the employed, and the coefficient ρ in the wage equation (D.2) equals $\overline{w}$. Then, the wage regression then becomes:

$$w_{itop} = \alpha_0 + \gamma s_{i(t-1)op} + \delta\phi_{i(t-1)op} + \overline{w}Em_{it} + \boldsymbol{X}'_{itop}\eta + \varphi_t + \tau_o + \mu_p + \varepsilon_{itop} \quad \text{(D.4)}$$

According to Equations (D.3) and (D.4), the indirect effect of $s_{i(t-1)}$ on starting wage through its impact on employment probability is $\vartheta\overline{w}$, while γ captures the direct effect of $s_{i(t-1)}$ on wages in absolute wage units. Furthermore, according to our framework, the $s_{i(t-1)}$ effect on the Em_{it} may through the R_{it}, with:

$$R_{it} = \alpha_0 + \rho s_{i(t-1)} + \xi\phi_{i(t-1)} + \boldsymbol{X}'_{it}\eta + \boldsymbol{N}'_{i(t-1)}\upsilon + \varphi_t + \varepsilon_{it} \quad \text{(D.5)}$$

Thus, the total indirect effect of $s_{i(t-1)}$ on starting wage through its impact on employment probability becomes $(\vartheta + \pi\rho)\overline{w}$. Finally, the shared of indirect effect of $s_{i(t-1)}$ on starting wage becomes $S = \frac{(\vartheta+\pi\rho)\overline{w}}{\gamma+(\vartheta+\pi\rho)\overline{w}}$. Table D below presents the results of the direct effect of network size on starting wages (absolute value) across various scenarios to estimate γ. The other coefficients, ϑ, π, and ρ, can be directly observed in Tables 3.3 and 3.4.

Table 3.D: The effect of network size and quality on the monthly starting wage

	(1)	(2)	(3)
	Full	High-Mid skill	Low skill
	(Table 3.3)	(Table 3.4)	(Table 3.4)
$s_{i(t-1)}$	129.33**	217.42***	57.90
	(49.82)	(52.72)	(72.15)
N	7,558	5,351	2,203
Basic controls	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes
Productivity	Yes	Yes	Yes

Notes: The table shows IV-Heckman estimates for effects of network size and quality on monthly starting wage (absolute value), segmented by job skill. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. ∗∗∗ denotes significance at the 1% level, ∗∗ denotes significance at the 5% level, and ∗ denotes significance at the 10% level.

Appendix E

Table 3.E: The robust check of IV and Heckman estimates

	OLS	Linear IV		Control function	
	w_{it}	Reduce	2SLS	Reduce	IV
	(1)	(1)	(2)	(4)	(5)
Panel A: DV $\boldsymbol{s_{i(t-1)}}$					
$M_{i(t-1)}$	0.005***	0.036*	0.045**	0.036*	0.045***
	(0.001)	(0.019)	(0.016)	(0.019)	(0.012)
res_s					
First-stage statistics					
$M_{i(t-1)}$			0.797***		0.797***
			(0.228)		(0.228)
Panel B: DV $\boldsymbol{\phi_{i(t-1)}}$					
$L_{i(t-1)}$	0.018*	0.080**	0.919**	0.080**	0.017*
	(0.010)	(0.033)	(0.382)	(0.033)	(0.010)
imr_q					0.152
					(0.177)
First-stage statistics					
$L_{i(t-1)}$			0.087**		0.087**
			(0.038)		(0.038)
N	7,558	7,558	7,558	7,558	7,558
Basic controls	✓	✓	✓	✓	✓
O & I & T FE	✓	✓	✓	✓	✓
$\boldsymbol{N}'_{i(t-1)}$	✓	✓	✓	✓	✓
Productivity	✓	✓	✓	✓	✓

Notes: The table shows Heckman-IV estimates for effects of network size and quality on monthly starting wage, segmented by job skill level. Notably, the wage and referral regressions are separated according to occupation skill levels (NS-SEC), while the employment probability regression is separated based on education level. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. The predicted mean of the starting wage estimated using the Heckman-IV model with all controls and fixed effects to calculate the indirect effect of network size. Standard errors clustered at the level of the time and industry-occupation are in parentheses. ∗/∗∗/∗∗∗ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix F

Table 3.F: The robust check of IV and Heckman estimates

	Em_{it}		R_{it}		Heckman	
	Reduce	IV	Reduce	IV	First	Reduce
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IV						
$M_{i(t-1)}$	0.061^{***}	0.062^{***}	0.050^{***}	0.051^{***}		
	(0.007)	(0.007)	(0.010)	(0.011)		
First-stage statistics						
$M_{i(t-1)}$		0.974^{***}		0.976^{***}		
		(0.088)		(0.080)		
Panel B: Heckman						
Gender Homophily					0.157^{**}	0.013
					(0.073)	(0.011)
N	38,321	38,321	38,500	38,500	48,936	7,554
R^2	0.0341	0.1820	0.0346	0.0159	0.253	0.7093
Person controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{i(t-1)}$	Yes	Yes	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes	Yes	Yes
F-test		70.42		91.00	269.66	

Notes: Columns 2 and 4 in Panel A reports the first-stage results for the IV specifications, displaying the estimated effects of the lagged short-term mental health on lagged network size. Also, we report the second stage of IV specifications, examining the casual effect of lagged network size on the monthly starting wage. Columns 1 and 2 in Panel A provides the reduced-form estimates, showing the effects of the lagged short-term mental health on the monthly starting wage without controlling the lagged network size. Columns 5 and 6 in Panel B report the results for the Heckman specifications, displaying the estimated effects of lagged age homophily on the employment probability and monthly starting wage, respectively. The full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. $*/**/***$ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix G

Table 3.G: The effect of network quality on the monthly current wage and wage growth

	Current wage			Wage growth
	All sample	Year 2-3	Year 4+	All
	(1)	(2)	(3)	(4)
$\phi_{i(t-1)} \times R_{it}$	0.079***	0.052**	0.129***	0.026***
	(0.017)	(0.021)	(0.029)	(0.006)
N	13,796	8,888	4,907	13,743
R^2	0.6698	0.6757	0.6643	0.0685
Basic controls	Yes	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes	Yes
$\boldsymbol{N}'_{it}$	Yes	Yes	Yes	Yes
Productivity	Yes	Yes	Yes	Yes

Notes: The table shows OLS estimates for effects of network quality and its interaction with referral used on monthly current wage and wage growth. We match the information of network quality and referral used from the job search period to the working period. The network size and network quality variables are added but is not reported. Also, the full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors are in parentheses. */**/*** denote statistical significance at the 10%/5%/1% level, respectively.

Appendix H

Table 3.H: The effect of network quality on the labour market outcomes by pervious tenure

	Previous tenure < 1	Previous tenure = 1	Previous tenure >= 2
	(1)	(2)	(3)
$\phi_{i(t-1)} \times R_{it}$	-0.045	0.025	0.061^{***}
	(0.078)	(0.036)	(0.013)
N	1,068	2,521	5,033
R^2	0.6127	0.6999	0.7061
Basic controls	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes
$\boldsymbol{N}'_{it}$	Yes	Yes	Yes
Productivity	Yes	Yes	Yes

Notes: The table shows IV-Heckman estimates for effects of network quality and its interaction with referral used on monthly starting wage, segmented by past job tenure. We identify each job seeker's tenure at their previous job and match this information with their current job. The network size and network quality variables are added but is not reported. Also, the full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. $*/{*}{*}/{*}{*}{*}$ denote statistical significance at the 10%/5%/1% level, respectively.

Appendix I

Table 3.I: The interaction effect of network size and quality on the monthly starting wage

	OLS	Heckman	IV-Heckman
	(1)	(2)	(3)
$\phi_{i(t-1)} \times s_{i(t-1)}$	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)
N	7,554	7,554	7,554
R^2	0.7027	0.7028	0.7036
Basic controls	Yes	Yes	Yes
O & I & T FE	Yes	Yes	Yes
$\boldsymbol{N}'_{it}$	Yes	Yes	Yes
Productivity	Yes	Yes	Yes

Notes: The table shows OLS, Heckman, and IV-Heckman estimates for interaction effects of network size and network quality on monthly starting wage. The network size and network quality variables are added but is not reported. Also, the full set of controls described in equation (17) is included but not reported. "O & I & T FE" refers to occupation, industry, and time fixed effects, while "Productivity" refers to job seekers' productivity levels, measured by multiple skills. Standard errors clustered at the level of the time and industry-occupation are in parentheses. $*/**/***$ denote statistical significance at the 10%/5%/1% level, respectively.