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兩篇關於勞動市場表現之論文
Two Essays on Labor Market Outcomes

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兩篇關於勞動市場表現之論文
Two Essays on Labor Market Outcomes

本論文係盧其宏君（學號 D02323005）在國立臺灣大學經濟學系完成之博士學位論文，於民國 108 年 06 月 16 日承下列考試委員審查通過及口試及格，特此證明

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連賢明

樊家忠

誌謝

在寫這篇誌謝的同時，長榮航空空服員正展開罷工。薪資停滯、工時過長，伴隨著企業獲利升高，這大概是這近十幾二十年台灣的寫照。當人們想到經濟學時，多半想起黑板上的兩條線，彷彿只要交給市場，社會福祉就會極大化，大同世界就會降臨。這樣的不證自明、簡單到不行的理論，創造出一種虛幻凌駕現實的狀況，潛在地限制更進一步的討論。

支持我繼續攻讀博士、完成論文的最大動力，其實就是對虛幻的經濟學演繹的質疑。實證是一條路，或許可以讓我們不只停留在想當然爾的理論推導之中，並且回頭指出理論與現實的落差。畢竟經濟不是存在於線性代數或是圖形裡面，而是真實地在我們的身邊。經濟學家真的了解經濟，就是一個這樣簡單的期望。

如果我們真的身處完全競爭市場，那工資應反映勞動生產力，強行調高基本工資只會帶來失業。然而，這本論文要說的故事並不如此。勞工能力就算有明顯差異，企業內也不一定會給予相應的高薪，且其淪於基本工資者的可能性也沒有明顯差異；而在政府調高基本工資後，就業並未下滑，反而可能使得就業提升。若實證如此，或許我們須反思過去所受到的經濟學教育。

很感謝劉錦添老師一路的指導協助，啟發了我對於實證研究的認識，以及對於一個離經叛道的學生的最大容忍。同時也感謝樊家忠、江淳芳老師的協助指導。這本論文絕大多數是在波士頓完成，如果沒有家人，尤其在旁支持的太太姚耀婷，我想我可能無法堅持下去。最後，我也感念所有在現實中為勞動權益作戰的弟兄姊妹，是他們讓我覺得研究或許還有改變些什麼的可能。

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中文摘要

本論文分兩篇，第一篇探討個人能力對於勞動市場表現的影響，第二篇探討基本工資調漲對於薪資、就業，以至於所得分配的影響。

第一篇摘要如下。文獻上對於個人能力的討論區分為認知能力與非認知能力。近年來，非認知能力的討論益得重視，不過，由於界定不易，研究多有爭議。本研究提出以大學入學考試成績來界定認知與非認知能力的方式，該方法不僅建立在行為觀察的基礎上，同時也可標準化考生表現與其行為動機。考生若參加推薦甄選或申請入學者，需有學力測驗成績；若其未成功推甄或申請上大學，須再參加指定考試。該類考生在其兩項考試間的表現差異，可視為其對於挫折的承受能力。這項能力與情緒管理、動機強弱、自制能力等非認知能力有關。同時，本研究以其學力測驗表現作為認知能力指標。此外，透過各項行政資料的輔助，本研究進而考慮家庭背景對能力之影響，以及能力對於就業選擇之影響，藉以釐清能力直接對於應試者 26 歲至 30 歲薪資與就業之影響。本研究發現，隨著年齡增長，對於挫折的承受力對進入私部門或公部門就業有逐步增長的直接正向影響，但對薪資無直接影響。相對的，認知能力對薪資有直接正面影響，對是否成為公務員跟挫折承受力有相似影響，但對於是否進入私部門就業無顯著影響。該結果顯示，與挫折承受力相關之非認知能力，較認知能力更能影響就業，但認知能力較能影響薪資。此外，是否成為基本工資者與上述兩種能力無明顯直接關聯，相較之下，家庭背景更具有解釋能力。

第二篇摘要如下。有別於過往文獻僅能以薪資分配數據驗證最低工資對薪資分配之影響，本論文以台灣月別的勞工保險與就業保險投保資料，驗證 2007 年本國基本工資調漲於個人與廠商層次所帶來的薪資、就業，以至於所得分配效果。透過完整的個人投保薪資紀錄，本研究得以直接區分出直接與間接受到基本工資影響者，補足過往文獻對於實際受最低工資影響者之評估的不足，並首次以斷點迴歸

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估計方式，降低過往文獻以設置控制組進行估計所產生的諸般研究爭議。本研究發現，在 2007 年基本工資調漲後，原薪資低於調漲後基本工資者之薪資提升，就業也微幅增加；原薪資微幅高於調漲後基本工資者之就業雖提升，但其薪資調漲受到壓抑。在同時考量就業與薪資效果下，前者整體所得提升，後者所得無顯著改變。該結果顯示，該次基本工資調漲並未產生外溢效果，即未發生基本工資調高也促使較高薪資者所得提升的現象。同時，本研究也發現，就業提升並非源於離職降低，而是源於新聘員工的增加。因此，廠商的獨買力較勞動市場摩擦，更能解釋台灣基本工資調漲對於就業的正面影響。此外，本研究也發現基本工資調漲導致企業內的職災率顯著提升，這對未直接受基本工資影響者形成負面的「外溢效果」。

關鍵字：非認知能力、挫折容忍度、基本工資、就業效果、薪資效果、外溢效果、所得分配效果

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ABSTRACT

This dissertation includes two essays. The first one is "Does Failure Tolerance Benefit People's Labor Market Outcomes? Evidence from Taiwan Joint College Entrance Examination." The second one is "Employment, Wage, and Inequality Effects from A Minimum Wage Increase: Evidence from Monthly Personnel Administrative Data of Taiwan."

The abstract of the first essay is as follows. Dealing with the difficulty of measuring non-cognitive skills, this study proposes a novel approach based on the particularity of the joint college entrance examination in Taiwan. Examinees who apply to a school but fail in their application need to take two tests, one is for the application and the other is for the final distribution. The performance difference between the two tests is proposed to be the measure of the tolerance for frustration, which is a skill related to examinees' emotional management, motivation, and perseverance after they experience a big frustration. This approach is not only based on an observed behavior but also meets the need of a standardized task. By taking advantage of several administrative personnel data, this study takes into consideration of factors behind the skills and the mechanisms after the skills to estimate the influence from skills on the cohorts' labor market outcome at ages 26 to 30. The results show that there is an increasing direct influence of frustration tolerance on employment in the private or public sector by age, but no direct effect on wages or being a minimum wage worker. While cognitive skills have a higher influence on wages, they have similar effects on being a civil servant, but no effect on employment in the private sector.

The abstract of the second essay is as follows. Different from previous studies using

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the wage distribution, this study uses monthly personnel administrative data of Taiwan to provide the firm-level and individual-level evidences to pin down the minimum wage effect on the earning inequality. Considering all the controversial settings in the minimum wage literature, including the methodology and the studied target, this study proposes a novel approach that uses the regression discontinuity design to estimate both employment and wage effects on firms and specific workers of different wage groups. The results show that workers bound by the minimum wage have their employment and wages increased, while workers with slightly higher wages than the new minimum also have their employment increased but their wages are lowered than they could be if there was no policy change. As a combining result of the employment and wage effects, the bound workers have their real earnings increased, while the unbound workers have no significant change. Hence, it proves that the minimum wage effect has no spillovers on the earning inequality. Furthermore, the increased employment of the bound workers is not because of less employment flow, but more new hirings, which matches the expectation of the monopsony model rather than the search frictions. Finally, I also find a negative "spillover" due to the reduced workplace safety after the minimum wage increase.

Keywords: non-cognitive skills, frustration tolerance, minimum wage, employment effect, wage effect, spillover effect, wage distribution effect

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1 Does Frustration Tolerance Benefit People's Labor Market Outcomes? Evidence from Taiwan Joint College Entrance Examination

1.1 Introduction

The influential book *"Schooling in capitalist America: Educational reform and the contradictions of economic life"* written by Samuel Bowles and Herbert Gintis in 1976 has provoked researchers to move their attentions from cognitive skills toward the effects of non-cognitive skills on the labor market outcome. But until today, it is still debatable in this field due to several reasons. The most critical one is that there are variety of ways to measure non-cognitive skills in the field of human capital. Although Kautz et al. (2014) defined *skills* as something able to be learned to resolve some tasks rather than *traits* set in stone at birth and determined solely by genes, previous studies usually inherited the personal traits self-reported questionnaires from the psychological field to measure non-cognitive skills.[1] Other than the ambiguity between *skills* and *traits*, the self-reported questionnaires also raise concerns about authenticity and objectivity.[2] Alternative methods include assessment from other people or directly observed behaviors.[3] However, as pointed out by Rauber (2007), the assessment from others is usually taken by parents or teachers and also has the problem of reference bias. Even recently Lindqvist and Vestman (2011) used a psychologist's evaluation of the mandatory conscripts in Sweden but still had the problem of fake performance as pointed out in Lundborg et al. (2014). Comparatively, measures by observed behavior may be more reliable. For example, Heckman and Rubinstein (2001) used the US Generational Educational Development recipients to identify people who have lower non-cognitive skills. But, just as Lundberg (2019) mentioned, observed behaviors may not be comparable between individuals if they have different incentives to perform their behaviors. In short, the lack of a standardized task that can

[1] Such as Mueller and Plug (2006) and Borghans et al. (2008)

[2] Self-reported questionnaires may not reflect the true traits of individuals, the respondents may fake the answer due to some reasons, see Viswesvaran and Ones (1999), Zickar et al. (2004), and Griffith et al. (2007). The objectivity infers to the reference bias among people with different backgrounds, see the discussion in Harzing (2006), Lipnevich et al. (2013), and Kautz et al. (2014).

[3] Others-reported assessments usually are taken by parents or teachers, see Carneiro et al. (2007), Rauber (2007), Neidell and Waldfogel (2010), and Silles (2010). The observed behavior method is usually measured by their education performance, as in Heckman and Rubinstein (2001), Jackson (2012), and Balart et al. (2018).

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be used to measure an individual's performance related to their non-cognitive skills is the main difficulty in this field.

Another debatable issue about estimating the effect of non-cognitive skills on the labor market outcome involves the difficulty in clarifying the factors behind and after non-cognitive skills. As skills may result from family backgrounds, the estimation on the labor market outcome may be overestimated if unobservable factors in the family are not ruled out.[4] But to my knowledge, only Fletcher (2013) has done the analysis between siblings until now. Besides, as education attainment and people's cognitive skills and non-cognitive skills may affect each other simultaneously, previous studies that did estimation among people with different ages or statuses cannot easily separate those influences.[5] Furthermore, even if non-cognitive skills may have effects on the labor market outcome, they may not raise the productivity directly. Potential mechanisms through which the labor market outcome is influenced include educational attainment, job attainment, and employers' preferences.[6] To identify whether non-cognitive skills can raise productivity, evidence is needed within firms, but until today only Nomura and Adhikari (2017) provided within-firm evidence.

Under consideration of all the issues above, this study proposes an approach that uses the difference between a individual's two test performances in the joint college entrance examination in Taiwan to measure the tolerance for frustration, which could be seen as a non-cognitive skill. This method is not only based on an observed behavior approach but also meets the need of a standardized task, and provides estimation on the labor market outcome within family and within firm. To my best knowledge, this is the first study that covers all the issues above. All the works are built on several administrative data, including Taiwan college entrance examination records, birth registration, labor and employment insurance records, and civil servant insurance records.

More specifically, the examinees can apply to colleges through the Recommendation and Screening or directly apply to colleges by submitting their scores on the General Scholastic Ability Test, which is held in January, and other required documents and oral tests. If

[4] For the influence from family can see Heckman et al. (2006), Björklund and Jäntti (2012), and Fletcher (2013).

[5] The correlation between cognitive skills and non-cognitive skills can be found in Schermer and Vernon (2010), and the influence from education can be seen in Heckman et al. (2006) and Meghir et al. (2018).

[6] For example, de Araujo and Lagos (2013) and Lundberg (2019) found non-cognitive skills have influence through education attainment. Rauber (2007), Lindqvist and Vestman (2011), Sahn and Villa (2015) and Glewwe et al. (2017) found job attainment as a mechanism. Heineck (2011), José Díaz et al. (2013) and Cunningham et al. (2016) found employers have preferences toward some traits.

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they fail in the application, they need to take Advanced Subjects Test, which is held in July, and then be distributed to a college and department by their scores and preference. I propose that those people's performance difference between the two tests reflects their tolerance for frustration, which is related to their emotional management, motivation, and perseverance after they experience a big frustration.

By taking advantage of the administrative longitudinal personnel data, this study estimates those cohorts' labor market outcomes at age 26 to 30, which is the period 8 to 12 years after the tests. After considering all the potential confounders including the birth information, family background, and high school status, and also the mechanisms including the college and department the examinees go to after the tests and the job matching, I find an increasing direct influence of the tolerance for frustration on employment in the private or public sector by age, but no direct effect on wages or being a minimum wage worker. While cognitive skills have a higher influence on wages, they have similar effects on being a civil servant, but no effect on employment in the private sector.

The rest of this study is organized as follows. Section 2 describes the related literature and is separated by the issues in this field. Data sources and the related background are described in Section 3. The empirical framework, the measure for the skills, and the data setting are presented in Section 4. Section 5 shows the estimated results, and Section 6 concludes and discusses.

1.2 Literature Review

Bowles and Gintis (1976) argued that the employer favors an employee having not only cognitive skills but also specific non-cognitive skills, especially in the lower-end occupations. Following this view, Carneiro and Heckman (2003) stressed emphasis on the importance of the non-cognitive skills and point out that they are more malleable than cognitive skills and should be considered as a critical field in children's development. In recent years, there is an emerging body of literature providing more and more empirical evidence supporting the common sense view that non-cognitive skills, soft skills, and personality traits do have impacts on an individual's labor market outcome. However, there are still several questions in this field, dealing with an ambiguous definition of non-cognitive skills, measurement errors of non-cognitive skills assessment, omitted variables in the estimation, and the mechanism of how non-cognitive skills affect labor market outcome. Hence, the

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story of non-cognitive skills is not clear to this day.

1.2.1 Definition and Measures of Non-cognitive Skills

First and foremost, what are the non-cognitive skill and how are they measured? In general, just like Kautz et al. (2014) mentioned, the term "non-cognitive skills" is used by economists to "describe the personal attributes not thought to be measured by IQ tests or achievement tests." Obviously, even people know non-cognitive skills are skills other than the cognitive ones; it is still difficult to directly tell what they actually are. Rauber (2007) tried to narrow down the definition of the term by introducing motivational psychologists' Rubicon-model and stating that "motivation" and "self-regulation" (Achtziger et al., 2008) are the important concepts to build up a large part of what economists describe as non-cognitive skills. However, these two concepts are still ambiguous and related to several personality traits or depend on situations people face. Duckworth et al. (2012) used five data sets from four countries (the U.K., the United States, Sweden, and Finland) and shown that the causal effects of non-cognitive skills on labor market outcome depends on how non-cognitive skills are defined. Thus, the absence of a general definition lead previous studies to have inconsistent estimation results.

Different from previous studies that wanted to directly point out what non-cognitive skills are, Kautz et al. (2014) defined the properties non-cognitive skills should have and how to measure them. To distinguish the traits and skills, Kautz et al. pointed out that "*"Traits" suggest a sense of permanence and possibly also of heritability. "Skills" suggest that these attributes can be learned.*" With these ideas, "*skills*" should be more malleable than "*traits*" and they can be learned to fulfill tasks. In short, they "*are not traits set in stone at birth and determined solely by genes.*" Nevertheless, it is not to say that non-cognitive skills are irrelevant to personality traits, actually they are correlated but share different concepts. In addition, since researchers can only measure skills by people's performance, a "*task*" should be well defined firstly and all of the potential contributors like incentives, effort, other skills, and the different characteristics between people should be standardized (Almlund et al., 2011; Kautz et al., 2014). Otherwise, the measures will be misleading. Especially, incentive is critical. Even if people face the same "*task*", they probably have different incentives. As a result, what researchers really measure is not people's skills but their incentives and related efforts.

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To this day, there are at least three kinds of methods used to measure non-cognitive skills: self-reported questionnaires, assessments from other people (like teachers, parents, or peers), and people's behaviors. However, based on the points above, those methods used in previous studies may be problematic.

Self-reported questionnaires, which are mainly about the Big Five (conscientiousness, openness to experience, neuroticism, extraversion, agreeableness), locus of control, aggression, or other traits, are convenient to be conducted in a survey and therefore served as the main method used by previous studies to measure non-cognitive skills (Mueller and Plug, 2006; Borghans et al., 2008). However, there are issues about the self-response: the incentive and the authenticity. If there is no benefit, people may have no incentive to make an effort to correctly respond. However, if the result of the assessment will lead them to gain potential benefits, they may fake the answers (Viswesvaran and Ones, 1999; Zickar et al., 2004; Griffith et al., 2007). Moreover, the respondents may be affected by "social desirability bias", that is, people may tend to respond more according to how they would like to be seen by other people rather than by how they actually are (José Díaz et al., 2013). Even if the responses are "real", answers from self-reported questionnaires can lead to a reference bias when comparing levels of non-cognitive skills across different groups of people (Kautz et al., 2014) and can be affected by answering styles. Lipnevich et al. (2013) noticed that, in general, different groups from different cultural backgrounds vary in their use of the scale (Harzing, 2006). For example, extreme answer scales are more favored by young males (Austin et al., 2006) and Hispanics (Marin et al., 1992). In brief, self-reported answers can be misleading because of the lack of incentive, authenticity, and objectivity. Hence, the measures of non-cognitive skills based on self-reported questionnaires are less precise than measures of cognitive measures (Borghans et al., 2008; Lindqvist and Vestman, 2011).

As an alternative, recent studies use assessments from other people to measure non-cognitive skills. In particular, teachers or parents are asked to answer a questionnaire to rate each individual child on their behavioral and social-emotional skills or underline some phrases describing particular aspects of behavior that apply to a child (Carneiro et al., 2007; Rauber, 2007; Neidell and Waldfogel, 2010; Silles, 2010). However, even though the methods avoid the measurement errors from children themselves, it leads to another measurement problem of how to compare all the assessments from different teachers and

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parents, as Rauber (2007) has already found that parents' judgements are not always comparable. Hence, even if the assessment is made by parents or teachers, the reference bias still exists. Lindqvist and Vestman (2011) provided a novel method by using certified psychologists' measures as the variable of non-cognitive skills. The data is from Swedish military enlistment, which is mandatory for all young Swedish men. All of the conscripts have to be interviewed by a certified psychologist to measure their ability to function in the very demanding environment of armed combat. Lindqvist and Vestman argued that this measure is more precise and objective. On the other hand, even though the measure is objective, the conscripts may have particular incentives to perform better, worse, or normal during the interview. Lundborg et al. (2014) used the same data and noticed that individuals may deliberately underperform on the enlistment test in order to avoid certain positions in the military. In other words, even though the people who conduct the assessment are more professional and objective, researchers still cannot get the correct measures of non-cognitive skills without standardizing people's incentives.

Compared to measures based on self-reported questionnaires and assessments from others, using measures of observed behaviors to capture non-cognitive skills a promising approach that avoids reference bias and has been pursued by recent studies. For example, Heckman and Rubinstein (2001) suggested that the Generational Educational Development (GED) recipients are as smart as secondary school graduates but have lower non-cognitive skills, thus they perform worse in the labor market. More recently, Jackson (2012) used four non-test score outcomes (absences, suspensions, GPA, and on-time grade progression) to create a single proxy for non-cognitive skills. Hedengren and Stratmann (2012) suggested that people who respond to more items on surveys tend to have higher cognitive and non-cognitive skills. Mendez and Zamarro (2015) measured non-cognitive skills by using cultural differences in the personality traits that children are encouraged to learn at home and the ownership of civic capital. Following the method of Borghans and Schils (2013), Balart et al. (2018) used the Programme for International Student Assessment (PISA) held by the OECD to evaluate the ability of 15-year-old students from all over the world in reading, mathematics and science. As students are randomly assigned different test sequences, they decompose the test scores of a specific country into the initial level and the decline in performance during the test. They used the initial level to be the variable of cognitive skills and the decline to be the non-cognitive skills of the specific country to see the

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correlations between the two skills and the GDP growth. Cohn et al. (2017) suggested that frequent job changes are a negative signal of non-cognitive skills. Although using observed behaviors to measure non-cognitive skills could be more objective, the behaviors may be driven by different factors. Similar to Kautz et al. (2014), Lundberg (2019) pointed out that if observed behaviors depend not just on skills, but also on contexts or the environments that people face, then difficulties arise in comparing non-cognitive skills across groups. To extend this view, there is still a lack of adequate non-cognitive skills measurement that can categorize most people into one group facing the same environment and having the same incentive.

1.2.2 Confounders with Non-cognitive skills

Concerns about estimating the effect of non-cognitive skills on the labor market outcome include the difficulty to exclude other confounders. In this part, intergenerational transmission and the correlation between non-cognitive skills, cognitive skills and education are substantial issues.

1.2.2.1 Family Backgrounds

Researches have shown that not only cognitive skills but also the personality traits are heritable.[7] The estimates of heritability of intergenerational transmission of the cognitive skills commonly range from 50% to 80%, while that of the personality traits range from 20% to 50% (Johnson et al., 2009; Anger and Schnitzlein, 2017). When fixing the measurement errors in ability assessment, the intergenerational transmission of non-cognitive skills are as high as the transmission of cognitive skills (Grönqvist et al., 2017). As Osborne (2001) and Blanden et al. (2007) have shown, the high correlation between parents and a child's personality traits or non-cognitive variables accounts for about 10 percent of the intergenerational income persistence. In other words, parental non-cognitive skills are not just related to a child's skills but also to the child's adult earnings through heritability or other channels.

In a family, besides the heritability, a child's non-cognitive skills may be influenced by parental socio-economic status (Fletcher and Wolfe, 2016; Gregg and Washbrook, 2011), non-socio-economic factors of family (Bjorklund et al., 2010; Anger, 2012), parental inputs

[7] Even though personality traits and non-cognitive skills may have different concepts, they still may be correlated with each other.

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and related factors like family size and birth order (Cunha and Heckman, 2008; Silles, 2010; Lehmann et al., 2012). Since those factors may also have impacts on a child's adult earnings, without controlling those factors, the estimated effect of non-cognitive skills may suffer a substantial bias. Unfortunately, factors related to families are not easy to collect. Researchers usually control common socio-economic background variables, such as parental education and income, and leave several omitted variables without control. Heckman et al. (2006) recognized that conventionally using test scores to measure both cognitive and non-cognitive skills may be fallible because the scores are not just affected by skills but also by people's family backgrounds, which cannot be observed and controlled, at the time tests are taken. Björklund and Jäntti (2012) tested the family effect on children's cognitive skills, non-cognitive skills, educational attainment, and adult earnings by using sibling and twin differences from Swedish register data. They concluded that conventional studies severely underestimate the role of family background.

To solve this problem, Fletcher (2013) claimed that they are the first paper using sibling differences to estimate the impacts of personality traits on employment and wage. Different from prior literature, the results show that the causal effects of conscientiousness on earnings are reduced substantially by controlling the sibling fixed effect. It suggests that the robust links found by previous studies between conscientiousness and earnings may not really reflect the impact of skills but rather reflect people's family background. As conscientiousness, like hard working, is generally considered to be a core factor of personality traits that can be used to predict people's success in education and the labor market (Almlund et al., 2011; Heineck, 2011; Humphries and Kosse, 2017; Yu et al., 2017), more studies are needed to examine whether previous findings should be adjusted because of the ignorance of family backgrounds.

1.2.2.2 Cognitive Skills and Education

Non-cognitive skills may be correlated with cognitive skills. Without simultaneous measures of cognitive skills, identification of non-cognitive skills needs to rely on a strong assumption that the two kinds of skills are irrelevant (Rauber, 2007). However, Schermer and Vernon (2010) used sibling data to create a general factor of personality (GFP) and showed that GFP is not independent from one's intelligence. As the strong assumption cannot be correct, ignoring cognitive skills leads the estimated effect of non-cognitive skills

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on labor market outcome to actually reflect the effect of cognitive skills.

Education is another factor making conventional approaches problematic. Heckman et al. (2006) suggested that schooling choices depend on cognitive and non-cognitive skills, and the schooling at the time of the cognitive or non-cognitive test affects test scores. Hence, the endogeneity of test scores, which are treated as the proxies of latent cognitive and non-cognitive skills, leads to problematic estimations. In fact, not only the test scores are affected by schooling, the latent skills are also affected by education. Meghir et al. (2018) showed that Swedish young men's development of cognitive and non-cognitive abilities were affected by the Swedish education reform. If researchers measure individual's skills and earnings at the same time, they cannot avoid the endogeneity between skills and education, thus the estimated results are biased. Even though most of the studies rely on longitudinal survey data or registered data, the measures of non-cognitive skills may be conducted at a time that respondents have already made their choices of education.[8]

To solve this problem, Heckman et al. (2006) used a latent factor model to allow the latent skills to determine measured skills and schooling choices, and for schooling to determine measured skills. Following the method introduced by Nyhus and Pons (2005) and Groves and Osborne (2005), Hartog et al. (2010) used the residuals of personality traits being regressed on gender, age, and schooling to free the impacts from those factors. Nevertheless, the most intuitive solution is still to measure people's non-cognitive or cognitive skills when they are receiving compulsory education or attending a broad educational level.

1.2.2.3 Mechanism of Non-cognitive Skills Effects

Although several evidences support that non-cognitive skills have impacts on earnings, the mechanism is not clear. The debate includes whether non-cognitive skills have a direct effect on the productivity or are effective through a channel like educational attainment or job attainment to influence the labor market outcome.

In the previous literature, non-cognitive skills are considered to directly affect the la-

[8] For example, the National Longitudinal Study of Youth (NLSY79), which is widely used by economists like Cameron and Heckman (2001) and Heckman et al. (2006), is a US nationally representative sample of 12,686 young men and women who were 14–22 years old when they were first interviewed in 1979. Other than the NLSY79, there are some papers, like Fletcher (2013), that use the data from the National Longitudinal Study of Adolescent Health (Add Health), which measures people's non-cognitive skills at age 25-30. The German Socio-Economic Panel (SOEP), which is one of the most important longitudinal survey data outside of the US, measures respondents' personality traits with their mean age being around 42 years. The data from the Swedish military enlistment of young men at age 18-19 still have problems of non-consistent schooling since primary school in Sweden typically ends the year one turns 16.

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bor market outcome by increasing productivity. For instance, Silles (2010) showed that both schooling and social maladjustment measures, which were measured in childhood, have impacts on earnings. Even when he took into account social maladjustment measures, the rate of return to schooling was not significantly changed, indicating schooling and non-cognitive skills have impacts on earnings separately and non-cognitive skills have a direct effect on earnings, at least not totally through education. However, more recently, several studies show that non-cognitive skills only have indirect impacts on labor market outcome rather than a direct effect through productivity. de Araujo and Lagos (2013) and Lundberg (2019) examined the correlation between non-cognitive skills and earnings, and provided new evidence that non-cognitive skills have strong associations with educational attainment, but only have a positive impact on subsequent labor market outcomes indirectly via education, indicating previous studies ignoring the substantial indirect effect. Similarly but not quite the same, Mendez and Zamarro (2015) found that once educational attainment is controlled, the intergenerational transmission of non-cognitive skills only has a smaller effect in labor outcomes.

On the other hand, the causal effect of non-cognitive skills on earnings is perhaps through the different behaviors of job search or work participation. Sahn and Villa (2015) found that the correlation between non-cognitive skills and adult hourly earnings does not rely on the direct effect on productivity as measured by earnings, but lies mainly in the indirect effect through the age of labor market entry and employment sector selection. Rauber (2007) showed that males' wage increase with self-regulation is partly due to selection into more prestigious jobs, and female's income increase with self-regulation is a result of working more hours. Glewwe et al. (2017) also found that non-cognitive skills affect people's labor force participation. Several studies show that non-cognitive skills have impacts on job search, and hence play an important role in unemployment probability and duration of unemployment.[9]

From another aspect, several studies use demand-side surveys to show that employers most value socio-emotional skills rather than cognitive skills (Mourshed et al., 2012; Cunningham and Villaseñor, 2016), and employers apply thresholds of non-cognitive skills in their hiring decisions (Protsch and Solga, 2015). It can be said that not only do people

[9] See Caliendo et al. (2014), Hilger et al. (2018), Lindqvist and Vestman (2011), Zhang (2012), Lee et al. (2015), and Marečková and Pohlmeier (2017). More specifically, Krassel and Sørensen (2015) found that non-cognitive skills affect the employment rate for workers with high cognitive and low non-cognitive skills.

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who have higher non-cognitive skills put more effort into finding a job, but employers also love to hire them. Thus, it is easier for them to get a job with higher wages. An interesting finding from previous studies is that agreeableness, a good personality trait seen as one of the non-cognitive skills related to teamwork and favored by employers (Cunningham et al., 2016), cannot help but actually be harmful for employee's earnings while other traits like emotional stability can help (Heineck, 2011; José Díaz et al., 2013). Nomura and Adhikari (2017) firstly used the within-firm data to examine the correlation between grit, agreeableness, emotional stability, and wages. They found that only emotional stability is positively correlated with wages while grit and agreeableness are negatively correlated with wages. While agreeableness is favored by employers, and grit is found to be helpful to wages without controlling firm factors (José Díaz et al., 2013), the findings from within-firm estimation and previous studies may indicate an interesting explanation. That is, employers do favor some personality traits such as grit and agreeableness; those traits can help people to get a job or even one with higher wages, but employers will not reward them more after hiring.

The phenomenon can be explained by two hypotheses. One is that because of the inefficiency of the labor market, especially after hiring, even though those traits can increase productivity, it will not be reflected on the wages. However, this hypothesis cannot hold if cognitive skills still have impacts on wages within a firm. Another one is that employers favor those traits because they can help employers to reduce some costs, for example depressing wages, or gain some external benefits which cannot be clearly attributed to specific employees. Therefore, employees with higher non-cognitive skills will not have higher wages but employers can increase their profits. Although Nomura and Adhikari (2017) have already provided within-firm evidence, unfortunately they do not present the results of cognitive skills, so we cannot check the hypotheses. In addition, the more serious problem is that the survey data they use, which obtained the self-reported assessments of non-cognitive skills and wage information at the same time, may cause adverse causality and problems from self-reported assessments. Hence, new within-firm evidence based on adequate observed behaviors is still needed.

As there are more and more studies promoting to improve the development of non-cognitive skills in education systems, to clarify whether the employee can benefit from higher non-cognitive skills through higher productivity or only the employer can gain more

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is an important issue.

1.3 Data Source

The dataset used in this study is matched by several administrative data sources. The one used to measure the cognitive skills and the frustration tolerance is Taiwan College Entrance Examination records. Information on family backgrounds is from the birth registration, and the information related to the labor market outcome is from the record of the labor insurance anda civil servant insurance. More specifically, the dataset is tracking the cohorts who were born in the period from 1981 to 1985 to see their college entrance examination scores during 2000 to 2003 and their labor market outcomes at ages 26 to 30 years old. Each data source and the related background is described as follows.

1.3.1 Taiwan College Entrance Examination Records

The compulsory education in Taiwan covered elementary school to middle school during the period of 1968 to 2014. So the data only contains cohorts who at least have a high school diploma and tend to go to a university/college. But, in Taiwan having a higher education degree is not so unique. In fact, there were about 380 thousand people born per year during 1981-1985, 110 thousand of them went to university/college. Thus, the data I use at least covers about 30% of the population born in 1981-1985.

Other than the university/college system, in Taiwan there is another similar education system focusing on technology rather than academy. 170 thousand of those cohorts went to an institute of technology, most of them were from vocational high schools. Unfortunately, the data I use, Taiwan College Entrance Examination records, does not contain the information about the entrance examination for institutes of technology. However, as vocational high schools and institutes of technology have the same education degree as the ordinary high school and the university/college, they actually have the same education (most of the time researchers use the schooling years to represent the education). If all the other differences between cohorts in the two systems can be explained by family backgrounds, then the estimate from the university/college system can be extended to 70% of the whole population when I control the family fixed-effect.

The College Entrance Examination records include two different test scores. The first one is from the General Scholastic Ability Test (GSAT), which is conducted in January and

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taken during the last year of high school, and the other one is from the Advanced Subjects Test (AST) that is taken in July of the same year. The GSAT contains the subjects of Chinese, English, Mathematics, Social Science, and Science. Similar to the GSAT, the AST also contains those subjects but separates Social Science into two subjects, History and Geography, and separates Science into three subjects, Physics, Chemistry, and Biology. Comparing to the GSAT, students have gone through one more semester when the AST is conducted, so the coverage of the AST is slightly larger than the GSAT.[10]

These two tests are related to three ways for entering college. GSAT scores are used for participating in Recommendation and Screening (RS) and to apply to universities directly. Compared to applying directly, through RS a candidate can use their high school's recommendation, and there were more quotas for students from RS than from directly applying at that time. The AST is used for the final distribution, which is based on the scores and the examinees' preferences. The quotas were much higher for the distribution than for RS or the direct application.

The main incentive for the examinees to go through RS or the direct application is that they are based on more than just one examination score. As the final distribution only relies on AST scores, students who are not extremely good at standardized tests but have other academic traits that are may go to a better school through the application process than they could go just based on the test scores. One of the keys that lets students, no matter how good they are at tests, have the opportunity to apply to a better school is the same limited quotas for high schools to recommend their students to a specific department and school. As the quota for every high school is limited, students with higher GSAT scores can be recommended to apply to a better school or department. So, GSAT scores influence the schools that students can be recommended to apply and of course the application result. To prove this academic skills, examinees who participate in RS or the direct application need to prepare several application documents, such as an autobiography, a recommendation letter, a study plan, high school grades, and other certificates of capability. The application may involve more than paper work; some schools also require oral tests. If their applications are not approved, they still can go through the distribution by taking the AST. The final

[10] There are some differences between the GSAT and the AST. The GSAT contains five subjects including Chinese, English, Mathematics, Social Science, and Science. The AST contains Chinese, English, Mathematics (two kinds, one is for non-science fields and one is for science fields), History, Geography, Physics, Chemistry, and Biology. Examinees can choose the subject tests to take. The scoring for the GSAT is based on rank of participants' test performances rather than "real" scores, while the scoring for the AST is by scores.

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distribution only relies on the AST scores and examinees' preferences; examinees do not have to provide any documents or attend any oral test.

During the data period, about half the students did not participate in RS or apply to schools directly. One of the main reasons should be the risk. If they fail application, it means that they also wasted time that could have been used to prepare for the AST rather than for the documents that are not needed for the final distribution. In addition, the frustration from the frustration may influence their performance on the AST. On the other hand, students who do the application will do their best on the GSAT to avoid such failure. Between the test year 2000 and 2003, there are about 65 thousands students per year participating in RS or making direct applications; 88% of them failed and 70% kept preparing for the AST. Those people were likely to suffer from feelings of frustration when they prepared to take the AST.

1.3.2 Birth Registration, Labor and Employment Insurance, and Civil Servant Insurance

The birth registration provides information of cohorts' birth and family backgrounds including birth weight, parity, and parents' years of schooling. As a contribution to the literature, through the identification of family, this study can further control the family fixed-effect, which can rule out the potential factors related to family backgrounds. To my knowledge, in the previous literature only Fletcher (2013) has estimated impacts of skills while fixing the family effect, and he finds some inconsistent results that should be rechecked.

The labor market outcome is from the records of the labor insurance and employment insurance, which are run by the government. According to the law, employers with at least five employees are required to have their employees join the labor insurance; companies registered with obligations to file tax returns are required to have their employees enrolled in the employment insurance. Hence, only the employees who are hired by firms with less than five employees and also having no obligation to file a tax return at the same time are not in the labor or employment insurance.[11] Other than the private sector, the information of being a civil servant is froma civil servant insurance. Considering that parents from the

[11] As workers who have no certain employers or are hired by firms with less than five employees still can enroll in the labor insurance on their own and set their insured wages by themselves, they are not included in the dataset.

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public sector may have specific effects on their children's education, I also matcha civil servant insurance data to check parents' employment status.

More specifically, through the labor/employment insurance data, I can generate a longitudinal data tracking workers' insured wages and their employers. The insured wage is set by levels. The insured wage level would be the one closest to and not less than the actual wage. The critical restriction of this data is that there is a ceiling of 43,900 New Taiwan Dollars (NTD) per month during the sample period. If an employee's actual wages are higher than this number, then their insured wages are still on that level. Hence, we may underestimate the skills effect on the wages. On the other hand, the lowest level for a full time employee is the monthly minimum wage. Other than the wage information, the employment status is based on employees' existence in the record of insurance. As the insurance is mandatory, if individuals are missing in the record then it means that they are not employed in the private sector. The employment status is also supplemented bya civil servant insurance.

The outstanding part of the labor/employment insurance data is that it contains the identification of the firms, thus we can control the firm characteristics. For example, the number of employees in a firm at a particular time can be calculated to represent the firm's scale, and also the industry of a firm can be observed. A further way to use the data is to control the firm fixed-effect, which can provide the with-firm estimates.

1.4 Methodology and Data Setting

As described above, the challenges of estimating the effect of non-cognitive skills on the labor market outcome include how to measure it and how to avoid other factors' influences. In this section, I firstly show the empirical model that aims to rule out other potential confounders with the frustration tolerance, and then in the data setting part I show the strategy of defining the frustration tolerance by the College Entrance Examination Records, which meets the needs of the empirical model.

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1.4.1 Empirical Framework

Considering the omitted variables that may occur in the estimation, the basic OLS model could be set as Eq. (6).

$$y_i = \alpha + \beta_c cog_i + \beta_n tol_i + F_i\delta + X_i\theta + \epsilon_i \tag{1}$$

y_i is the labor market outcome of person i, such as employment status or wages. cog_i is person i's cognitive skills, and tol_i is the frustration tolerance of persons. F_i is a vector representing variables related to family backgrounds, while X_i represents other characteristics of person i. As mentioned before, education may have effects on both types of skills and also the labor market outcomes, and it could also be the consequence of skills. As a result, in previous studies, it is hard to rule out the influence from education (Heckman et al., 2006; Meghir et al., 2018). However, in the case here, as all the examinees are at the same stage of schooling, the only variations about their education are after these skills are measured. The coefficients β_c and β_n show the estimated effects of the two skills. They include direct and indirect effects from the skills, as no more variables after the examination are controlled in this equation.

Notice that although skills may change over time, in practice those skills are measured by some proxies, hence it is hard to measure their real changes. Thus, it is hard to set the model as a framework of panel data. In other words, this model only uses the variation between individuals to estimate the effect of skills. Besides, as the labor market outcomes can vary over time and are easily observed, an alternative setting is to use y_{it} as the dependent variable. However, as the skills are only descriptive on the individual level, the estimated result would reflect the skills effect on the average outcome during a period. A more ideal method is to set outcomes at different times as the dependent variables on the left hand side, for example, the wages at different ages. Then it can let us know the skills effects on the different period outcomes.

As there may be some factors related to the family but unobservable, one way to further rule out the influence from the family is to control the family fixed-effect, as shown in Eq. (7).

$$y_i = \alpha + \beta_c cog_i + \beta_n tol_i + \delta_f + X_i\theta + \epsilon_i \tag{2}$$

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δ_f is the family fixed-effect, which replaces the F_i in Eq. (6). The difference between the two models is that Eq. (7) estimates the variation within-family. That is, it estimates the differences of the labor market outcomes and skills between siblings. The cost is that it neglects the individuals who are alone in their families. It is actually a trade-off, but as there is concern that the skills may just reflect the influence from family backgrounds, this cost would be necessary. Since the effects of skills on the labor market outcome may not reflect their productivity but their education attainment and behaviors for searching jobs, factors of college attainment and firm characteristics should be included in the framework, as in Eq. (8).

$$y_i = \alpha + \beta_c cog_i + \beta_n tol_i + \delta_f + X_i\theta + \gamma edu_i + J_i\lambda + \epsilon_i \tag{3}$$

edu_i is the department and the university that person i goes to after the examination. J_i is the vector of the firm information. As it is not a panel data framework, the firm information is similar to a characteristic along with the individuals. But as the firm information can only exist when people are employed, it means that Eq. (8) is conditional on workers' employment. Similar to the family effect, some unobservable factors related to the firm may still influence the results. Hence, a firm fixed-effects model is needed.

$$y_i = \alpha + \beta_c cog_i + \beta_n tol_i + F_i\delta + X_i\theta + \gamma edu_i + \lambda_j + \epsilon_i \tag{4}$$

Eq. (9) shows the firm fixed-effects model, in which the λ_j replaces the vectors of firms' variables. Although in theory both the family and firm fixed effects can be controlled at the same time, in practice it is hard to have the siblings working in the same firm. As a result, I only control the vector of family information here rather than the family fixed effects. The coefficients β_c and β_n show the skills effects on the workers in the same firms, so the effects on job matching are ruled out. If the coefficients are different from null, it means that employers indeed give higher wages to the workers who have higher skills. It would be closer to the concept of a higher productivity.

Just as Krassel and Sørensen (2015) showed that the non-cognitive skills only affect the employment rate for workers with high cognitive and low non-cognitive skills, in this study the skills effect may not be linear and may be specific under a specific combination of two skills. To take the phenomenon into concern, Eq. (5) shows the nonlinear model with

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controlling family fixed-effects, education afterwards, and the vector of firm information.

$$y_i = \alpha + \beta_1 h_i^c + \beta_2 l_i^c + \beta_3 h_i^t + \beta_4 l_i^t + \beta_5 h_i^c h_i^t + \beta_6 h_i^c l_i^t + \beta_7 l_i^c h_i^t + \beta_8 l_i^c l_i^t + \delta_f + X_i\theta + \gamma edu_i + J_i\lambda + \epsilon_i \tag{5}$$

In this model, the continuous variables of skills are replaced by dummy variables. h_i^c and h_i^t represent whether the individual has the higher cognitive skills and the frustration tolerance, respectively; l_i^c and l_i^t represent the lower status of the two skills. The interaction terms are employed to see whether there are any effects under specific combinations. In particular, for example, the total effects of having high cognitive skills are the summation of $\beta_1, \beta_5, \beta_6$, the last two only contribute when there is a specific tolerance for frustration; the total effects of having high tolerance for frustration are the summation of $\beta_3, \beta_5, \beta_7$.

1.4.2 Skills Measure and Data Setting

1.4.2.1 Skill Measure

The most challenging part in this field should be how to measure the skills. Based on the college entrance examination records, we can measure the skills by behaviors rather than self-reported questionnaires or assessments from others. In addition, if we think the skills as something that people can learn to deal with some tasks, the joint examination provides a perfect measure for them. First of all, all the students who want to go to a college need to take a test. Second, all the examinees have the same *incentive* to have higher scores in the tests, as it is no doubt that going to a better college is important to the individual's development. Third, the *task* they take is the same for all the examinees and the score is objective.

As there are two tests, I propose the performance of examinees who choose RS or the direct application via the GSAT as the proxy of the cognitive skills, and the difference between the performance on the GSAT and the AST as the proxy of their frustration tolerance. This setting shares a similar concept as the approach of Borghans and Schils (2013) and Balart et al. (2018). They proposed that the initial performance on the test reflects the cognitive skills and the decline afterwards reflects the non-cognitive skills, which are related to motivation and ambition. Unfortunately, they only can evaluate it on the country level. In this study, examinees who apply to colleges through RS or the direct application need to take the GSAT, which is held in January. If they fail in the

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application, they need to take the AST, which is taken place in July. That is, examinees who fail in the application have to take both tests, and the performance in the latter one is conditional on their previous failure. An intuitive thinking is that two examinees who have the same GSAT score and fail in the application may have different performances on the AST, when one has higher skills of emotional management, motivation, and perseverance than the other.

A concern with this interpretation is that the difference might be related to the coverage or the difficulty of the test. If it was the case, then the performance difference would still be the consequence of the cognitive skills rather than the frustration tolerance. Several points can be helpful when pondering the concern. First, students who attend the RS and the direct application have incentive to have higher scores on the GSAT. It means that their performance on the GSAT should sufficiently reflect their cognitive skills. Second, compared to the GSAT, the AST only covers one more semester of content. Third, after students who choose RS or the direct application take the GSAT, they need to prepare the documents and possibly prepare for an oral test. The result may be announced weeks to months later depending on the college and department to which they apply, so they actually have just a few months to prepare for taking the AST when they know their failure. Hence, only if their cognitive skills significantly change in a very short time, could the concern be valid. Instead, the performance difference under the similar content and the very short time is more likely to show their tolerance under a big failure.

As scoring methods of the GSAT and the AST are different, I use percentile rank (PR) of individuals' scores to replace the original scores of each subject in the two tests to make them comparable. For example, one will have a PR close to 100 if he has a highest score in the selected cohorts, while one can have a PR close to 0 if his original score is lowest.[12] If one has a PR of a on the GSAT and a PR of b on the AST, then the difference between these two tests is $b-a$. Although the GSAT and the AST have different subjects, both of them have English and Mathematics, which are needed for most of the colleges. Assume that the performance in the two subjects reflects the latent skills, I then generate a proxy of the cognitive skills by using the factor analysis through the PR in the GSAT English

[12] As I restrict the cohorts to those who choose RS or the direct application but fail and take the AST later, the PR rank is calculated in this sample rather than the whole cohorts. Because the test difficulty may vary in different test years, the PR is calculated by particular years. Hence it may happen that examinees actually have a higher ability but a lower PR than some others who take the test in other years. To fix this problem, I control the test year fixed-effects in estimation.

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and Mathematics. Correspondingly, I also generate a proxy of the frustration tolerance through the factor analysis with the PR change in the two subjects. In this way, having high cognitive skills means having higher scores than others rather than just having high scores; having high frustration tolerance means that people outperformed a larger percentage of people on the AST than did on the GSAT.[13]

1.4.2.2 Data Setting

Although examinees who do not apply to colleges through RS or the direct application can also take the GSAT, the approach I propose is to evaluate the frustration tolerance by restricting cohorts to the examinees who apply to colleges through RS or the direct application but fail and then take the AST. A concern about the limitation is that the restricted cohorts may be different from the total cohorts in the score distribution. Take the test year 2002, the middle year of the sample period, as an example. There were about 104 thousand students taking the GSAT, but 59 thousand participate in RS or making direct applications. Among the 59 thousand, 51 thousand failed and 40 thousand continued and took the AST. In other words, I limit the sample from 104 thousand to 40 thousand. Figures 11 to 4 show the PR distribution in the GSAT English and Mathematics in 2002.

Figure 11 shows that PR distributions of examinees who applied to college via RS or edirect application. In order to compare the groups, the PR is calculated on the basis of total examinees here rather than within their own groups. It shows that examinees who did not apply to schools through RS or the direct application actually have a higher proportion in the higher tail of the distribution than people who applied to schools. The reason is that some top students cannot apply to their preferred departments. As RS is the main way to apply to schools, but every high school has a limited quota to recommend students to a particular department, some students from top high schools may be crowded out by other top students in the same high school. Since those students cannot apply to their preferred departments, they would rather prepare for the AST directly.

Figure 2 shows the PR distribution of the examinees who applied successfully and

[13] In the AST there are actually two kinds of mathematics tests, the PR is calculated respectively in this study. That is, although the GSAT has only one mathematics test, I cut off two groups. One is the examinees taking the harder AST Mathematics; the other is examinees taking the easier AST Mathematics. Their PR for the Mathematics in the GSAT is also calculated based on their group only but not on the whole sample. If examinees take both AST Mathematics tests, then the easier one, which is also the one more students take, will be used.

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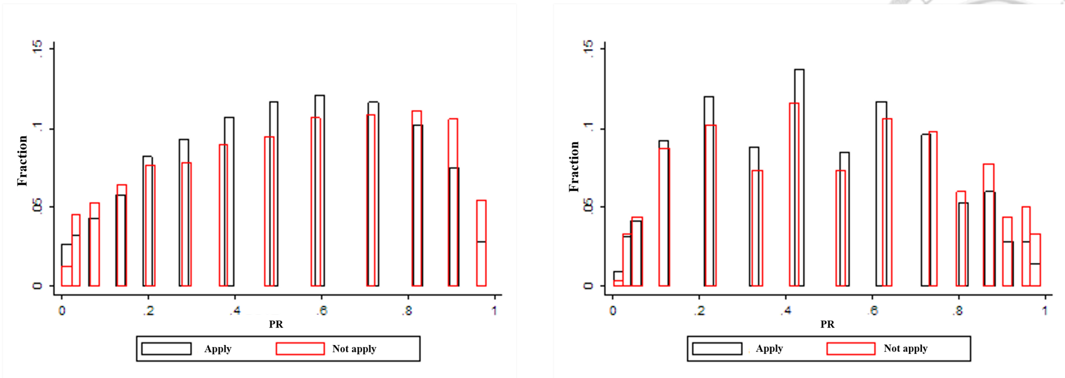
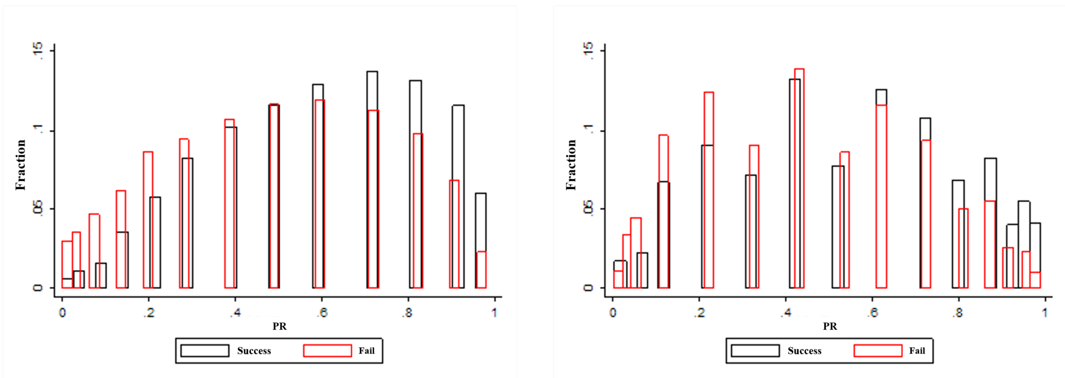


(a) English (b) Mathematics

Figure 1: PR Distribution in 2002 GSAT (Applying Schools or Not)

Notes: The figure shows the PR distribution of two groups of 2002 GSAT examinees. One is the examinees who applied to schools through RS or the direct application, and the other examinees making the other group. The PR is derived on the same basis for both groups, which is the total number of examinees. The PR is discrete because every subject in the GSAT has only fifteen scores, ranging from 1 to 15.



(a) English (b) Mathematics

Figure 2: PR Distribution in 2002 GSAT (Application Approved or Not)

Notes: The figure shows the PR distribution of two groups of 2002 GSAT examinees. One is the examinees who applied to schools through RS or the direct application and succeeded, and the other is those whose applications failed. The PR is derived on the same basis for both groups, which is the total number of examinees. The PR is discrete because every subject in the GSAT has only fifteen scores, ranging from 1 to 15.

the examinees whose applications were rejected. It is clear that examinees who applied successfully have higher proportions in the higher tail. It means that examinees who have higher GSAT scores are more likely to succeed.

After examinees fail in their applications, they can choose to take the AST a few months later so to have the final chance in the distribution or they can choose to give up. Figure 13 shows the PR distribution of the examinees who make different choices after their failure

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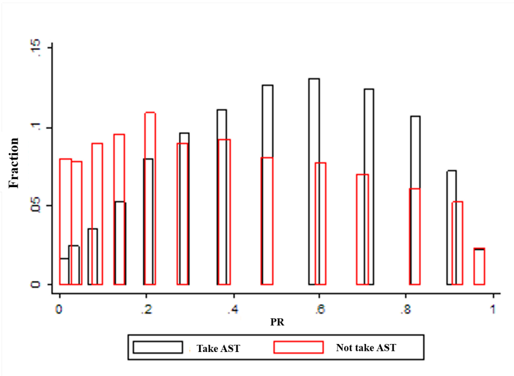
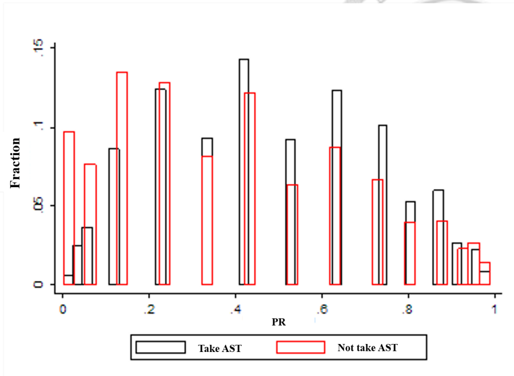


(a) English

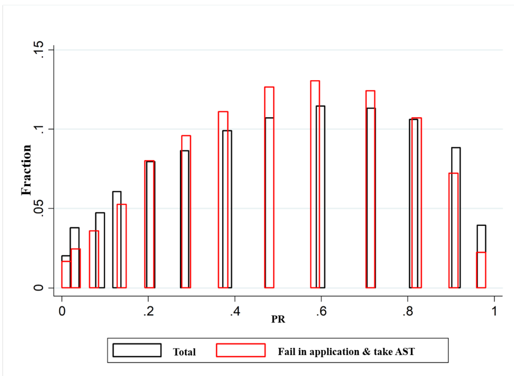


(b) Mathematics

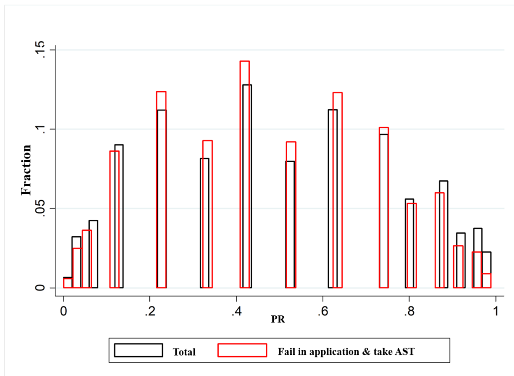
Figure 3: PR Distribution in 2002 GSAT (Take AST or Not)

Notes: The figure shows the PR distribution of two groups of 2002 GSAT examinees. Examinees in both groups applied to schools through RS or the direct application but failed. One group of examinees took the AST, while the other did not. The PR is derived on the same basis for both groups, which is the total number of examinees. The PR is discrete because every subject in the GSAT has only fifteen scores, ranging from 1 to 15.

in their applications. The difference between the two distributions is obvious, examinees who have lower scores are more likely to choose to give up.



(a) English



(b) Mathematics

Figure 4: PR Distribution in 2002 GSAT (Total and Analyzed Sample)

Notes: The figure shows the PR distribution of two groups of 2002 GSAT examinees. One is the total examinees; the other is the examinees who applied to schools through RS or the direct application but failed and then took the AST. The latter one is the research sample in this study. The PR is derived on the same basis for both groups, which is the total number of examinees. The PR is discrete because every subject in the GSAT has only fifteen scores, ranging from 1 to 15.

Because some top students do not apply to schools, applicants who have higher scores are more likely to succeed, and examinees in the lower tail of the distribution are more likely to give up after they fail their application, the sample I choose actually concentrates

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to the middle PRs as shown in Figure 4. In other words, through this sample selection, the estimated result would amplify the effects on the examinees in the middle part of the distribution, which would be even better if we want to see the effects on the ordinary students.

1.4.2.3 Variables Setting

As the analyzed sample is restricted, the PR should be calculated based on the restricted sample rather than all examinees. A mechanical problem follows the measure of the skills. The PR difference between the GSAT and the AST, which is taken as the proxy of the frustration tolerance, is actually constrained by the performance on the GSAT. For example, if the PR of the GSAT is α, then the PR difference just can be $-\alpha$ to $100-\alpha$. As a result, the two variables of skills are mechanically negatively correlated as Figure 14 shows.

Except for the negative correlation between the PR in the GSAT and the PR difference between the GSAT and the AST, the lowest tail and the highest tail of the PR in the GSAT seem to have a positive correlation with the difference. Thus, examinees who are at the bottom in the GSAT are more likely to have their performance exceeded by others in the AST if they have higher score. In other words, examinees who have the lowest scores in the GSAT are more likely to stay at the bottom in the AST. On the other hand, the best performing examinees on the GSAT are more likely to be the best performers on the AST. The phenomenon can be interpreted as the students who have the poorest cognitive skills may also have lower frustration tolerance, and the students who have the highest cognitive skills may also have higher frustration tolerance. As the two kinds of skills may be influenced by family factors or other factors at the same time, the phenomenon is reasonable. To rule out the mechanical correlation between the two variables, I propose to use the residual from the linear regression of the difference on the PR in the GSAT to replace the original difference. Figure 6 shows the residual of the difference and the PR in the GSAT.

Using linear regression to generate the residual keeps the positive correlation in the bottom and top of the PR distribution, which is not caused by the mechanical setting. However, this method may underestimate the mechanically negative correlation, hence the correlation between the two variables is still slightly negative in the middle part of the distribution. To further fix this problem, I then form a subgroup which excludes

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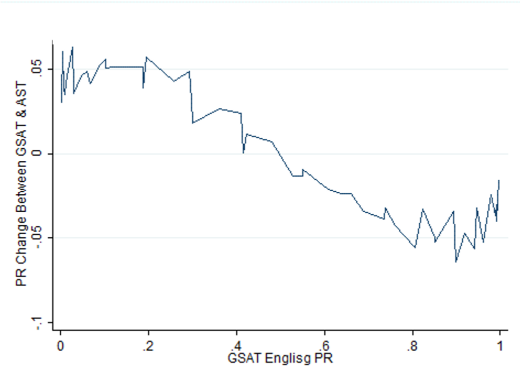
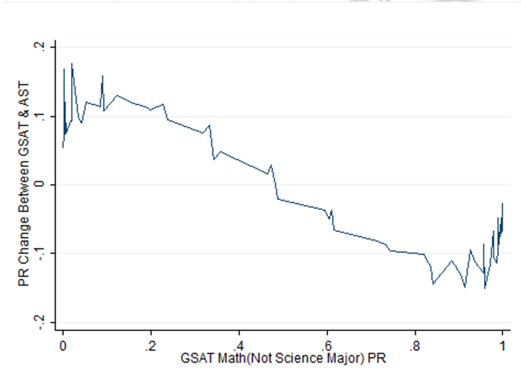
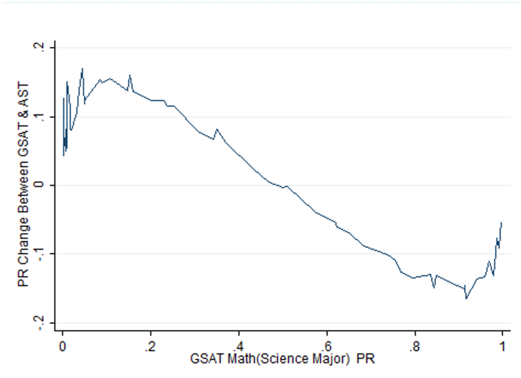


(a) English



(b) Mathematics (Non-science)



(c) Mathematics (Science)

Figure 5: Correlation between GSAT and The Difference

Notes: The figure shows the correlation between the PR in the GSAT and the PR change between the GSAT and the AST. All the PRs are derived from the corresponding sample. For example, the PR in the mathematics test for the non-science majors is based on the group of examinees who applied to schools but failed and took the mathematics test in the GSAT and the non-science mathematics test in the AST.

examinees whose PR in the GSAT is lower than 0.2 or higher than 0.8, and then generate an alternative residual which is not influenced by the bottom and top.

After the residual setting, the factor analysis for the latent frustration tolerance is employed through the residual of PR difference in the English and mathematics tests. The other one for the latent cognitive skills is through the PR in the GSAT. To evaluate the skills effect on the labor market outcomes, these two variables are standardized while estimating.

1.4.3 Data Describing

After the data setting, the descriptive statistics of the personal characteristics are shown in Table 12. The subsample of examinees whose PR in the GSAT tests is between 0.2 and

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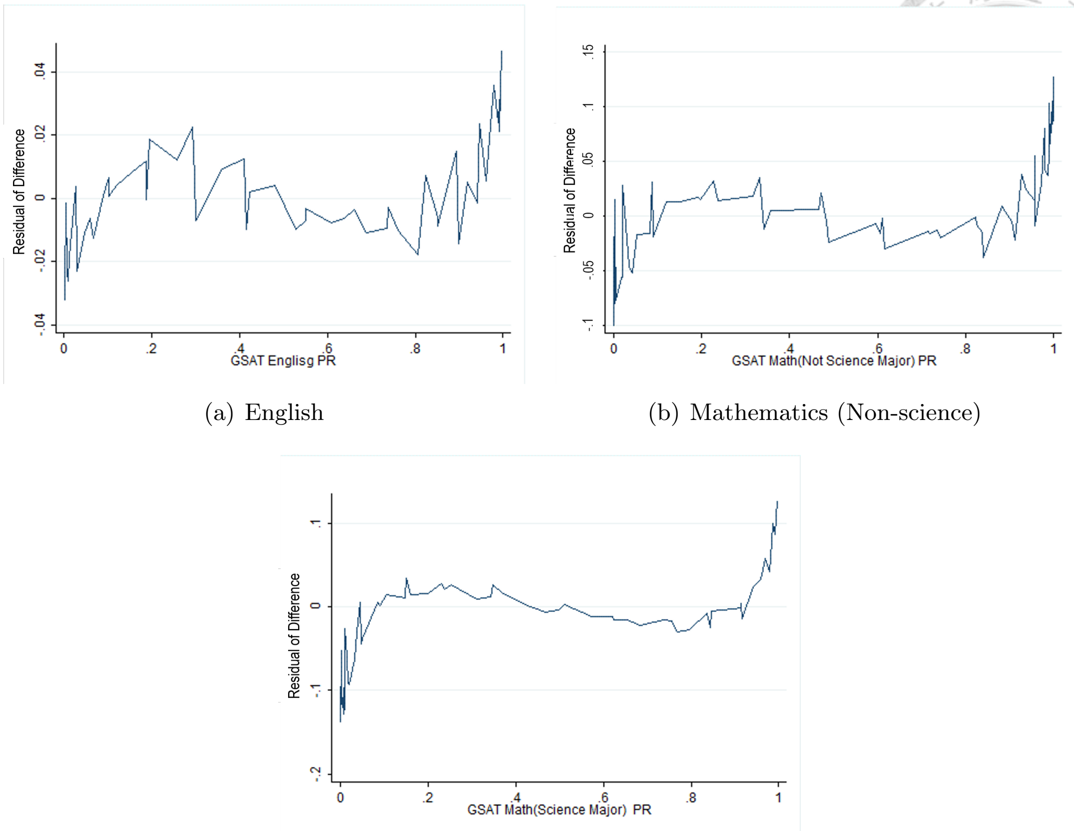

(a) English

(b) Mathematics (Non-science)

(c) Mathematics (Science)

Figure 6: Correlation between GSAT and The Residual of Difference

Notes: The figure shows the correlation between the PR in the GSAT and the residual of the PR change between the GSAT and the AST. The residual is from the linear regression of the difference on the PR in the GSAT. All the PRs are derived from the corresponding sample. For example, the PR in the mathematics test for the non-science majors is calculated in the group of examinees who applied to schools but failed and took the mathematics test in the GSAT and the non-science mathematics test in the AST.

0.8 is generated to avoid the mechanical influence causing the negative correlation between the two proxies of skills. As the proxies of skills are standardized, the mean is 0 with the SD being 1. The examinees in the subsample have lower probabilities of being from top high schools and going to public colleges since students with the highest test scores are excluded.[14] Besides, as a result of the same reason, the parents in the subsample have a slightly lower probability of being in the public sector and have fewer schooling years, since family background has a high correlation with students performance.

The labor market outcomes after the examinees leave schools are shown in Table 13. The probabilities of employment, being public servants, and wages are increasing over time. On the contrary, the probabilities of taking the monthly minimum wage are decreasing over

[14] In Taiwan public colleges are more prestigious than private ones.

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Table 1: Descriptive Statistics of Characteristics

Variable	Full sample		Middle GSAT	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Panel A. Skills				
Proxy of cognitive skills	0.000	1	0.000	1
Proxy of frustration tolerance	0.000	1	0.000	1
Panel B. Personal characteristics				
Male	0.50	0.50	0.48	0.50
Birth weight	3,284	442	3,282	440
Birth order	1.82	0.95	1.83	0.96
Top high school	0.028	0.164	0.016	0.126
Test year	2001.77	1.10	2001.77	1.11
Public college	0.25	0.43	0.18	0.39
Panel C. Family backgrounds				
Father in public sector	0.132	0.338	0.123	0.329
Mother in public sector	0.089	0.284	0.080	0.271
Father's shoolling years	11.19	3.31	11.08	3.29
Mother's shoolling years	10.14	3.28	10.02	3.25
Obs.	145,257		60,974	

Notes: Both the full sample and the subsample of the middle GSAT are composed of examinees who applied to schools but failed and then took the AST. The subsample restricts examinees whose PR in the GSAT tests is between 0.2 and 0.8. The proxies for skills are standardized. Top high schools include the best boys school and girls school in Taipei, Taichung, Tainan, and Kaohsiung, and an experimental high school at Hsinchu Science Park.

time. The subsample has higher higher probability of employment, lower probability of being public servants or minimum wage workers, and lower wages. But the difference between the two samples are slight.

1.5 Empirical Results

Except for the difficulty of measuring the non-cognitive skills, according to the literature, the estimation of the latent skills' contribution to individuals' labor market outcomes may just reflect the influence from their unobservable family backgrounds (Fletcher, 2013), cognitive skills (Schermer and Vernon, 2010), and education attainments (Heckman et al., 2006; Meghir et al., 2018). In addition, the mechanisms that non-cognitive skills may

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Table 2: Descriptive Statistics of Labor Market Outcomes

Variable		Full sample Mean (1)	Full sample SD (2)	Middle GSAT Mean (3)	Middle GSAT SD (4)
Employment	at age 26	0.628	0.483	0.643	0.479
	at age 27	0.691	0.462	0.707	0.455
	at age 28	0.724	0.447	0.739	0.439
	at age 29	0.737	0.440	0.753	0.431
	at age 30	0.741	0.438	0.756	0.430
Public servant	at age 26	0.045	0.207	0.039	0.194
	at age 27	0.062	0.241	0.055	0.228
	at age 28	0.080	0.271	0.072	0.259
	at age 29	0.095	0.293	0.087	0.282
	at age 30	0.106	0.308	0.099	0.298
Initial wage		27,521	10,508	27,294	10,090
Wage	at age 26	32,345	12,064	32,158	11,725
	at age 27	34,077	13,120	33,860	12,762
	at age 28	35,793	14,270	35,520	13,795
	at age 29	37,608	15,589	37,255	14,913
	at age 30	39,328	16,581	38,972	15,877
Minimum wage	at age 26	0.109	0.311	0.106	0.308
	at age 27	0.099	0.299	0.096	0.295
	at age 28	0.070	0.255	0.067	0.251
	at age 29	0.058	0.234	0.057	0.232
	at age 30	0.057	0.232	0.056	0.229
Obs.		145,257		60,974	

Notes: Both the full sample and the subsample of the middle GSAT are composed of examinees who applied to schools but failed and then took the AST. The subsample restricts examinees whose PR in the GSAT tests is between 0.2 and 0.8. Employment includes being a civil servant. Wages are deflated by the CPI and only can be observed under employment. Initial wage is the first observed full time wage after leaving school. Minimum wage is the dummy variable of taking minimum wage or not.

go through to affect the labor market outcome are not clear. Rather than increasing productivity directly, non-cognitive skills may have influence on education (Silles, 2010; de Araujo and Lagos, 2013; Lundberg, 2019), job searching (Sahn and Villa, 2015; Glewwe et al., 2017), or the demand side preference (Nomura and Adhikari, 2017), and hence have a correlation with the labor market outcome. Contributing to the literature, this study provides estimation by controlling the family fixed-effects, cognitive skills, education attainments, firm characteristics, and even the firm fixed-effects to clarify the factors behind

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and after the non-cognitive skills. In addition, by taking advantage of the longitudinal insured wage record in the labor and employment insurance, this study can evaluate the skills contribution to their labor market outcomes at different ages. Outcomes at ages 26 to 30, which are in the period just 8 to 12 years after the college entrance tests, are focused on in this study. To avoid the potential high correlation between cognitive skills and frustration tolerance, the subgroup of examinees whose PR in the GSAT tests is between 0.2 and 0.8 is separated for estimation. Finally, the nonlinear effects are also provided in the following.

1.5.1 Estimation on Employment

The estimation for cohorts' employment at ages 26 to 30 is listed in Table 14. The dependent variable is the dummy variable of being employed by the private sector or the public sector; the independent variables of skills are standardized proxies. Hence the coefficients give the probability change of being employed when the skills increase one standard deviation. Panel A provides the estimation based on the full sample, which contains examinees who apply to schools through RS or the direct application but fail and then take the AST. Panel B shows the results for the examinees who have a PR from 0.2 to 0.8 in the GSAT tests. As mentioned before, this subgroup can avoid the high correlation between the proxies of cognitive skills and frustration tolerance. Results show that no matter what the sample is, both skills have high correlations with employment status when the family fixed-effects are not controlled. However, the correlation is dramatically weakened between siblings. This indicates that the family background is the main reason that highly skilled cohorts have higher employment probability during their early career periods. In other words, the family backgrounds that makes the cohorts have lower skills also makes them unlikely to be employed. Studies neglecting the influence from family are lead to overestimate the correlation between skills and employment.

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Table 3: Estimates of Skills Effects on Employment by Age 26-30

	Frustration tolerance					Cognitive skills				
Age	26	27	28	29	30	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Full sample										
OLS	0.00904***	0.00902***	0.0107***	0.0117***	0.0109***	0.00549***	0.00672***	0.00962***	0.0120***	0.0156***
	(0.00128)	(0.00124)	(0.00120)	(0.00118)	(0.00117)	(0.00132)	(0.00127)	(0.00124)	(0.00122)	(0.00122)
Family FE	0.00272	-0.00120	0.00144	0.000536	0.00751	-0.00247	-0.00815	-0.00372	-0.000766	0.00865
	(0.00604)	(0.00578)	(0.00550)	(0.00544)	(0.00523)	(0.00682)	(0.00664)	(0.00638)	(0.00643)	(0.00632)
Family FE & college	0.00542	0.00145	0.00498	0.00135	0.00684	0.00228	-0.00342	0.00227	-3.77e-05	0.00694
	(0.00647)	(0.00622)	(0.00599)	(0.00593)	(0.00576)	(0.00812)	(0.00792)	(0.00775)	(0.00779)	(0.00763)
Panel B. Middle GSAT										
OLS	0.00960***	0.00886***	0.0123***	0.0130***	0.0125***	0.00502**	0.00665***	0.00909***	0.00862***	0.0102***
	(0.00196)	(0.00189)	(0.00182)	(0.00179)	(0.00178)	(0.00197)	(0.00188)	(0.00182)	(0.00179)	(0.00179)
Family FE	-0.0100	-0.0154	0.00517	0.0106	0.0284**	0.0177	-0.000482	-0.00966	-0.00565	-0.00372
	(0.0133)	(0.0124)	(0.0123)	(0.0119)	(0.0115)	(0.0129)	(0.0119)	(0.0119)	(0.0116)	(0.0121)
Family FE & college	-0.00655	-0.0140	0.00689	0.0129	0.0297**	0.0208	0.00242	-0.00638	-0.00151	-0.00172
	(0.0144)	(0.0139)	(0.0139)	(0.0134)	(0.0130)	(0.0141)	(0.0132)	(0.0132)	(0.0130)	(0.0132)

Notes: The table reports the estimates of the skills effects on employment, which is a dummy variable of 0 or 1. The coefficient shows the employment change in percentage points when skills are increased by 1 standard deviation. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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Although the family background strongly extracts the correlation, it makes the only remaining correlation, which is between the frustration tolerance and the employment status at age 30 in the middle GSAT sample, more unique. Different from other changes, the coefficient becomes even larger when comparing the difference between siblings and controlling the college. It indicates that in the subgroup even after controlling the potential influence from family and the mechanism of college, cohorts with a frustration tolerance one standard deviation higher have their employment probability increased by about 3 percentage points when they are at age 30. As the subgroup can more separate the frustration tolerance from the cognitive skills, the non-significance in the full sample means that the irrelevance between the cognitive skills and the employment let the correlation between the frustration tolerance and employment be underestimated. It is consistent to previous studies, for example, Lindqvist and Vestman (2011) found that men with low non-cognitive ability are significantly more likely to become unemployed than men with low cognitive ability. However, by the estimation here, we can find that the correlation is not fixed over time, it is increasing by age as shown in Figure 7. When it comes to the subgroup, it is clear that the correlation between the frustration tolerance and the employment is increasing from age 27 to 30. Although all the correlations are not significant before age 30, they are moving up by age. It means that the frustration tolerance has higher influence on the employment when people are older. In other words, when people are older, being employed or not is more related to their frustration tolerance rather than their cognitive skills, while both skills have no influence on their employment when they are younger.

1.5.2 Estimation on Being A Civil Servant

Being a civil servant is a special employment status. Civil servants usually have a steadier employment environment and their children are more likely to have a better education (Luoh, 2002). That is, it relates to the human capital accumulation in family. The results are shown in Table 15. Even controlling the family fixed-effects, both skills have positive correlations with this specific employment. It means that high skills do help to be a civil servant. Figure 8 shows the results after controlling the family fixed-effects.

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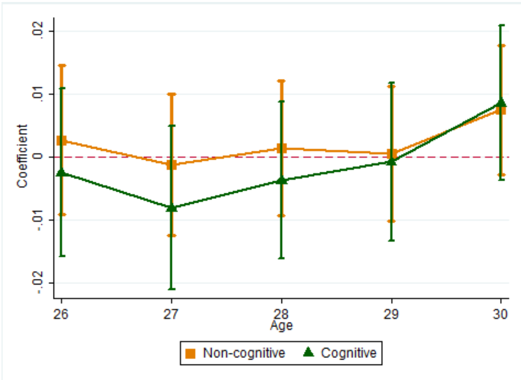
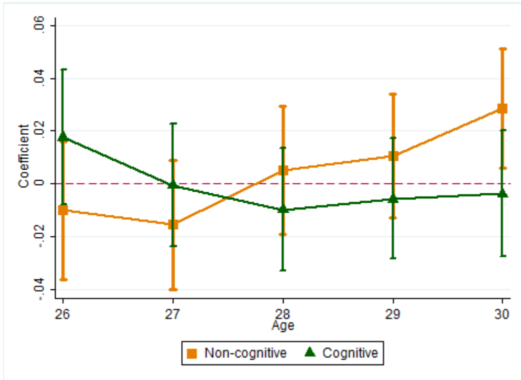
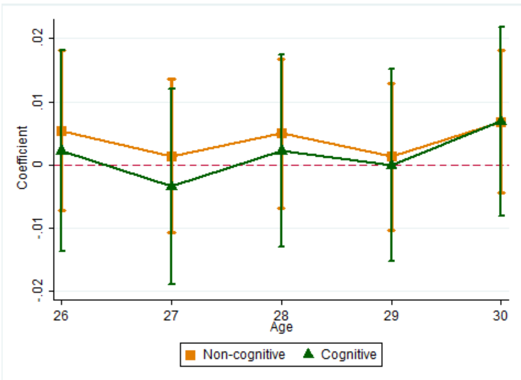


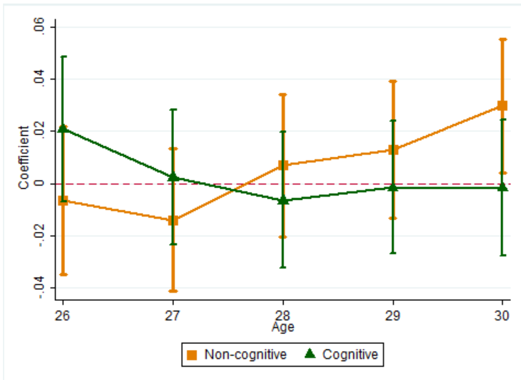
(a) Control Family FE (Full Sample)



(b) Control Family FE (Middle GSAT)



(c) Control Family FE & College (Full Sample)



(d) Control Family FE & College (Middle GSAT)

Figure 7: Estimates of Skills Effects on Employment

Notes: The figure shows the coefficients and 95% confidence intervals of the estimation under controlling family fixed-effects and the college information. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, and high school status. College information includes whether an examinee attended a public college after the tests, and the category of the department. Examinees in the full sample and the subsample of middle GSAT applied to schools through RS or the direct application but failed and then took the AST. The subgroup of middle GSAT contains examinees whose PR in the GSAT tests is between 0.2 and 0.8. The red dotted line is set on 0.

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Table 4: Estimates of Skills Effects on Being Civil Servant by Age 26-30

	Frustration tolerance					Cognitive skills				
	26	27	28	29	30	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Full sample										
OLS	0.0106***	0.0138***	0.0170***	0.0188***	0.0197***	0.0244***	0.0316***	0.0390***	0.0434***	0.0461***
	(0.000558)	(0.000643)	(0.000719)	(0.000779)	(0.000818)	(0.000589)	(0.000678)	(0.000751)	(0.000804)	(0.000841)
Family FE	0.0185***	0.0200***	0.0241***	0.0235***	0.0277***	0.0294***	0.0314***	0.0360***	0.0372***	0.0403***
	(0.00294)	(0.00337)	(0.00364)	(0.00389)	(0.00403)	(0.00335)	(0.00381)	(0.00414)	(0.00443)	(0.00462)
Family FE & college	0.0111***	0.0102***	0.0119***	0.00954**	0.0125***	0.0176***	0.0158***	0.0164***	0.0147***	0.0157***
	(0.00299)	(0.00343)	(0.00374)	(0.00398)	(0.00409)	(0.00387)	(0.00433)	(0.00476)	(0.00507)	(0.00529)
Panel B. Middle GSAT										
OLS	0.0105***	0.0139***	0.0188***	0.0218***	0.0231***	0.0124***	0.0176***	0.0221***	0.0247***	0.0262***
	(0.000856)	(0.000987)	(0.00111)	(0.00121)	(0.00127)	(0.000830)	(0.000969)	(0.00109)	(0.00117)	(0.00124)
Family FE	0.0128*	0.0143*	0.0201**	0.0271***	0.0315***	0.0183***	0.0215***	0.0241***	0.0253***	0.0262***
	(0.00695)	(0.00750)	(0.00803)	(0.00842)	(0.00842)	(0.00652)	(0.00725)	(0.00806)	(0.00830)	(0.00866)
Family FE & college	0.00694	0.00651	0.00944	0.0137	0.0172**	0.0155**	0.0165**	0.0170**	0.0163*	0.0170**
	(0.00724)	(0.00787)	(0.00861)	(0.00882)	(0.00871)	(0.00649)	(0.00713)	(0.00796)	(0.00830)	(0.00860)

Notes: The table reports the estimates of the skills effects on being a civil servant, which is a dummy variable of 0 or 1. The coefficient shows the probability change in percentage points when skills are increased by 1 standard deviation. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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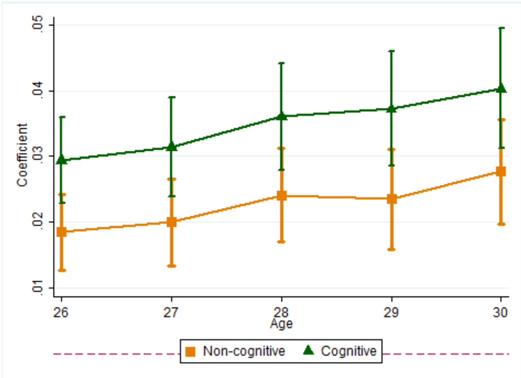
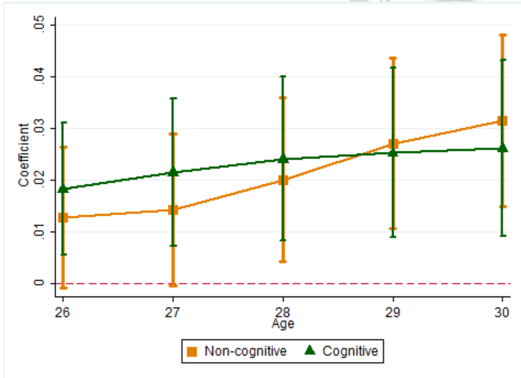
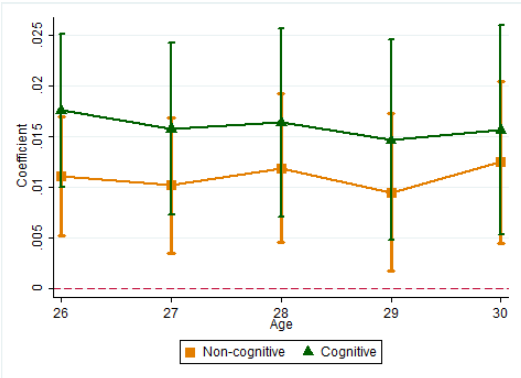


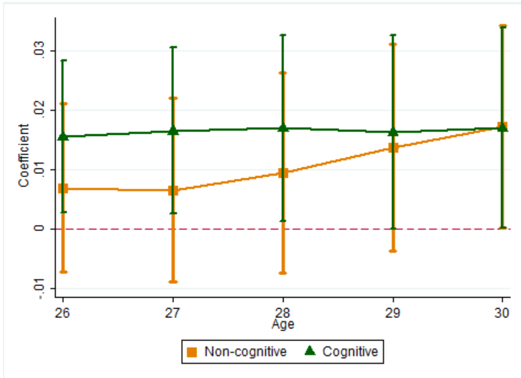
(a) Control Family FE (Full Sample)



(b) Control Family FE (Middle GSAT)



(c) Control Family FE & College (Full Sample)



(d) Control Family FE & College (Middle GSAT)

Figure 8: Estimates of Skills Effects on Being A Civil Servant

Notes: The figure shows the coefficients and 95% confidence intervals of the estimation under controlling family fixed-effects and the college information. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, and high school status. College information includes whether an examinee attended a public college after the tests, and the category of the department. Examinees in the full sample and the subsample of middle GSAT applied to schools through RS or the direct application but failed and then took the AST. The subgroup of middle GSAT contains examinees whose PR in the GSAT tests is between 0.2 and 0.8. The red dotted line is set on 0.

After controlling the family fixed-effects, although in the full sample the frustration tolerance has highly significant correlations with the specific employment, it may mix with the correlation with cognitive skills. The correlation becomes less significant when it comes to the subgroup, especially in the younger ages. Before controlling the college information, the subgroup estimation shows that one standard deviation higher in frustration tolerance leads the probability of being a civil servant to increase by 1 to 3 percentage points, which is also increasing by age. Similar to its effects on being employed, the frustration tolerance may have a higher influence on being civil servants when the cohorts are older. After considering the college and department to which the cohorts go after the tests,

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the coefficients are lowered to 0.7 to 1.7 percentage points and insignificant before age 30. It means that college is a main mechanism through which frustration tolerance has an influence on being a civil servant. But, the pattern by age keeps, showing that the frustration tolerance may still have a direct effect when cohorts are getting older.

The subgroup separates the correlation between two skills, but the cost is missing the lowest and highest tails in the GSAT distribution. For the estimation before controlling the college information, cognitive skills have higher coefficients in the full sample, 3 to 4 percentage points, than in the subgroup, 2 to 3 percentage points. Both samples have a increasing pattern by age. But the difference is lessened when considering the influence from college; then the coefficients for cognitive skills in both samples are about 1.5 to 1.8 percentage points with no obvious difference between ages. The results mean that the cognitive skills may totally have an increasing influence when cohorts' ages go up; one standard deviation higher in cognitive skills lead the probability of being a civil servant to increase by 3 to 4 percentage points. But the increasing pattern and half of the influence is from the college; only the other half is directly from cognitive skills and it is steady by age. Hence, when cohorts are getting older, only frustration tolerance can help them have more probability of being a civil servant.

1.5.3 Estimation on Wages

Table 16 shows the estimated results on wages, and Figure 32 shows the results other than those from the OLS model. As it is mixed with the influence from cognitive skills, frustration tolerance has more significant coefficients in the full sample. But when limiting to the subgroup, the correlation between the initial wage and frustration tolerance is mainly from family background. The slightly significant correlation between wages and the frustration tolerance in siblings is then explained by the college and department they attend after the tests. The frustration tolerance has no direct correlation with the wages, no matter whether it is the initial wage or wages at ages 26 to 30. On the other hand, if we only control the firm fixed-effects and not the family fixed-effects, there are still strongly significant correlations. But it turns to be insignificant from 0, once the family fixed-effects is controlled even with just some observable firm information. It means that the positive correlation with cohorts' frustration tolerance and their wages within-firm is actually due to their family backgrounds. It also means that some previous studies indicating that

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the frustration tolerance has positive influence through the job attainment (Rauber, 2007; Sahn and Villa, 2015) may actually neglect the influence from the family.

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Table 5: Estimates of Skills Effects on Log Wage by Age 26-30

	Frustration tolerance						Cognitive skills					
	Initial wage	26	27	28	29	30	Initial wage	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Full sample												
OLS	0.0297***	0.0333***	0.0334***	0.0344***	0.0356***	0.0374***	0.0866***	0.0900***	0.0915***	0.0921***	0.0941***	0.0974***
	(0.000938)	(0.00115)	(0.00114)	(0.00116)	(0.00119)	(0.00122)	(0.000966)	(0.00118)	(0.00118)	(0.00120)	(0.00124)	(0.00128)
Family FE	0.0196***	0.0215**	0.0266***	0.0274***	0.0301***	0.0309***	0.0759***	0.0878***	0.0874***	0.0821***	0.0933***	0.0983***
	(0.00499)	(0.00905)	(0.00771)	(0.00757)	(0.00805)	(0.00840)	(0.00582)	(0.00941)	(0.00867)	(0.00876)	(0.00945)	(0.00941)
Family FE	0.0114**	0.0157*	0.0241***	0.0264***	0.0276***	0.0247***	0.0547***	0.0729***	0.0754***	0.0732***	0.0845***	0.0809***
& college	(0.00522)	(0.00945)	(0.00811)	(0.00786)	(0.00862)	(0.00895)	(0.00680)	(0.0112)	(0.0105)	(0.0103)	(0.0109)	(0.0112)
Firm FE	0.0107***	0.00694***	0.00726***	0.00873***	0.00922***	0.0111***	0.0402***	0.0267***	0.0268***	0.0285***	0.0295***	0.0317***
& college	(0.00179)	(0.00128)	(0.00122)	(0.00121)	(0.00126)	(0.00128)	(0.00224)	(0.00165)	(0.00156)	(0.00155)	(0.00159)	(0.00163)
Family FE,	0.0158*	0.00900	0.0213***	0.0254***	0.0261***	0.0227***	0.0686***	0.0518***	0.0597***	0.0595***	0.0737***	0.0668***
college, & firm	(0.00940)	(0.00948)	(0.00804)	(0.00780)	(0.00854)	(0.00877)	(0.0119)	(0.0114)	(0.0104)	(0.0106)	(0.0112)	(0.0113)
Panel B. Middle GSAT												
OLS	0.0300***	0.0325***	0.0313***	0.0314***	0.0330***	0.0358***	0.0460***	0.0468***	0.0492***	0.0501***	0.0509***	0.0541***
	(0.00146)	(0.00177)	(0.00175)	(0.00177)	(0.00182)	(0.00185)	(0.00144)	(0.00176)	(0.00174)	(0.00177)	(0.00182)	(0.00186)
Family FE	0.0213*	0.0125	0.0224	0.0334*	0.0339*	0.0389**	0.0470***	0.0461**	0.0595***	0.0519***	0.0399**	0.0581***
	(0.0113)	(0.0226)	(0.0176)	(0.0177)	(0.0181)	(0.0189)	(0.0113)	(0.0179)	(0.0152)	(0.0167)	(0.0180)	(0.0173)
Family FE	0.00422	-0.00173	0.0158	0.0277	0.0144	0.0250	0.0258**	0.0231	0.0374**	0.0273	0.0165	0.0409**
& college	(0.0116)	(0.0224)	(0.0189)	(0.0191)	(0.0201)	(0.0205)	(0.0114)	(0.0184)	(0.0156)	(0.0169)	(0.0180)	(0.0175)
Firm FE	0.0119***	0.00559**	0.00440**	0.00523**	0.00583***	0.00995***	0.0198***	0.0133***	0.0152***	0.0166***	0.0155***	0.0157***
& college	(0.00321)	(0.00230)	(0.00217)	(0.00211)	(0.00223)	(0.00223)	(0.00317)	(0.00233)	(0.00217)	(0.00218)	(0.00221)	(0.00226)
Family FE,	-0.0211	-0.0168	0.0128	0.0264	0.0184	0.0146	0.0238	0.00417	0.0285*	0.0291*	0.00840	0.0419**
college, & firm	(0.0246)	(0.0245)	(0.0197)	(0.0181)	(0.0205)	(0.0205)	(0.0242)	(0.0214)	(0.0158)	(0.0171)	(0.0197)	(0.0186)

Notes: The table reports the estimates of the skills effects on log wages which are deflated by the CPI. The coefficient shows the wage changes in percents when skills are increased by 1 standard deviation. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department. Firm information includes the scale and the industry. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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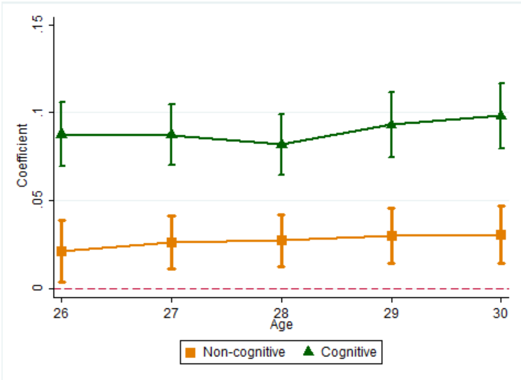
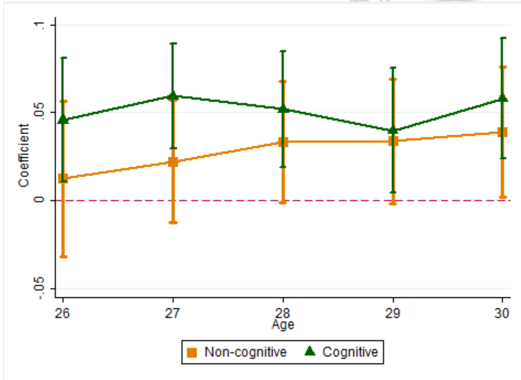
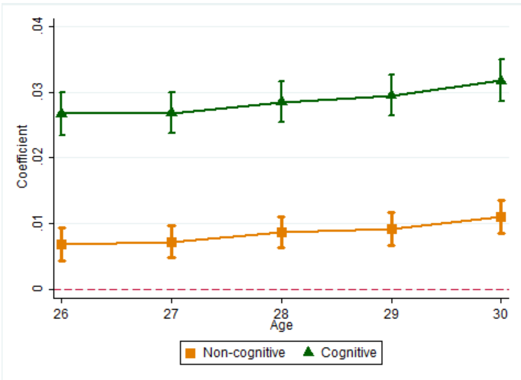


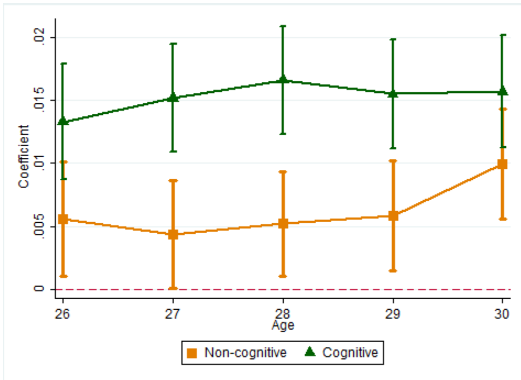
(a) Control Family FE (Full Sample)



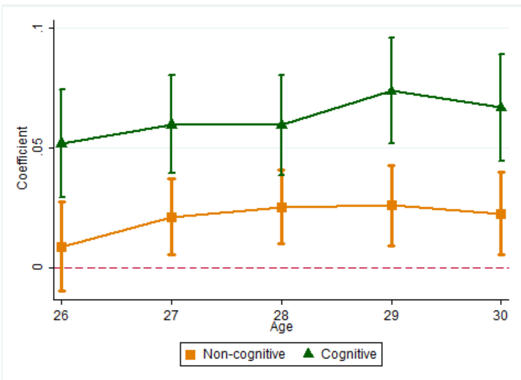
(b) Control Family FE (Middle GSAT)



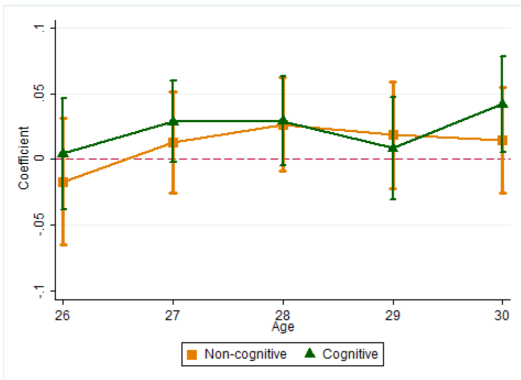
(c) Control Firm FE & College (Full Sample)



(d) Control Firm FE & College (Middle GSAT)



(e) Family FE, College, & Firm (Full Sample)



(f) Family FE, College, & Firm (Middle GSAT)

Figure 9: Estimates of Skills Effects on Wage

Notes: The figure shows the coefficients and 95% confidence intervals of the estimation under controlling family fixed-effects, college, and firm information. All the models control the test year fixed-effects and personal information including sex, birth weight, birth order, and high school status. College information includes whether an examinee attended a public college, and the category of the department. Firm information includes scale and industry. Examinees in the full sample and the subsample of middle GSAT applied to schools through RS or the direct application but failed and then took the AST. The subgroup of middle GSAT contains examinees whose PR in the GSAT tests is between 0.2 and 0.8. The red dotted line is set on 0.

Compared to the frustration tolerance, cognitive skills can more explain the wage differ-

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ence between cohorts, even if they are in the same family or the same firm. The result is in line with the finding of Lindqvist and Vestman (2011). In the full sample that contains the lowest and highest tails in the GSAT distribution, when controlling the family fixed-effects, one standard deviation higher in cognitive skills will lead cohorts' wages to be increased by 7% to 10%. After considering the mechanism of college and the firm characteristics, the influence is between 5% to 8%. However, when controlling the firm fixed-effects without the family ones, the coefficients are lowered to about 3%. Unfortunately, the data cannot support controlling the two kinds of fixed-effects at the same time. Once the family fixed-effects are controlled, the coefficients in the same firm should be lower than 3%. The lower coefficient within firms indicates that a large part of the wage difference related to cognitive skills is due to the job matching. In other words, one standard deviation higher cognitive skills can lead people to have higher wages of up to 10%, but less than 3% is contributed within firms.

1.5.4 Estimation on Being A Minimum Wage Worker

Although higher frustration tolerance is expected to help people improve their labor market performance (Heckman and Rubinstein, 2001), the following result shows that higher frustration tolerance cannot help people to get rid of a lowest-wage position. Table 17 shows the estimated results on being minimum wage workers. It is clear that the negative correlation between being a minimum wage worker and the frustration tolerance is due to the family background. This indicates that unobservable family factors cause individuals to have lower frustration tolerance and also increase their probability to be in the lowest-wage position. On the contrary, cognitive skills are more related to being minimum wage workers but only in the younger ages. Figure 15 shows the estimated results after controlling family fixed-effects. In the subgroup cognitive skills also have no significant coefficient after controlling family fixed-effects, but some of them are significant in the full sample. After ruling out the influence from family, one standard deviation higher in cognitive skills directly and indirectly leads the probability of being a minimum wage worker to decrease by 1 to 3 percentage points. When subtracting the indirect effects from college and firm characteristics, there are still 2 to 3 percentage points left at ages 26 to 28, while there is no significant correlation at ages 29 and 30. But when controlling the firm fixed-effects, only 0.2 percentage points is left at age 26. It means that increasing cognitive skills rather than

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frustration tolerance reduces the probability of cohorts going to a firm providing them a minimum wage to them when they are younger.

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Table 6: Estimates of Skills Effects on Being A Minimum Wage Worker by Age 26-30

	Frustration tolerance					Cognitive skills				
Age	26	27	28	29	30	26	27	28	29	30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Full sample										
OLS	-0.0124***	-0.0111***	-0.00785***	-0.00718***	-0.00584***	-0.0285***	-0.0261***	-0.0185***	-0.0146***	-0.0162***
	(0.00109)	(0.00100)	(0.000864)	(0.000794)	(0.000768)	(0.00112)	(0.00105)	(0.000879)	(0.000810)	(0.000815)
Family FE	-0.00969	-0.00691	-0.00660	-0.0108**	-0.00323	-0.0303***	-0.0278***	-0.0143**	-0.0124**	-0.0140**
	(0.00815)	(0.00706)	(0.00610)	(0.00545)	(0.00538)	(0.00889)	(0.00787)	(0.00658)	(0.00588)	(0.00605)
Family FE	-0.0117	-0.00843	-0.00839	-0.0123**	-0.00296	-0.0339***	-0.0293***	-0.0165**	-0.0149**	-0.0122*
& college	(0.00873)	(0.00773)	(0.00665)	(0.00596)	(0.00578)	(0.0108)	(0.00973)	(0.00778)	(0.00728)	(0.00740)
Firm FE	-0.000461	-1.51e-05	-0.000121	-1.12e-05	0.000448	-0.00194**	-0.000460	-0.000147	0.000709	0.000145
& college	(0.000724)	(0.000601)	(0.000514)	(0.000452)	(0.000438)	(0.000927)	(0.000754)	(0.000633)	(0.000568)	(0.000527)
Family FE,	-0.00928	-0.0124	-0.0115	-0.0107*	-0.00166	-0.0239**	-0.0315***	-0.0182**	-0.0122	-0.00810
college, & firm	(0.00905)	(0.00789)	(0.00715)	(0.00612)	(0.00601)	(0.0119)	(0.0102)	(0.00831)	(0.00764)	(0.00805)
Panel B. Middle GSAT										
OLS	-0.0123***	-0.00972***	-0.00548***	-0.00691***	-0.00553***	-0.0143***	-0.0124***	-0.00857***	-0.00754***	-0.00974***
	(0.00165)	(0.00152)	(0.00130)	(0.00122)	(0.00116)	(0.00165)	(0.00152)	(0.00126)	(0.00116)	(0.00120)
Family FE	-0.0233	0.00126	-0.000237	-0.0152	-0.0139	-0.0137	-0.00876	0.00770	-0.00432	0.00277
	(0.0198)	(0.0153)	(0.0118)	(0.0128)	(0.0123)	(0.0155)	(0.0140)	(0.0122)	(0.0117)	(0.0110)
Family FE	-0.0198	0.00530	0.000230	-0.00667	-0.0116	-0.00530	-0.000766	0.0146	0.00510	0.00895
& college	(0.0211)	(0.0175)	(0.0135)	(0.0138)	(0.0142)	(0.0169)	(0.0146)	(0.0132)	(0.0125)	(0.0109)
Firm FE	-0.000231	0.000148	0.000355	0.000255	0.000633	0.000558	-0.000466	0.000779	0.00112	0.000554
& college	(0.00120)	(0.000953)	(0.000798)	(0.000702)	(0.000627)	(0.00119)	(0.000960)	(0.000841)	(0.000696)	(0.000660)
Family FE,	-0.0251	0.0100	0.00155	-0.00294	-0.00446	0.0176	-0.00565	0.00520	0.00652	0.000146
college, & firm	(0.0234)	(0.0191)	(0.0163)	(0.0164)	(0.0143)	(0.0217)	(0.0161)	(0.0153)	(0.0148)	(0.0109)

Notes: The table reports the estimates of the skills effects on being a minimum wage worker, which is a dummy variable of 0 or 1. The coefficient shows the probability change in percentage points when skills are increased by 1 standard deviation. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department. Firm information includes the scale and the industry.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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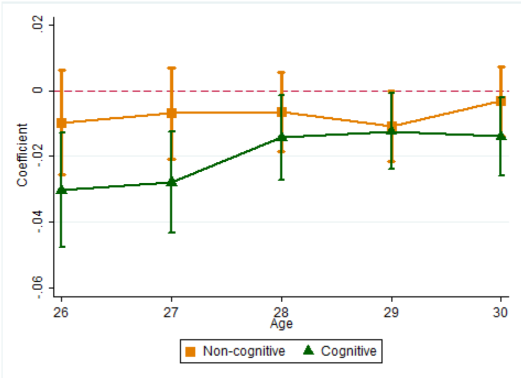
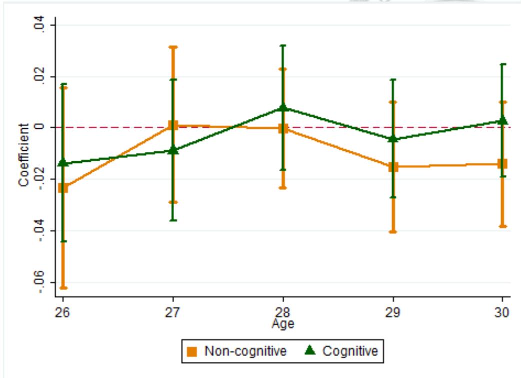
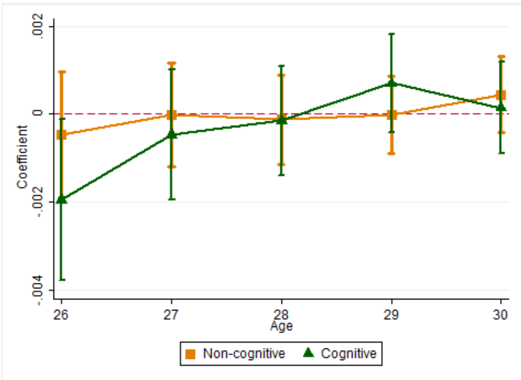


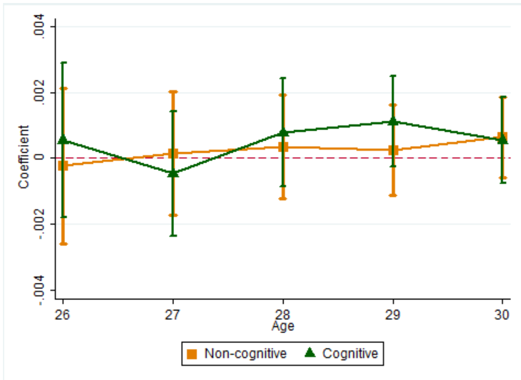
(a) Control Family FE (Full Sample)



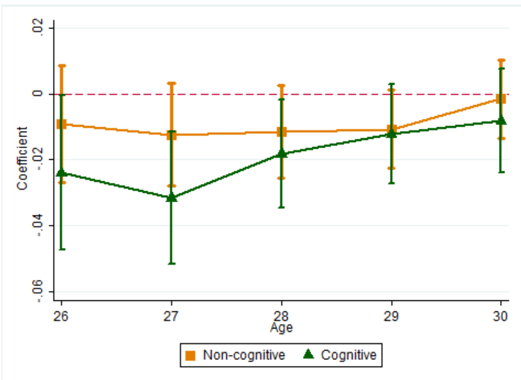
(b) Control Family FE (Middle GSAT)



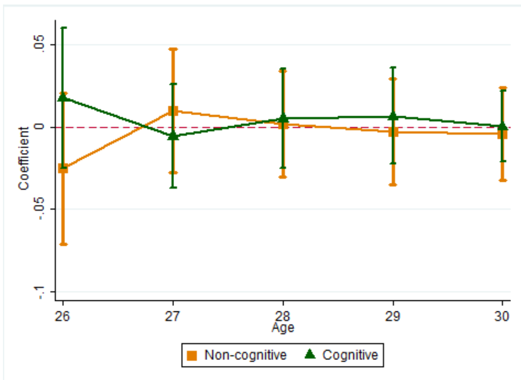
(c) Control Firm FE & College (Full Sample)



(d) Control Firm FE & College (Middle GSAT)



(e) Family FE, College, & Firm (Full Sample)



(f) Family FE, College, & Firm (Middle GSAT)

Figure 10: Estimates of Skills Effects on Being Minimum Wage Worker

Notes: The figure shows the coefficients and 95% confidence intervals of the estimation under controlling family fixed-effects, college, and firm information. All the models control the test year fixed-effects and personal information including sex, birth weight, birth order, and high school status. College information includes whether an examinee attended a public college, and the category of the department. Firm information includes scale and industry. Examinees in the full sample and the subsample of middle GSAT applied to schools through RS or the direct application but failed and then took the AST. The subgroup of middle GSAT contains examinees whose PR in the GSAT tests is between 0.2 and 0.8. The red dotted line is set on 0.

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1.5.5 Nonlinear Estimation

Considering that the skills may have nonlinear effects on the labor market outcome as found in Krassel and Sørensen (2015), the following provides the estimated results of the influence of high and low skills which are defined as be at least one standard deviation lower or higher than the mean value. To simplify the analysis, in this part, I just focus on the outcome at age 30. Different from the previous analysis, performing the nonlinear analysis requires that the full sample be used. Hence, the low and high frustration tolerance may actually be mixed with low and high cognitive skills respectively. Therefore, the results should be explained more carefully.

Table 18 shows the estimation on employment. After controlling the family fixed-effects, only the interaction term of low frustration tolerance and high cognitive skills has a significant correlation with the employment. As shown in Figure 6, higher cognitive skills are usually present along with higher frustration tolerance, and from Table 14 only frustration tolerance matters. The null effect of frustration tolerance here may be mixed with the null effect of cognitive skills. And the special combination of low frustration tolerance and high cognitive skills may actually reflect cohorts who have high but not the highest cognitive skills that are present along with higher frustration tolerance. In other words, influence of cognitive skills on employment may be nonlinear; cohorts who have high but not the highest cognitive skills may have a higher employment probability.

Table 19 shows the nonlinear estimation on being a civil servant. It is clear that high skills increase the probability, although the high frustration tolerance may be mixed with high cognitive skills. The special combination of high frustration tolerance and low cognitive skills may actually reflect cohorts who have low but not the lowest cognitive skills. Those cohorts are more likely to have lower probability to be in the public sector than the middle.

The results in Table 20 are in line with the finding before. After controlling family fixed-effects, only cognitive skills correlated to the initial wages. The high cognitive skills lead wages to increase by 12.5% when ruling out the influence from the college and some observable firm characteristics. But it is reduced to 6% when comparing the variation within firms. Columns (4) and (5) combine to indicate that only cognitive skills may have direct influence in the same firm with an increase less than 6%. Similar to the initial wage,

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Table 7: Nonlinear Estimates of Skills Effects on Employment at Age 30

	(1)	(2)	(3)
High non-cog.	0.0114***	0.0226	0.0213
	(0.00394)	(0.0176)	(0.0179)
Low non-cog.	-0.0297***	-0.0145	-0.0122
	(0.00392)	(0.0179)	(0.0181)
High cog.	-0.000773	0.0112	0.0110
	(0.00365)	(0.0168)	(0.0180)
Low cog.	-0.0492***	-0.0146	-0.00596
	(0.00379)	(0.0189)	(0.0196)
High non-cog * high cog.	-0.0187**	-0.0600	-0.0572
	(0.00874)	(0.0376)	(0.0372)
High non-cog * low cog.	0.0226**	-0.0318	-0.0366
	(0.00890)	(0.0414)	(0.0417)
Low non-cog * high cog.	0.0532***	0.0861**	0.0837**
	(0.00947)	(0.0379)	(0.0382)
Low non-cog * low cog.	0.00375	-0.00190	0.00258
	(0.00946)	(0.0446)	(0.0449)
Control:			
Personal and family variables	V	V	V
Family FE		V	V
College			V

Notes: The table reports the estimates of the skills effects on employment, which is a dummy variable of 0 or 1. The variable of high skills equals 1 if the proxy is larger than 1 standard deviation, and equals 0 otherwise. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 21 shows that only high cognitive skills may have a direct effect on wages at age 30 in the same firm with an increase less than 4%.

Finally, regarding the probability of being minimum wage workers at age 30, Table 11 shows that once we control the family fixed-effects, none of the skills can explain whether a person is a minimum wage worker or not. The result is in line with the finding before.

1.6 Conclusion and Discussion

Contributing to the literature about the influence of non-cognitive skills on the labor market outcome, this study uses the college entrance tests to provide an objective behavior measure to frustration tolerance and rules out the potential influence from family backgrounds and

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Table 8: Nonlinear Estimates of Skills Effects on Being A Civil Servant at Age 30

	(1)	(2)	(3)
High non-cog.	0.0473***	0.0681***	0.0355***
	(0.00314)	(0.0139)	(0.0136)
Low non-cog.	-0.0233***	-0.0222*	0.00108
	(0.00239)	(0.0120)	(0.0117)
High cog.	0.0950***	0.0805***	0.0284**
	(0.00311)	(0.0138)	(0.0142)
Low cog.	-0.0474***	-0.0174	0.0111
	(0.00196)	(0.0117)	(0.0118)
High non-cog * high cog.	-0.0430***	-0.0425	-0.0237
	(0.00771)	(0.0335)	(0.0319)
High non-cog * low cog.	-0.0420***	-0.0705**	-0.0590**
	(0.00519)	(0.0277)	(0.0270)
Low non-cog * high cog.	-0.0364***	-0.0458	-0.0305
	(0.00761)	(0.0321)	(0.0311)
Low non-cog * low cog.	0.0200***	0.0193	-0.00412
	(0.00432)	(0.0255)	(0.0252)
Control:			
Personal and family variables	V	V	V
Family FE		V	V
College			V

Notes: The table reports the estimates of the skills effects on being a civil servant which is a dummy variable of 0 or 1. The variable of high skills equals 1 if the proxy is larger than 1 standard deviation, and equals 0 otherwise. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

the mechanisms that skills may go through. The results show that by age frustration tolerance has an increasing direct influence on employment probability and the probability of being a civil servant. Being one standard deviation higher in frustration tolerance directly leads those two probabilities at age 30 to increases by 3 and 2 percentage points respectively, and indirectly increase the probability of being a civil servant over ages 26 to 30 by 1 to 2 percentage points through the college. However, frustration tolerance can only have indirect influence on wages through the college, through which one standard deviation higher in frustration tolerance leads wages to increase by about 3% to 4% and there is more significance when cohorts are older. No direct or indirect influence from frustration tolerance on the probability of being a minimum wage worker is found. In

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Table 9: Nonlinear Estimates of Skills Effects on Initial Wage

	(1)	(2)	(3)	(4)	(5)
High non-cog.	0.0492***	0.0280*	0.00745	0.0117**	0.0320
	(0.00338)	(0.0167)	(0.0165)	(0.00568)	(0.0305)
Low non-cog.	-0.0387***	-0.0112	0.000168	-0.0193***	0.0247
	(0.00301)	(0.0157)	(0.0154)	(0.00550)	(0.0280)
High cog.	0.162***	0.129***	0.0854***	0.0601***	0.126***
	(0.00334)	(0.0166)	(0.0170)	(0.00575)	(0.0293)
Low cog.	-0.101***	-0.0761***	-0.0474***	-0.0406***	-0.0433
	(0.00264)	(0.0160)	(0.0168)	(0.00558)	(0.0287)
High non-cog * high cog.	-0.0192**	0.00915	0.0132	-0.0169	-0.0214
	(0.00808)	(0.0401)	(0.0388)	(0.0122)	(0.0641)
High non-cog * low cog.	-0.00865	-0.0138	-0.0145	-0.00494	-0.0652
	(0.00674)	(0.0364)	(0.0355)	(0.0122)	(0.0637)
Low non-cog * high cog.	-0.0408***	-0.0227	-0.00686	0.00794	-0.0346
	(0.00845)	(0.0394)	(0.0381)	(0.0130)	(0.0723)
Low non-cog * low cog.	0.0137**	-0.0323	-0.0310	0.0182	-0.0579
	(0.00606)	(0.0350)	(0.0349)	(0.0124)	(0.0603)
Control:					
Personal and family variables	V	V	V	V	V
Family FE		V	V		V
College			V	V	V
Firm FE				V	
Firm information					V

Notes: The table reports the estimates of the skills effects on initial wage which is deflated by the CPI. The variable of high skills equals 1 if the proxy is larger than 1 standard deviation, and equals 0 otherwise. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department. Firm information includes the scale and the industry.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

other words, someone increasing their frustration tolerance can help them to be more likely to be employed, especially when they are older, but the skills cannot help them to have higher wages or get rid of a minimum wage position.

On the other hand, cognitive skills are a stronger predictor of wages. When ruling out the influence from family backgrounds, one standard deviation higher in cognitive skills can lead the wages at age 26 to 30 to increase by 8% to 10%, while 1% to 2% is contributed from college, 4% to 5% is through the job matching, and around 3% is directly from the higher skills. As to being a minimum wage worker, one standard deviation higher in cognitive

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Table 10: Nonlinear Estimates of Skills Effects on Wage at Age 30

	(1)	(2)	(3)	(4)	(5)
High non-cog.	0.0575***	0.0657**	0.0407	0.0139***	0.0493*
	(0.00431)	(0.0285)	(0.0282)	(0.00399)	(0.0283)
Low non-cog.	-0.0485***	0.0121	0.0255	-0.0129***	0.0348
	(0.00394)	(0.0258)	(0.0257)	(0.00397)	(0.0261)
High cog.	0.179***	0.158***	0.113***	0.0446***	0.0834***
	(0.00422)	(0.0256)	(0.0268)	(0.00395)	(0.0273)
Low cog.	-0.114***	-0.0629**	-0.0337	-0.0307***	-0.0249
	(0.00365)	(0.0265)	(0.0273)	(0.00427)	(0.0277)
High non-cog * high cog.	-0.00566	0.0292	0.0246	-0.0112	0.0119
	(0.0104)	(0.0574)	(0.0566)	(0.00841)	(0.0546)
High non-cog * low cog.	0.00507	-0.122**	-0.108*	-0.00435	-0.0957
	(0.00896)	(0.0591)	(0.0585)	(0.00893)	(0.0584)
Low non-cog * high cog.	-0.0494***	-0.0972	-0.0879	-0.0132	-0.0989*
	(0.0106)	(0.0644)	(0.0630)	(0.00939)	(0.0581)
Low non-cog * low cog.	0.0175**	-0.0555	-0.0505	0.00887	-0.0471
	(0.00880)	(0.0610)	(0.0601)	(0.0102)	(0.0608)
Control:					
Personal and family variables	V	V	V	V	V
Family FE		V	V		V
College			V	V	V
Firm FE				V	
Firm information					V

Notes: The table reports the estimates of the skills effects on wage at age 30, which is deflated by the CPI. The variable of high skills equals 1 if the proxy is larger than 1 standard deviation, and equals 0 otherwise. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department. Firm information includes the scale and the industry.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

skills can lead the probability at age 26 to 30 to decrease by 1 to 3 percentage points, but all of it goes though the job matching rather than directly by the skills. But when it comes to employment, cognitive skills have no direct or indirect effect after controlling the family fixed-effects. And they have similar effects to frustration tolerance on being a civil servant. One standard deviation higher in cognitive skills directly leads the probability of being a civil servant over ages 26 to 30 to increase about 2 percentage points, and indirectly raises it 1 to 2 percentage points through the college.

The results that frustration tolerance is more relevant to employment and cognitive

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Table 11: Nonlinear Estimates of Skills Effects on Being A Minimum Wage Worker at Age 30

	(1)	(2)	(3)	(4)	(5)
High non-cog.	-0.00608**	0.0122	0.0141	0.00216*	0.0141
	(0.00252)	(0.0183)	(0.0189)	(0.00124)	(0.0195)
Low non-cog.	0.00968***	0.00423	0.00321	-0.000577	-8.10e-06
	(0.00264)	(0.0182)	(0.0182)	(0.00143)	(0.0196)
High cog.	-0.0212***	-0.0207	-0.0149	0.00240*	-0.00442
	(0.00212)	(0.0151)	(0.0167)	(0.00124)	(0.0183)
Low cog.	0.0266***	0.00257	0.000620	0.000285	-0.00400
	(0.00274)	(0.0193)	(0.0199)	(0.00150)	(0.0212)
High non-cog * high cog.	0.000427	-0.0164	-0.0129	-0.00312	-0.0166
	(0.00460)	(0.0287)	(0.0290)	(0.00217)	(0.0289)
High non-cog * low cog.	-0.0112*	0.0253	0.0214	-0.000176	0.0415
	(0.00597)	(0.0430)	(0.0431)	(0.00341)	(0.0413)
Low non-cog * high cog.	-0.00109	0.0165	0.0149	-0.00124	0.0304
	(0.00566)	(0.0332)	(0.0338)	(0.00272)	(0.0362)
Low non-cog * low cog.	0.00396	0.0934*	0.0944*	0.00376	0.104*
	(0.00729)	(0.0551)	(0.0556)	(0.00419)	(0.0583)
Control:					
Personal and family variables	V	V	V	V	V
Family FE		V	V		V
College			V	V	V
Firm FE				V	
Firm information					V

Notes: The table reports the estimates of the skills effects on being a minimum wage worker at age 30, which is a dummy variable of 0 or 1. The variable of high skills equals 1 if the proxy is larger than 1 standard deviation, and equals 0 otherwise. All the models control the test year fixed-effects, and the personal information including sex, birth weight, birth order, high school status, and the family background including parents' schooling years and whether they are in the public sector or not. College information includes whether an examinee attended a public college after the tests, and the category of the department. Firm information includes the scale and the industry.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

skills are stronger to explain the wage difference are in line with the previous finding in Lindqvist and Vestman (2011). This study has further considered the mechanisms that skills go through. The smaller part of the direct influence from both skills on wages indicates that these two skills may not efficiently raise people's productivity if we assume that the wage sufficiently reflects the marginal product of labor. If it is true, then it means that people who have higher frustration tolerance only make more effort to find jobs or attain better educations but do not make more effort on their work, and people who have

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higher cognitive skills have abilities to go to better schools and find highly-paid jobs but contribute very little on their work.

However, this explanation is only based on the labor supply side and would be challenged, while previous studies have already found some evidence on the labor demand side that employers prefer employees who have higher non-cognitive skills (Mourshed et al., 2012; Protsch and Solga, 2015; Cunningham and Villaseñor, 2016). As long as the preference reflects a rational choice from the profit-maximizing employer, it means that those kinds of labor can help employers to either lower the costs or increase the output. The result shows that cohorts who have higher frustration tolerance do not have lower wages, thus they may have a higher productivity or at least be helpful to the production. If it is the case, their wages should be higher but they are suppressed due to some reasons. The same reasons may also cause people who have higher cognitive skills have lower wages than they could have. As non-cognitive skills acquire more attention in the labor market, the question that future research could solve is why people having higher skills do not have correspondingly higher wages.

Finally, the result on being a minimum wage worker shows that people have fall minimum wage positions not because they have lower frustration tolerance or cognitive skills; it is more related to their family background. Hence, to improve those workers' situations, policy makers should think either to improve their family situations or to raise the minimum wage directly rather than to raise their skills.

2 Employment, Wage, and Inequality Effects from A Minimum Wage Increase: Evidence from Monthly Personnel Administrative Data of Taiwan

2.1 Introduction

In spite of abundant minimum wage studies, it remains an open question empirically as to how a minimum wage increase affects wage inequality. Recently, Autor et al. (2016) use three decades of data after 1980 to reassess the study of the minimum wage effect on the US earning inequality that was done by Lee (1999), who found that the falling relative level of the US federal minimum wage can account for the growth in inequality not only

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in the lower tail but also the higher tail of the wage distribution. Although they use an IV strategy eliminating most of the ripple effects found by Lee, they still observe spillover effects which they argue are a result of a measurement error, but needs to be proved with ideally administrative payroll data.

In fact, other than spillover effects, influence of the minimum wage on wage inequality also depends on the effects—both the wage and employment effects—on workers who are directly affected by the minimum wage. Studies using the wage distribution to identify the minimum wage effect on the wage inequality, such as these two influential studies, need to be based on the assumption of no employment effect on workers bound by the minimum wage in their sample.[15] However, the assumption may not hold, especially when minimum wages increase and affect more than teenagers and other minority workers.

In a more recent working paper, Cengiz et al. (2018) used a bunching-based method to estimate the wage frequency distribution and proved that there were spillover effects of minimum wage increases in the administrative data with no disemployment effect at the same time, which is inferred by tracking the changes in the number of jobs throughout the wage distribution. The drawback of this method is that under the accumulated number it failed to identify who gained or lost jobs, which is rather a critical issue in the minimum wage research. In other words, to answer the open question about the wage inequality effect resulting from minimum wage increases, other than keeping focusing on the wage distribution, it would be helpful to go back to the individual-based analysis—how are the lowest-wage workers affected by the minimum wage increases, and also what are the effects on workers who earn slightly above the new minimum?

However, previous literature fails to answer those questions due to combined issues in three key aspects. First, there is a paucity of research on the directly affected workers, which is the lowest-wage group (Belman et al., 2015; Manning, 2016; Jardim et al., 2017; Boffy-Ramirez, 2019). Second, similar to the first but not identical, previous studies mostly include directly affected workers and indirectly affected workers with slightly higher wages than the minimum. As a result, both the direct and indirect effects, which may result from the labor-labor substitution, the scale effect, or the compensation hierarchies, cannot be identified (Neumark, 2018). Third, the empirical methodology needs to

[15] Lee (1999) only focused on the observed wage distribution without accounting for employment effects caused by the minimum wage. Autor et al. (2016) argued that limiting their sample to 25-64 years old can prevent their findings from being affected by disemployment effects.

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rely on a valid counterfactual, which could be contaminated by unobservable confounders, trends, or impacts caused by minimum wage changes (Allegretto et al., 2017; Neumark and Wascher, 2017; Totty, 2017). These three aspects have been discussed separately in prior studies more or less, in particular the methodology. But to my knowledge, there has not been a study dealing with all of them yet. Actually, without the separation of workers with different levels of wages, the methodology cannot prevent attenuated estimates of the effect as they blend workers and rule out unobservable impacts of minimum wage in the counterfactual.

This study aims to pin down the minimum wage effects on the earning inequality by providing firm- and individual-level evidence built on an unique method containing several features. First, the changes in the wage and employment of bound and unbound workers are separated and tracked in this study. This study has the advantage of being based on monthly administrative personnel longitudinal data of Taiwan, the monthly record of the mandatory labor and employment insurance run by the government. Second, rather than using the method of difference in difference, I employ the approach of sharp regression discontinuity (RD) design. This high frequency data along with there being only one increase in the national minimum wage during the research period allow me to use RD to avoid the need of creating a valid counterfactual. Third, this study also analyzes the difference between individuals rather than the difference between percentiles in the wage distribution.

To my best knowledge, this is the first study using high quality personnel administrative data to estimate the direct and indirect effects of a minimum wage increase simultaneously, hence providing individual-level evidence for the wage inequality effect and contributing to the literature examining the wage difference in the wage distribution (Dinardo et al., 1996; Lee, 1999; Autor et al., 2016; Cengiz et al., 2018). Meanwhile, the research also contributes to the literature on employment effects by dealing with the issues of research targets (Neumark and Wascher, 2007; Sabia et al., 2012; Belman et al., 2015; Manning, 2016; Jardim et al., 2017; Neumark, 2018; Boffy-Ramirez, 2019), the methodology (Allegretto et al., 2017; Neumark and Wascher, 2017; Totty, 2017; Boffy-Ramirez, 2019), and the employment flow (Brochu and Green, 2013; Dube et al., 2016). In addition, it also contributes to the literature on spillover effects (Gramlich, 1976; Grossman, 1983; Akerlof and Yellen, 1990; Katz and Krueger, 1992; Spriggs, 1993; Card and Krueger, 1994; Neumark

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et al., 2004; Clemens et al., 2018; Cengiz et al., 2018; Dube et al., 2019), in which recent studies find positive wage spillover effect and refer to the equity effect (Cengiz et al., 2018; Dube et al., 2019). Finally, since the inequality has worsened in most of the countries since the 1980s, and the minimum wage is a common tool universally, results from Taiwan also enrich the literature.

To explain the effect on wage inequality, the hypothesis of the "equity effect" for the spillover effect plays a main role in recent studies, such as Giupponi and Machin (2018) and Dube et al. (2019). This effect reflects the firm's response to maintain the wage structure, given workers' efforts depend on relative wages rather than absolute ones. Under this hypothesis, firms simply raise overall wages and product prices to mitigate the increase in the minimum wage (Gramlich, 1976; Grossman, 1983; Akerlof and Yellen, 1990; Katz and Krueger, 1992). Recently, Dube et al. (2019) used data from a large US retailer showing that workers with wages up to 15% higher than the new minimum had raises after the minimum wage increase because of the firm's policy. If it is the case generally, there should be no employment effect but wage increases, then using the wage distribution to analyze earning inequality would not be problematic as described above.

In this study, I began by generating the matched employee-employer data to analyze the changes of employment and average wages in firms to see if this hypothesis is supported by the data. By using the RD approach with the monthly periods as the running variable, I do not have to assume a counterfactual. Instead, the assumption behind the methodology is that the labor market outcomes are continuous around the cutoff time, which is the time of the implementation of the new minimum. If a discontinuity exists, it reflects the exogenous policy impacts. In other words, I use the time cutoff as an instrument variable to replace the minimum wage variable when identifying the causal effects of the minimum wage increase. Compared to assuming the counterfactual has a similar trend but is totally unrelated to the minimum wage change, the assumption in this study is weaker.

The estimates from the firm-level analysis initially show that there is a significant positive effect on the average nominal wage and no significant employment effect on full-time workers of all firms. Those results are in line with the expectation of the hypothesis. However, the results do not hold when it comes to the real wage and the employment of small scale firms, which were more likely to be affected by the minimum wage increase. My best estimates show that the average real wage significantly decreased by 1% compared to

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the prior level, and the employment effect is mixed but apparently not null. In a short bandwidth, about six months before and after the minimum wage increase, the lowest-wage full-time employment including the bound workers and the new minimum was increased by 1%, and the slightly higher-wage employment was increased by 2%; in a longer bandwidth, about 10 months before and after, the lowest-wage employment was decreased by 1% but may result from minimum wage employers moving to higher-wage positions rather than disemployment, since the higher-wage employment was increased more and the total employment was increased by 2%. In addition, the part-time employment was increased by 12% to 25% from short bandwidth to longer bandwidth.

Except for the employment change, since the increase in the minimum wage exceeded the consumer price index, the decline of the average real wage may come from the unbound workers, which is opposite to the hypothesis. I then transfer my focus from employment of wage positions to individual workers to clarify from where the effects came.

To identify the effects on individuals, I firstly show the challenge of classifying workers by using observable wages. Since the workers' wage information only can be observed given they are employed, using wages just prior to the increase can lead to missing the effects of minimum wages on transitions from non-employment to employment (Neumark, 2018). Previous studies usually used workers' previous average wage to do classification (Clemens and Wither, 2014, 2017). However, it may mix the effects from directly affected and indirectly affected workers. In addition, since the outcomes—the wage and the employment—are also used to classify groups of workers, I show that using more restrictive information to select groups may lead to artifacts.

Workers with previous wages bound by the new minimum all the time or just bound before the cutoff time had a dramatic decline in employment after the increase. This is not due to the policy effect, but instead is a result of the target group being restricted to those employed with low wages previously instead. It also leads to an artifact of wage changes. To avoid the influence from the restriction, I use the earliest wage information, about forty months before the increase, to classify workers, and then track their wages and status of employment overtime. The results show that the former bound workers experienced significant increases on their nominal and real wages and even on their employment probability. As a result, their real earnings, including the change of their employment, were increased. On the other hand, the former unbound workers had their employment probability raised

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but with an adverse effect on their real wages. The two opposite effects lead to total real earnings having no significant change.

The drawback of this classification strategy is the mixing of the direct and indirect effects, since the former status may change after forty months. To solve this problem, I then use the wage distributions before the minimum wage increase for those two groups of workers to separate both direct and indirect effects. The additional assumptions are that there is no effect on workers with much higher wages than the unbound workers I define, and all the workers share the same unemployment rate. Even after this adjustment, all the directions of results stay the same but with larger and more distinctive estimates of effects on the two groups of workers. My estimates show that the employment of full-time workers bound by the minimum wage is significantly increased by 1% to 4% due to the minimum wage increase, while their real wage is increased by 1% to 3%. On the other hand, the employment of workers who had wages slightly higher than the new minimum wage is also increased by 3% to 7%, but their real wage is decreased by 6% to 11%. The decrease reflects that they lose their potential nominal wage increase and suffer because of the hike of the CPI rather than having their wages cut. As a combining result of the employment and wage effect, the bound workers have their real earnings increased by 4% to 5% but the unbound workers have no significant change on their real earnings.

The positive employment effect on the low-wage workers is not so striking, since it has also been found by some recent studies (Dolton et al., 2012; Giuliano, 2013). The most surprising part is that there is no wage spillover effect but a negative influence on the unbound workers' real wages. The result is opposite to the results shown by Dube et al. (2019), which used data from a large individual firm, and Cengiz et al. (2018), in which they focused on the wage distribution. By tracking individuals to identify effects, this study at least points out an alternative expectation compared to those results. Meanwhile, the results show that there is no spillover effect on the earning inequality, also different from the studies focusing on the wage distribution (Lee, 1999; Autor et al., 2016).

In addition to estimating separated effects on bound and unbound workers, I further analyze the difference between the former bound and unbound workers across time. Compared to separated estimates, this method can rule out factors that may influence both kinds of workers. I show that differences in nominal wages, real wages, and earnings between them had discontinuous declines around the cutoff time, which means the wage and

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the earning inequalities were narrowed. The effects persisted during the remainder of our data period, forty months after the minimum wage increase. Since the wage increase of bound workers was determined by their previous nominal wages and the new minimum, I then test whether the unbound workers had any nominal wage change after the increase by using the wage distribution prior to the wage increase to generate the expected wage increase amount. The result also shows that rather than increasing, the unbound workers' nominal wage was significantly lower than before, which still proves that there is no spillover effect on the wage inequality.

Since the results I show are not in line with the hypothesis of the equity effect (Gramlich, 1976; Grossman, 1983; Akerlof and Yellen, 1990; Katz and Krueger, 1992; Dube et al., 2019) or the demand shift resulting from the labor-labor substitution effect expected by standard economic theory, the next question is how did it happen. The most influential alternative theories behind the minimum wage studies include monopsony power (Burdett and Mortensen, 1998; Bhaskar and To, 1999; Manning, 2003; Flinn, 2006; Ahn et al., 2011) and search models (Ahn et al., 2011; Brochu and Green, 2013; Giuliano, 2013; Gittings and Schmutte, 2016). In more recent studies, Brochu and Green (2013) and Dube et al. (2016) found that separations and accessions rates among potentially affected workers were reduced because of minimum wage increases, which is consistent to search models' expectations. Following the studies, I use the matched employee-employer data to test the employment flow to check if results are in line with the search models.

Different from Dube et al. (2016), in which they used border discontinuity design, I use RD to observe the changes of the separation and new hire rates of the full-time and part-time workers among small scale firms. Rather than finding a robust decline on those rates, the results are mixed and show robustly contrary effects on part-time workers. The full-time separation rate was decreased by 3.3% in a longer bandwidth, but there was no significant change in a shorter one; the new hire rate had no significant change in a longer bandwidth but was increased by 39% in a shorter bandwidth. Turning to part-time employment, results show that there were higher separation and new hire rates after the minimum wage increase no matter the length of the bandwidth. Compared to the prior level, the separation rate on part-time workers was increased by a percent in the low 30s, and the new hire rate was increased by a percent in the high 30s to the low 40s. Apparently, the search friction previous studies expected may not explain this result.

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Through the advantage of identifying individual workers, I further generate the transition matrix of the former bound workers during the research period. I classify five employment statuses, including full-time employed with the lowest wage, full-time employed with a low wage, full-time employed with a high wage, part-time employed, and non-employment. I track the individuals' transitions among the five statuses between every month, hence there would be twenty-five situations at every time. This approach helps to identify the transition changes more directly.

I find that the full-time employed workers have a steady probability of staying in the original statuses after the minimum wage increase. It means that their separation does not decline, which is in line with the finding in the short run firm-level analysis. On the other hand, full-time workers have a higher probability of transferring to part-time jobs although the scale of transferring to part-time jobs is too small to affect the main transition. However, one month after the full-time workers transit to part-time jobs, the probabilities of part-time workers transferring to full-time employment and non-employment are increased. These higher inflow and outflow on the part-time employment are also consistent to the firm-level finding. In other words, even the individual transition still rejects the possibility of the search friction hypothesis.

Although the transition matrix cannot directly prove the non-employed but potentially low-wage workers' (including workers bound by the minimum and slightly above the new minimum) transition, their higher employment along with the steady separation rates indicate the increased employment should be from non-employment. The entry of the bound full-time workers and the transition from full-time to part-time are more close to the theory of the traditional monopsony theory, in which the labor market is "supply-constrained" (Card and Krueger, 1994).

However, the increased entry to full-time employment with wages slightly higher than the new minimum wage may be from the contribution of labor demand or labor supply. Since their growth in employment coincides with a decrease in their real wages, there seems to be an outward shift of the labor supply. Previously, Neumark et al. (2004) found similar results but attributed it to slightly higher-wage workers needing to work more to cover their lower-wage family members' disemployment due to the minimum wage increase. That explanation is obviously not valid here since there is no apparent disemployment effect on the bound workers. That is, even though the phenomenon is really from a shift of the

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labor supply, the reason is unknown. An alternative explanation is employers suppress the wage growth of the slightly higher-wage workers to mitigate the hike in labor cost, but they also have higher demand for these workers because they are relatively cheaper. And due to high labor supply elasticity of those workers, their wages have not increased because of the higher labor demand.

Finally, considering another channel of a "spillover" effect, I also estimate the minimum wage effect on workplace safety. By matching employee-employer data with injury benefit records of the labor insurance, I find that the occupational injury rate in firms was increased about 23% to 30% compared to the prior level. It might be inferred that the employers cut safety-related costs or forced an increase in labor productivity increased to lower the cost pressure.

The study proceeds as follows. In Section 2 I review the literature related to the minimum wage effect. The policy background and the data used in this study are described in Section 3. The empirical methodology and data setting are described in Section 4, and the empirical findings are shown in Section 5. The final section concludes and discusses the limitation.

2.2 Literature Review

This study is related to three strands of literature. Studies analyzing the minimum wage effect on the wage inequality, literature on estimating the employment effect of minimum wage, and researches related to spillover effects of minimum wage.

2.2.1 Wage Inequality Effects of Minimum Wage

The massive increase in wage inequality in the US has raised attention to the role of the minimum wage. Dinardo et al. (1996) first used the density of wage distribution to identify the effect of minimum wages on changes in the US distribution of wages between men and women. They simulated a counterfactual wage distribution for 1988 by adjusting the minimum wage back to the 1979 level in real terms. They estimated the effects in the lower tail of the distribution by assuming neither the disemployment effect, the spillover effect, nor the change on the shape of density existed. Their results showed that the decline in the real value of the minimum wage accounted for about one-third of the increase in residual wage inequality, which was larger for women. Lee (1999) relaxed the assumptions

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of Dinardo et al. (1996) by simply using the variation in the relative level of the federal minimum wage, which was measured as the log-differential between the logs of the minimum wage and some measure of the median, between states and years to identify the effects on the log wage differentials in the lower tail and also the upper tail during 1979 to 1988. The results Lee found are striking. The decline in the real value of minimum wages can explain more than the whole rise of the 10/50 wage differential. It means that if the real value of the minimum wage stays constant during the period, the wage inequality would have compressed rather than increased. In addition, other than the effect on the 10/50 wage differential, there was also a large effect on the 25/50 wage differential and even effects in the upper tail. Because the estimates are based on the wage distribution, Lee listed the effects may be due to three possible cases which have still not been proven: censoring with no spillover effect or disemployment, spillovers with no disemployment, and truncation by disemployment but with no spillover effect. Acknowledging the huge difference between these two studies, Teulings (2003) found evidence in line with Lee (1999) and rejected the assumptions of Dinardo et al. (1996) that there were no spillovers nor change in the shape of the wage distribution.

However, given the median wage is the standard benchmark for identifying wage differentials in Dinardo et al. (1996) and Lee (1999), Autor et al. (2016) raised the question of correlation existing between the residuals of the median wage and the wage differentials. Considering that shocks on the median may have less impact on wages farther from it, the non-zero correlation between the median and wage differentials lead to biased estimates. Alternatively, they employed an IV approach with three IVs including the log of the real minimum wage, the square of the log of the real minimum wage, and the interaction between the log minimum wage and the average log median real wage for the state over the sample period. By using the IV approach and a longer period from 1979 to 2012, they found that the OLS estimates shrank noticeably and there were no effects left in the upper tail. Compared to the extent of 85% to 110% that could be seen as the contribution of the falling minimum wage in the lower tail distribution by the OLS estimates, 2SLS estimates found only less than 40%. Even with that, they still found effects extending to the wage distribution that the minimum wage cannot mechanically affect, hence, still leaving open the question of whether spillovers existed. They pointed out it could be from measurement error, but they lacked proof from ideal administrative payroll data.

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Although the IV strategy solves the problem of the potential correlation, the work of Autor et al. (2016) is still built on the density of the wage distribution. Results could still reflect the truncation case as Lee (1999) noted, in which disemployment caused a reduction in the number of low wages in the distribution, and hence present a more positive effect on reducing the wage inequality. Facing this potential question, Autor et al. (2016) limited their sample to those who were 25−64 years old, but they could not resolve the concerns of whether higher-age but low-wage workers were actually affected by the minimum wage increase. By using administrative payroll data, Cengiz et al. (2018) used a novel method by counting the change in the frequency of jobs in the lower tail of the wage distribution. More specifically, they generated a counterfactual wage distribution and then compared the missing jobs that were below the new minimum in the counterfactual to the excess jobs that had wages at or slightly above the new minimum in the post-treatment period. The employment effect was measured by the summation of the missing and excess jobs; the change in the total wage between the bunching range was calculated by multiplying the change in jobs and the related wage levels, and then was used to derive the average wage effect divided by jobs below the mew minimum. This wage effect was further used to quantify the wage spillover effect, which was measured by comparing the change in the average wage and the mechanical wage change which was calculated without no spillovers. They also separated sample based on incumbents and new entrants, classified by employment status at one year before the increase. Their results show that there was no disemployment effect and only modest wage spillovers, which is in line with the finding from Autor et al. (2016), and the spillovers mainly came from the incumbents.

Even though Cengiz et al. (2018) gave new evidence combined with employment and wage effects by using the bunching-based method, there are some unclear aspects in this method. First, the employment effect they estimated may mix up effects from workers with higher wages than the new minimum before the policy change. Instead of the bound workers staying in employment, alternative explanations to the excess jobs may be increased employment for the higher-wage workers or workers in other wage levels moving to those positions. In other words, how to decide the bunching range for the bound workers may lead to different results. If the range is smaller, they may find disemployment on the bound workers and the positive employment effect on the unbound workers. Even though they separated out the incumbents and still found no employment effect, employment in-

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formation one year before the policy change may be inappropriate to use if employers are more likely to reemploy workers just left. Meanwhile, the total sample seems to have more positive employment change than the subsamples of incumbents and new entrants, which also raises the concern that there may be some part having higher employment change neglected in the analysis of the subsample. Second, similarly, the wage and spillover effects they measured also have to stand on the fact that there as no employment effect on the high-wage workers. If it is the case, then the wage change and the spillovers reflect the change in the distribution rather than the the effects on the bound and unbound workers. Third, there also seems to be a concept confusion between the wage effect and the spillover effect, since they used the whole wage change between the bunching range to measure the wage effect on the jobs below the new minimum, and also used it to measure the spillovers on the higher-wage jobs.

Hence, studies going back to the individual-based analysis to clarify the employment and wage effects on workers in different statuses are needed in this strand of literature.

2.2.2 Employment Effects of Minimum Wage

The second related strand of literature is on the employment effect. By the standard economic theory, workers with lower wages than the minimum would be forced to unemployment due to their lower productivity. As a result, people who are supposed to be helped suffer instead because of the policy. However, the assumption behind the story, the perfect competition in the labor market, may not hold. Alternative theories at least include the monopsony labor market (Burdett and Mortensen, 1998; Bhaskar and To, 1999; Manning, 2003, 2011; Ashenfelter et al., 2010), search frictions (Burdett and Mortensen, 1998; Flinn, 2006; Ahn et al., 2011; Brochu and Green, 2013), efficiency wages (Rebitzer and Taylor, 1995), and informational asymmetries (Drazen, 1986). Due to the ambiguity of theories, the empirical result is the key to understand the real employment effects. Unfortunately, even today estimates are still elusive (Schmitt, 2013; Manning, 2016; Neumark, 2018) and negative publication biases may exist (Doucouliagos and Stanley, 2009), even on teenagers and restaurant workers, which are the groups thought to be more likely to be affected.[16]

[16] Teenagers are the most concerned group in the literature on minimum wage effect. For example, a meta-analysis study, Doucouliagos and Stanley (2009), noted that there were more than half of the minimum wage studies focusing on teenagers in their collection of 64 US studies. More recently, in the review of Belman et al. (2015), more than forty studies (one third of studies they reviewed) provided estimates of effects on teenagers. Restaurant workers are the second most popular group studied. Studies include Card and Krueger (1994), Dube et al. (2010), Giuliano (2013), Addison et al. (2013), Neumark

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2.2.2.1 The Methodology

The elusiveness results from the evolution of the methodology. In the development of the US literature, the conference launching the New Minimum Wage Research in 1991 was a critical turning point. Prior to the conference, as Brown et al. (1982) reviewed, early studies relied on one-dimensional variation, more from time-series data than cross-sectional data. The main limitation of time-series data is that disaggregation by region and detailed industry is precluded. Following the conference, Neumark and Wascher (1992) adapted the framework of the time-series research into panel data, which generated more variation to consider potentially influential factors even between the same time period. To rule out confounding factors, they form the standard two-way fixed effect model to eliminate unobservable influence from time and states. Results they found are similar to previous time-series studies, the employment elasticity of the minimum wage was -0.1 to -0.2 among teenagers and -0.15 to -0.2 among young adults. On the other hand, Card and Krueger (1994) introduced the approach of the quasi-experiments and the technique of difference−in−difference to evaluate the employment effect on the fast-food industry of a minimum wage increase in New Jersey. They used survey data comparing the employment difference between New Jersey and eastern Pennsylvania, in which there was no minimum wage change, before and after the change in New Jersey. Rather than finding a significant negative elasticity, their results even showed a positive effect.

The striking finding of Card and Krueger (1994) gave challenges to the conventional thought of economists. However, there might be concerns about generalizing results from this approach of quasi-experiments, which was based on a short-term case study (Belman et al., 2015). Compared to case studies, analysis on panel data seems more likely to capture the overall minimum wage effect. Nevertheless, estimates from panel data analysis may just reflect correlation rather than causal effect. This doubt is also raised by the following studies. Dube et al. (2010) and Allegretto et al. (2011) gave two challenges to the standard two-way fixed effect model. First, the minimum wage implementation may be endogenous or have correlation with labor demand and supply. For example, technological changes cause the job polarization that pushes middle-skilled adults into low-skilled jobs traditionally held by teenagers, thus lowering teenage employment (Smith, 2011), and high minimum wage states usually have higher job polarization (Allegretto

et al. (2014a), Dube et al. (2016), and Totty (2017).

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et al., 2017), hence states with higher minimum wages have correlation with higher teenage unemployment. Second, there may be specific time trends in states where the minimum wage is raised. For example, implementation of minimum wage increases usually has a time lag from enactment, which usually happens when there is an expanding economy and low unemployment. At the time of implementation, though, the economy may be contracting with increasing unemployment. To solve these problems, Dube et al. (2010) and Allegretto et al. (2011) argued that economic shocks could be the same in close regions, so it is more precise to estimate by using the border discontinuity design in which the control group is selected from a contiguous area. In other words, they mix the panel data analysis with the approach of the quasi-experiment. Besides, they consider an estimate is biased if there are pre-trends in both treatment and control areas, so they argue that the state-specific trend should be controlled. After making these two settings, they found that there was no significant decrease in restaurant employment nor teen employment. Totty (2017) used a factor model and also found results in line with Dube et al. (2010) and Allegretto et al. (2011) that were significantly lower than ones from a standard two-way fixed effect model.

The setting of panel data with quasi-experiments combines advantages of two approaches. However, the debate continues as to whether a contiguous area is a good control. The other camp of this debate (Neumark et al., 2014a,b; Neumark and Wascher, 2017; Neumark, 2018) argued that taking nearby areas as controls is even worse than using others due to two reasons. One is that there could exist some unobservable differences that lead to the nearby areas having different minimum wages, and hence the estimates would be more biased. The second is that if the migration cost is low, then low-skilled workers may move to nearby areas, so the comparison would be problematic. Rather than relying on a prior assumption about which controls are valid, they argued that data-driven methods are more plausible to find adequate controls.[17] In addition, they questioned whether controlling state-specific trends absorbs the treatment effects if the effect is dynamic.[18] Sabia et al. (2012) first used the synthetic control and found negative effects, even the results were the same when they used geographically proximate comparison and the within-state comparison. Neumark et al. (2014b) also found negative effects by using the synthetic control and pointed out the cross-border design of Dube et al. (2010) and Allegretto et al.

[17] The synthetic controls used by minimum wage studies usually followed the technique from Abadie et al. (2010).

[18] See Meer and West (2016).

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(2011) was not supported by the data and hence lead to problematic conclusions. Other studies, like Powell (2016), also found negative results.

Nevertheless, the synthetic control approach cannot cease the debate. Dube and Zipperer (2015) and Allegretto et al. (2017) still found small negative and insignificant effects by using data-driven approaches to select controls. They also showed that the estimates are biased without controlling the pre-exiting trends. Reich et al. (2017) also found no effect with synthetic controls. Besides, as Belman et al. (2015) reviewed, it is hard to employ synthetic controls in a long panel, since the minimum wage rises both frequently and asynchronously across states. When synthetic controls include states with minimum wage increases in the pool (such as one that Neumark et al. (2014b) used), which disobeys the concept of the quasi-experiments, the method loses its intuitive appeal. In a recent working paper using administrative data to study the effects of minimum wage increases in Seattle, Jardim et al. (2017) found results influenced by controls. Even the local area controls cannot pass the falsification test. Estimates by synthetic control are not significant, but estimates are significantly negative via an approach of interactive fixed effects. The method was also employed by Totty (2017), but no significant effect was found.

Following the debate, a challenge for future researches is not just how to select an appropriate control as in Neumark et al. (2014b), but how to select controls that can defeat all the potential alternatives. Otherwise, the debate may keep going. More recently, some studies began trying to avoid making the assumption of a counterfactual control. Harasztosi and Lindner (2017) and Cengiz et al. (2018) switched focus from panel data to wage distribution. The counterfactual they assumed is not a group of units, individuals or firms, but a wage distribution before the minimum wage changes. The former used Hungary data and found the low-wage jobs were kept with higher payment after a huge minimum wage increase, and the latter used US data and found the same results. However, this novel method needs to rely on an unclear assumption that the earnings distribution would not be affected by the minimum wage change. Meanwhile, as mentioned before in this review, the employment effect estimated by the bunching-based method may mix up effects from higher-wage workers, which would may underestimate the disemployment effect.

Other than estimating changes in the wage distribution, in a very recent working paper, Boffy-Ramirez (2019) used a within-individual difference by using an individual-level panel

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data to avoid choosing or creating a control group. They limited their sample to workers affected by the increases. The method they employed uses a four-month window to identify workers' statuses prior to and past the policy changes. In other words, they used workers' information one to three months before the adjustment in minimum wage to find out those affected, and estimated their employment status three months to one month after the increase, respectively. To avoid confounding factors, they provided estimates by controlling individual, month, and state fixed effects. They found no disemployment effect right after the increase in the minimum wage, but there was decreased labor force participation. This approach takes advantage of the capacity of identifying individual workers, but it still needs to be built on the assumption that panel data analysis requires, which is that the wage changes are not correlated with labor market factors that influence the individuals as well. Besides, since they used a very short window to identify workers, they may neglect entry effects from workers with longer duration of unemployment. Regardless of their imperfections, those few studies with new methods are favorable to provide new evidence without debatable controls.

2.2.2.2 The Affected

The elusive results of the employment effect on the specific groups of workers, especially the teenagers, has raised another stream of discussion: who is affected by the minimum wage? Concerns about focusing on particular demographic groups lead to increasing studies evaluating effects on the affected.

In fact, in the US minimum wage literature, studying youth employment has been a tradition lasting several decades. According to a previous review by Brown et al. (1982), most of the time-series studies before the 1980s only provided estimated results on youth and some only on teenagers. Even today, as Belman et al. (2015) reviewed, more than one-third of studies estimated effects on teens. The reason behind this is obvious, if the minimum wage has a negative effect, it should be more likely to have effects on teenagers since they have higher proportions of low-wage jobs. However, there are increasing doubts if teens should still receive so much attention from researchers nowadays. First, teenagers may move to higher-wage positions from the lowest-wage status in the beginning of their careers, and they also are not the main economic resource of their family. Hence, older low-wage workers may be more affected by the minimum wage policy, which aims to provide

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supports to poor households (Neumark and Wascher, 2007; Sabia et al., 2012). Second, even though teenagers have more exposure to low wages, their share of the low-wage workforce is smaller than that of older workers and it is decreasing. For example, Belman et al. (2015) noted that in the US labor market, teenagers (16-19) comprised 19% of workers who earned no more than the minimum wage; older adults at least 25 years old were the main part of these workers. Manning (2016) used US data from 1979 to 2014 and pointed out that it is odd to put so much attention on teenagers, since their share of the total employment is small and decreasing; they represented only 2% of total hours worked in 2014. On the contrary, youth with higher ages, such as 20-24 and 25-29, have higher proportions of minimum wage workers, but they were not emphasized by researchers.[19] It means that focusing particularity on teens leads to missing a comprehensive understanding of the employment effect resulting from the minimum wage (Belman et al., 2015; Manning, 2016; Allegretto et al., 2017). Third, using effects on teens may cause the estimates to be biased. One possibility is that the actually unaffected workers among the teens lead to an underestimate of the disemployment effect, if there is no effect on them (Neumark, 2018). The other opposite argument is that since teens are the minority of the low-wage workers, there may be a sample selection problem when researchers use results on teens to infer the "employment effect" of the minimum wage increases. It is worth noticing that the last two arguments can be also used to question researches on restaurant workers, which is the other main group researchers studied.

Compared to studying effects on particular demographic or industrial groups, there is still a paucity of research on the affected workers, the low-wage/low-income groups (Belman et al., 2015). One possible explanation to the insufficiency of researches on the affected is about the data. Only individual-level panel data with information on wage and employment status can be used to identify people who would be affected by minimum wage increases. Besides, it is hard to determine the changes in employment status if the frequency of the data is just annual. In other words, without monthly, or at least quarterly, individual-level panel data, it is difficult to study the effects on the affected.

Neumark et al. (2004) first tried to match the US monthly Current Population Survey (CPS) data from 1979 to 1997. The difficulty is that only households were available for doing the matching, and they were unable to use individual identifiers. In addition,

[19] Manning (2016) also found no negative employment effect on youth 20-24 and 25-29, even though they also experienced wage increases due to minimum wage increases.

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probably because of the matching problem, they just analyzed differences of wages and employment one to two years before and after. They found that low-wage workers were strongly adversely affected by minimum wage increases when they added the lagged minimum wage effect. Although the low-wage workers' wages increased due to the policy, a decline in working hours led to a net adverse consequence. A more interesting finding is that the higher-wage workers had their employment increased while their wages declined. They explained that the labor supply of higher-wage workers may increase due to the disemployment of their low-wage family members.

However, regardless of the matching problems, Neumark et al. (2004)'s conclusion was built conditionally on workers initially employed, hence it was difficult to interpret the overall employment effects. Neumark (2018) noted that more clearly. Since workers unemployed during the pre-treatment period cannot be identified, the effect on the entry into employment is neglected. Nevertheless, Neumark argued that omitting the entry effect will lead to underestimating the disemployment effect, because employers tend to decrease the turnover rates of employees due to minimum wage increases (Dube et al., 2016; Gittings and Schmutte, 2016) and low-skilled workers have higher turnover rates (Choi and Fernández-Blanco, 2017). To deal with this problem, Neumark noted that a longer-term panel data may be helpful.

Recently, by using an individual-level data from a large US retailer, Giuliano (2013) made a contribution by estimating the differences of wage and employment between the affected group and the less affected group, although they were still represented by teenagers and adults, respectively. Giuliano showed that teenagers' wages relative to adults' were significantly raised due to the federal minimum wage increase in 1996, and the higher wages also led to higher participation and employment of the teenagers in average. However, an interesting result was also found, the positive employment effect was driven by the increased entry of younger and more affluent teenagers. As a more progressive way to compare the difference between the affected and the unaffected, Clemens and Wither (2014) employed an approach of triple-difference that was from states, periods, and wage groups. The data they used is the Survey of Income and Program Participation (SIPP), which allows them to see 12 months of individual-level wage data, from August 2008 through July 2009. They used pre-treatment wages to identify the affected and found a much larger negative employment effect after the 2007 to 2009 increases in the federal minimum wage.

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However, the evidence was challenged by Zipperer (2016), which showed that the results just reflected the consequence of the recession rather than the minimum wage increase. On the other hand, they still faced the same challenge as Neumark et al. (2004). Besides, the triple-difference method they used took the higher-wage workers as the counterfactual, which may lead to biased estimates if there is a substitution effect.

In more recent working papers, Jardim et al. (2017) used quarterly administrative data from Washington state covering the period of 2005 through the third quarter of 2016 to evaluate the minimum wage increases in Seattle. Other than using the triple-difference method, they used the framework of the combination of the panel data and the quasi-experiments, but focused on low-wage workers instead of teenagers. Although there was a longer time window to identify workers, the low-wage workers they defined were not only the bound workers but also workers who had slightly higher wages. Hence, it mixed up the substitution or spillover effects. By using Integrated Public-Use Microdata Series (IPUMS) to revise CPS individual identifiers and create an individual-level panel, Boffy-Ramirez (2019) evaluated the short-run, that is, four months around minimum wage increases, employment effects through the within-individual variation. The advantage of this approach is that creating a counterfactual can be avoided. Results showed no disemployment effect, but labor participation fell. However, because of the small time window, it may neglect entry effects. From these two studies, it is observed that even though the quality of data is getting better, how to identify low-wage workers is still a challenge.

2.2.3 Substitution/Spillover Effects of Minimum Wage

2.2.3.1 Substitution Effect

Although a few studies have began to evaluate effects on the affected, virtually no study has addressed the effects on workers with wages just above the minimum.[20] To more understand the potential effects on slightly higher-wage workers, I review the literature about the substitution effect and the spillover effect of a minimum wage increase.

Going back to introductory economics, increasing the minimum wage lowers the relative wage of high-skilled workers, hence leads profit-maximizing firms to shift away from low-skilled workers toward high-skilled ones if they are substitutes in production. In other

[20] To my knowledge, only Neumark et al. (2004) separated effects on low-wage workers of different wage levels.

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words, an increased employment of higher-wage workers should be the other side of the coin to the disemployment effect. As a result, it is inappropriate to have a priori that the minimum wage has no impact on higher-wage workers. If there is labor-labor substitution, studies providing estimates including higher-wage workers will mask the disemployment effect, which is one of the explanations to why it is hard to find a significantly adverse employment effect (Neumark, 2018).

Similar to the evaluation of the disemployment effect, evidence for or against the labor-labor substitution is mostly from the particular demographic groups, which are classified by age, gender, education, or other backgrounds. For age and gender, studies found results similar to there being no substitution effect. Card (1992) evaluated the employment effect due to a minimum wage increase in 1988 in California, and found no corresponding changes in the age or gender composition, though there was a higher faction of Hispanic workers in retailing after the minimum wage increase, which is contrary to the theory expectation. Using panel data on teens from 1990 to 2009, Allegretto et al. (2011) also found older teens had no significant increase in employment, indicating no substitution toward them. However, although finding no substitution between teens and adults on average, Giuliano (2013) found there may be a substitution toward younger teens. It means that the substitution effect between teens may be from older toward younger instead. Nevertheless, based on composition, Dube et al. (2016) still found no substitution with respect to age nor gender for the restaurant workforce. They also argued that the reallocation between workers should coincide with higher employment flows, while they found less instead. Harasztosi and Lindner (2017) estimated a huge increase in minimum wage in Hungary in 2001, they found that the composition, including age and gender, of the workforce had no significant change.

However, by education or family wealth, more studies gave evidence proving the substitution effects. Neumark and Wascher (2003) analyzed effects on skill acquisition and education from the late 1970s through the 1980s, and found that minimum wage increases cause firms to substitute enrolled teenagers for non-enrolled teenagers. Fairris and Fernandez Bujanda (2008) found that after the implementation of the 1997 Los Angeles Living Wage Ordinance, workers with higher pre-treatment wages had an increased proportion of the worker composition, which reflects labor-labor substitution. Ahn et al. (2011) formed a structural search model and found that the small employment effect actually included

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substitution away from teens with poor backgrounds toward teens from more wealthy and well-educated families. Even Giuliano (2013) found that on average teens' employment increased as a result of their raised relative wages, but the increase came from teens from more wealthy families. In a poorer area, the effect may be negative instead. Recently, an opposite finding is provided by Harasztosi and Lindner (2017), which found that the composition of the workforce classified by education and location had no change after a huge minimum wage increase in Hungary.

It is worth to noticing that there is almost no study analyzing the higher-wage group of workers directly to see if substitution effects drive their increased employment. To my knowledge, Neumark et al. (2004) is the only study contributing to the literature, even though the lack of a longer period of personnel data and the methodology they used make their results not so convincing today. They separated workers into previous wage groups, such as workers with previous wages lower than new minimum minus 0.1 dollars, between new minimum $\pm$ 0.1 dollars, between new minimum + 0.1 dollars and 1.1 times new minimum, between 1.1 and 1.2 times the new minimum, between 1.2 and 1.3 times the new minimum, etc.[21] Compared to studies analyzing the composition of the workforce, they more directly looked into the effects on those workers who were supposed to gain the benefit due to the substitution effect. However, their results are more complicated than just an increase in the employment of those higher-paid workers. Those workers also had a significant wage decrease after minimum wage increases. One of their explanation is that the higher-wage workers' labor supply increased because their lower-wage family members had less working hours due to the increase in the minimum wage. And the increase of the labor supply outweighed the effect of increased labor demand due to the substitution effect. The other explanations is the substitution effect was outweighed by the scale effect, which caused firms to produce less. However, under the limit of the data and methodology, the explanations are more likely to be conjectures.

2.2.3.2 Spillover Effect

The substitution effect may lead to the wage increases for the higher-wage workers. In other words, spillovers could be easily explained by the classical theory that the labor demand of those workers increases because of their lower relative wage. However, the size of the

[21] Neumark et al. (2004) used one to two years before minimum wage increases to do classification.

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induced increase in the wage of high-wage workers actually depends on the elasticities of supply and demand for the higher-skilled workers. Besides, the wage should be gradually changed as firms may need some time to adjust their production methods to corporate more higher-skilled labor. Because of the complexity in the labor demand and supply and even the production method change, other than the effect induced by the substitution effect, the other possible mechanism of spillovers is more attractive.

Around the 1980s, there are some studies in the US literature focusing on the institutional factors influencing the minimum wage effects. The very first, Gramlich (1976) noted that except for the substitution effect, the spillovers may be from unions or higher-wage workers asking for an emulation of the increase in the wages of lowest-wage workers. Following the intuition, Grossman (1983) formed a model assuming that workers care about the relative wages rather than just their own wages. When higher-wage workers' relative wages go down because of the minimum wage increase, it makes them lower their effort. Hence, the profit-maximizing firms would also raise the wage for higher-wage workers and the price of products to nullify the impacts from the minimum wage hike. Studies such as Akerlof and Yellen (1990) extended the work-effort hypothesis to a broader field.

Evidence of the spillover effect is more from the wage distribution, for example, Dinardo et al. (1996) and Lee (1999). However, it cannot be proved where the effects came from, and more critically, using wage density to measure wage changes due to minimum wage increases may just reflect the truncation of the disemployment (Cengiz et al., 2018). More recently, Cengiz et al. (2018) used administrative payroll data to form an analysis by frequency in the wage distribution and found spillovers from minimum wage increases extending up to around 40% of the overall wage increase. However, even though they separated the incumbents and the entrants, they still needed some assumptions to let the outcome of the summation among the wage distribution reflect individuals.

Compared to the evidence or outcomes shown by the wage distribution analysis, evidence from firm-level or individual-level analysis is more mixed. Few studies provided evidence by using firm-level data. Katz and Krueger (1992) used survey data showing that some firms raised other wages to maintain their wage hierarchies. They also found that the spillover effect in higher wages was less, and the effect was different in the minimum wage increases in 1990 and 1991, in which the effect was less. Spriggs (1993) found that the wage structure was maintained, but not by raising wages for the higher-skilled workers but

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through high turnover rates. Card and Krueger (1994) also found no apparent spillover on higher-wage restaurants, although they also found no disemployment effect which is contrary to the finding in Spriggs (1993).[22]

Even until today, there have been few studies providing evidence by using individual-level data. One is Neumark et al. (2004), which showed a negative result that the higher-wage workers having their wages reduced, even though their employment was raised after minimum wage increases. Clemens et al. (2018) used survey data from 2011 to 2016 and found that employers were more likely to cut workers' health insurance perks. The cut may offset 10% to 25% of the minimum wage increase. At the same time, they also found there was a wage spillover effect on occupations with slightly higher wages before minimum wage hikes, and they also experienced health insurance cuts. However, they used occupations to categorize workers rather than their previous wages directly. Dube et al. (2019) used a payroll data from a large US retailer and noted that there was spillovers for workers earning up to 15% above the new minimum wage. But it is hard to make inference to the overall market by this finding, especially on higher-wage workers from smaller firms. Obviously, more studies providing individual-level evidence are needed as the spillover effect is the key of deciding the wage inequality effect of the minimum wage policy.

Other than the compensation change on higher-wage workers, Hradil (2018) found that the safety in the workplace deteriorated because of minimum wage increases. As not only bound workers will be influenced by the worsened security, it may become another "spillover effect" on the unbound workers in the same firms. However, until today, no other research focuses on this topic.

2.3 Policy Background and Data

2.3.1 Policy Background

The minimum wage regulation is nationwide in Taiwan with monthly and hourly rates. The monthly minimum regulates the payment of employment based on the monthly wage calculation, which could be a full-time job or a part-time job. When referring to the part-time job, the regulated amount is calculated by the proportion of actual working hours to

[22] The limit of the firm-level data analysis is the use of the firm's mean wage to proxy workers' wage status. Higher-wage firms represent that they have more higher-wage workers. The underlying assumption is that if those workers' wages were raised because employers maintained the wage structure, the mean wage of those firms should be raised too.

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the standard working hours.[23] The hourly minimum regulates the payment of employment based on hourly and daily wage calculations, both of which are for the part-time jobs.[24]

In the recent two decades, the largest minimum wage increase was introduced on 1 July 2007. At the time, the monthly minimum was increased by 9%, from 15,840 to 17,280 New Taiwan Dollars (NTD), while the hourly minimum wage was dramatically increased by about 44%, from 66 to 95 NTD. The previous adjustment of the minimum wage was in 1997 and the next one was in 2011. In other words, there is only one minimum wage change during 14 years, hence it provides a sufficient period to easily identify the policy effects. Figure 11 shows the history of minimum wage changes after 2000 in Taiwan.

To think more carefully about the different increases for the monthly and hourly minimum, the huge hike of the hourly minimum reflects a legal protection for the part-time worker. Before July 2007, the hourly minimum wage was equal to the monthly minimum wage divided by 240 hours, which is based on eight hours a day and thirty days a month. Under this setting, only monthly wage workers had paid holidays. The new regulation incorporated holiday payments into the hourly minimum wage. The standard working hours at the time were 84 hours for two weeks, so the monthly working hours were about 182 hours.[25] The new hourly minimum wage equals to the new monthly minimum wage divided by 182 hours instead of 240 hours.

As part-time workers can be employed by monthly wage contracts, not all the minimum wage part-time workers experienced a wage increase of 44%. However, based on the government's survey right after the new minimum wage increase, the share of monthly wage contracts for part-time workers was just 21%.[26] On the contrary, 78% of the part-time workers were paid by hourly or daily wages, which are affected by the hourly minimum wage. It means that part-time workers paid by the minimum wage prior to the policy change were more likely to experience a 44% wage increase rather than a 9% one.[27] On the other hand, all the minimum wage full-time workers only experienced a wage increase

[23] For example, if the worker's working hours are half of the standard working hours, then the wage is the half of the monthly wage.

[24] Workers paid a daily wage cannot be paid less than eight times the hourly minimum wage for a day.

[25] The monthly standard working hours equal yearly working hours (84*(52/2)+8) divided by 12 months

[26] The "Part-time Labour Employment Survey" was conducted by Ministry of Labor of Taiwan from 1 September 2007 to 15 October 2007.

[27] Before the minimum wage adjustment in 2007, part-time workers with hourly wage contracts could only earn 12,012 NTD (66 NTD*182 hours) when fulfilling the standard working hours. Someone working the standard hours under a monthly wage contract would have earned 15,840 NTD, which was the monthly minimum wage. After the adjustment, as the hourly minimum is calculated by the monthly minimum divided by the standard working hours, there is no wage difference for different contracts given the same working hours. Therefore, there would be no incentive for the employer to change the form of the contract.

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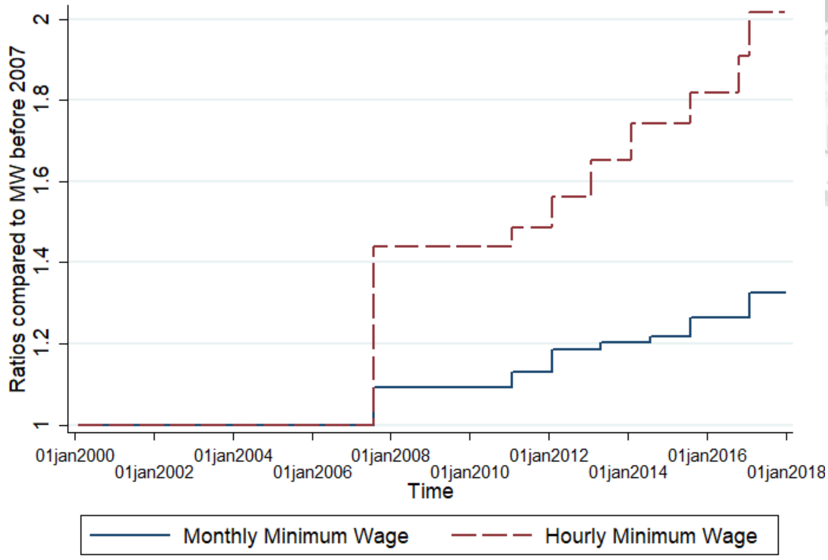

Figure 11: Minimum Wage in Taiwan

Notes: The latest minimum wage increase before the change in July 2007 was in 1997. The figure shows the ratios of the recent minimum wages relative to the original one set in 1997. Compared to the 9% increase in the monthly minimum, the hourly minimum was hugely increased by 44% in 2007 as it included a payment for holidays. This adjustment let the hourly minimum wage be equal to the monthly minimum wage divided by standard working hours.

of 9%.

2.3.2 Data Source

The data used in this study is from two sources of Taiwan. One is the monthly insured records of the labor insurance; the other is the monthly insured records of the employment insurance. Both of the insurances are mandatory and operated by the Taiwanese government. The monthly data period covers the 84 months from January 2004 to December 2010, which is three and half years before and after the minimum wage change in July 2007.

Actually, according to the law, there are some differences of the mandatory ranges in the two insurances. By combining both records, though, the data provides information of employees from companies with at least five workers or companies registered with obligations to file tax returns.[28] In other words, the only unavailable information in this data

[28] Employees here indicate three categories of workers: industrial workers, workers of commercial firms and shops, and employees in journalistic, cultural, nonprofit organizations, or cooperative enterprises. Other than those three categories, the insured laborer contains employees in government agencies and schools, workers employed in fishing production, persons receiving training in vocational training orga-

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is of employees from firms having less than five employees and having no obligation to file a tax return at the same time.[29] As there is nearly no method for the government to know the wages paid by such employers, it is the group having the highest likelihood to violate the minimum wage regulation.[30] Therefore, the employees outside of the data are potentially not affected by the minimum wage increase.

More specifically, employers with at least five employees are required to be the policyholder of the labor insurance for their employees; all employers are required to be the policyholder of the employment insurance for their employees except for employers with no obligation to be registered and file a tax return. Employers need to bear 70% of both insurances fees.[31] In 2007, the monthly insurance fee for the labor insurance was 5.5% of the employee's insured wage, and for the employment insurance the corresponding fee was 1%.[32]

The information provided by the records includes an employee's monthly insured wage, employment status, and the identification number of their employer, and individual characteristics like birth year and sex.[33] Different from the actual wage, the insured wage is set on discrete levels, which should be higher but also the closest to the actual wage.[34]

nizations, craft workers, members of fisherman's associations, and voluntary insured persons. Since the employment and the regulation of involving the labor insurance of those categories are different from ordinary employees, I exclude them from my analysis. Besides, those three kinds of workers I focus on have the main share in the insurances. For example, the proportion they were of an insured laborer for labor insurance was 63.5% at the end of June 2007, the time right before the minimum wage increase. The biggest category of others is the craft workers, who can choose whether to be involved in the labor insurance and share a proportion of 26.2% at the end of June 2007.

[29] Most of the firms with less than five employees and having no obligation to file a tax return are vendors.

[30] The only way that the government can know the wages paid by firms of this kind is to go trough labor inspection. However, it is impossible for the government to thoroughly inspect those firms.

[31] 70% of the fee is paid by the employer, 10% is paid by the government, and only 20% is paid by the employee.

[32] As the amount of the fee depends on the employee's insured wage, employers may have incentive to lower down the insured wage of their employees (Hsien-Ming Lien, 2011). Based on the Manpower Utilization Survey conducted by the Taiwanese government in May 2007, about 8.41% of employees who had monthly wages between NTD 15,000 to 19,999. On the other hand, about 30.23% of employees had insured wages between 15,840 to 20,100 in the labor insurance at the end of June 2007. If the survey is reliable, the difference may reflect that the insured wage is less than the actual wage, but it may also be due to differences in defining employees and salary. Unfortunately, neither one of the possibilities can be ruled out due to the limitation of the data. Nevertheless, possible directions of effects can still be derived based on our estimated results given there is a measurement problem.

[33] There is no direct information of employment status, but as the insurances are mandatory, the status can be inferred from the records.

[34] Before the minimum wage increase in 2007, the lowest few insured wage levels were 15,840, 16,500, 17,400, 18,300, and 19,200 NTD. For example, if there was an employee having actual wage of 17,000 NTD, then the insured wage would be 17,400 NTD rather than 16,500 NTD. Because of the rule, employees who had actual wages between 16,500 and 17,280, which were lower than the new minimum of 17,280 NTD, before the minimum wage increase, had their insured wages higher than the new minimum instead. Thus, the level of 17,400 NTD is not appropriate to represent workers who already had higher wage before the minimum wage increase.

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In addition to the monthly minimum wage being the insured wage floor for the full-time workers, there is also a floor for the part-time workers.[35]

Through the information of employment and the monthly insured wage, whether the full-time workers who were supposed to be affected by the monthly minimum wage increase can be identified. Unfortunately, there is no information on working hours or hourly wages that can be used to identify the status of part-time workers. Besides, the information on part-time employees is only available after Jan. 2006. Nevertheless, by taking advantage of this data, a matched employer-employee dataset can be generated to identify the change of the part-time employment in firms without using information before 2006.

In addition, on the firm level, the separation and hire rate can be measured by employment flows of part-time and full-time workers.[36] Besides, by combining the records of the injury benefits of the labor insurance, an additional information of firm's occupational injury rate can be used to specify whether the minimum wage increase leads to workers having a higher likelihood of occupational injury, which may reflect that employers reduced spending on workplace safety.[37]

2.3.3 Wage Distribution and Employment

Through the insurance data, the insured wage distribution at the time before the minimum wage increase and three months after the increase is shown on Figure 12. Figure 12(a) shows that the two wage distributions are very similar except for the lowest tail. Referring to the methodology introduced by Cengiz et al. (2018), the employment effect is the summation of the missing jobs under the new minimum and the excess jobs above the new minimum, and spillovers can be identified by employment changes on higher wage levels. Figure 12(b) shows that the cumulative difference turns to be positive not on the spike but on the level slightly higher than the new minimum. Based on the assumption Cengiz et al. (2018) made that the bunching range extends to wage levels a little bit higher than the new minimum, there seems to be no disemployment effect. Besides, as there are employment increases on

[35] The wage floor for the part-time workers was 11,100 NTD in 2007. If part-time workers had monthly wage lower than the floor, their insured wages would still be the floor.

[36] The separation rate at time t is the number of separations at time t divided by the total number of workers observed at time t-1; the hire rate at time t is the number of new hires at time t divided by the total number of workers observed at time t-1.

[37] The labor insurance provides benefits for employees suffering occupational injuries or diseases. The benefit record can also link to insured employees. Hence, the data in this study is much better than the data used by the solely empirical study of Hradil (2018), which excluded firms with fewer than 11 employees and only analyzed the industry-state-level variety.

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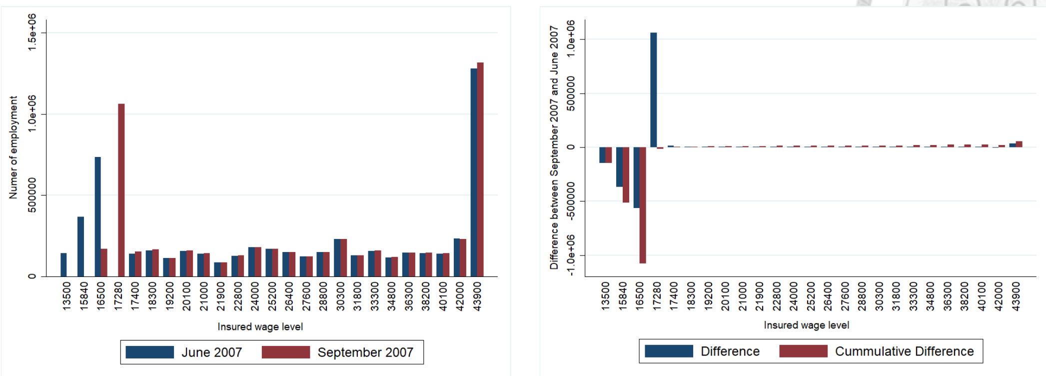
higher wage levels, there should be spillovers, although not so many.



(a) Wage Distribution (b) Difference of Employment over Wage

Figure 12: Wage Distribution before and after Minimum Wage Increase

Notes: The figure shows the insured wage distributions from the records of labor insurance in June and September 2007, which is right before and three months after the minimum wage increase on 1 July 2007, respectively. Only ordinary categories of employees are included, which are industrial workers, workers of commercial firms and shops, and employees in journalistic, cultural, or nonprofit organizations or cooperative enterprises. As the employment difference between these two distributions is close to 0 and even positive on levels above the spike, based on Cengiz et al. (2018), there should be no disemployment and slight spillover effect.

Considering that there may be some time trends that cannot be captured by the comparison of periods before and after, the total employment of full-time workers under the level of 17,400 NTD is graphed on Figure 13.[38] Although the low-paid employment was decreasing for both male and female during the time period from 2006 to 2008, there seems to be no obvious change around the time at which the minimum wage increase, which is close to the finding in the minimum wage literature. Instead, the male employment seems to increase a little right after the minimum wage increase. More specifically, when even just focusing on the young people aged 15 to 21, it is hard to observe a significant drop happening after the policy change. Of course, the graphs only show the overall employment and do not necessarily say that the affected workers were still staying in employment, as there could be some workers expelled out of employment and others entering.

As the hourly minimum wage had a huger increase, effects on the part-time workers should be more easy to observed . Since full-time and part-time workers are mixed in the wage distribution, it is hard to tell the change of the part-time workers. But from Figure 14, which shows the employment of part-time workers from 2006 to 2008, obvious

[38] 17,400 NTD is the higher and closest level to the new monthly minimum wage NTD 17,280 in 2007.

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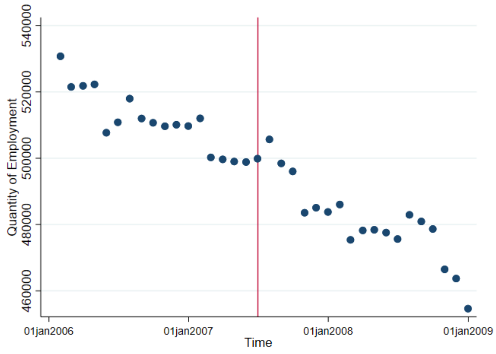
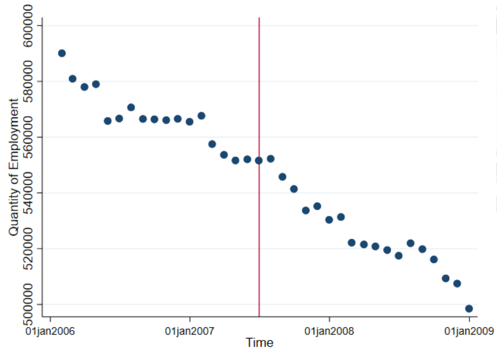
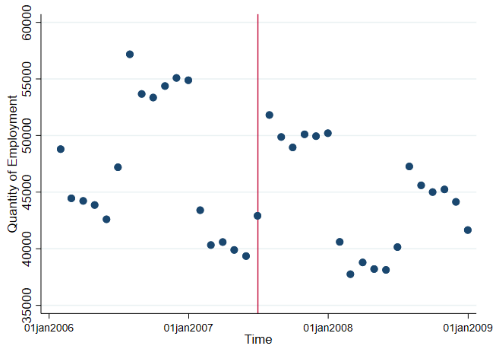


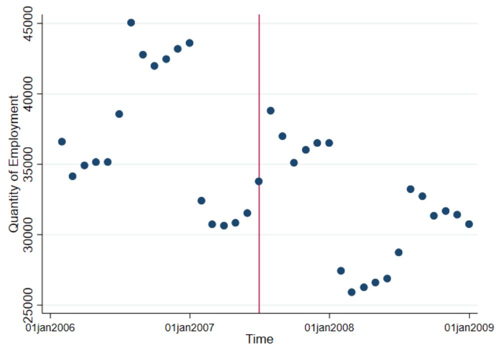
(a) Male



(b) Female



(c) Male with Aged 15-21



(d) Female with Aged 15-21

Figure 13: The Number of Low-paid Full-Time Workers

Notes: The figure shows the over time employment of low-wage full-time workers from 2006 to 2008. Every bin is the number of employees at each month. The red line represents June 2007, which is the latest month before the minimum wage increase. The low wage here is defined as not greater than the level of 17,400 NTD, which is slightly higher than the new minimum and exists both before and after the new minimum wage increase. Suppose that the affected workers' wages are not raised to higher than the new minimum wage, no matter before or after the policy change, all the affected full-time workers should be at levels not greater than 17,400 NTD if they are still employed.

employment increases happened just after the minimum wage increase. Although the part-time employment usually had increases in July, the one after the new minimum seems to reflect a raise in the level of employment rather than a very short-run change, especially among the females. Compared to the prior employment, about 70,000 for males and 80,000 for the females, the discontinuous increases of about 10,000 for both males and females are not small. Considering that teens may be more influenced by the hourly minimum wage increase, Figures 14(c) and 14(d) also show employment of the part-time workers aged 15 to 21, but it is hard to notice any change after the minimum wage increase.

The phenomenon of the significant employment increase on the part-time workers is

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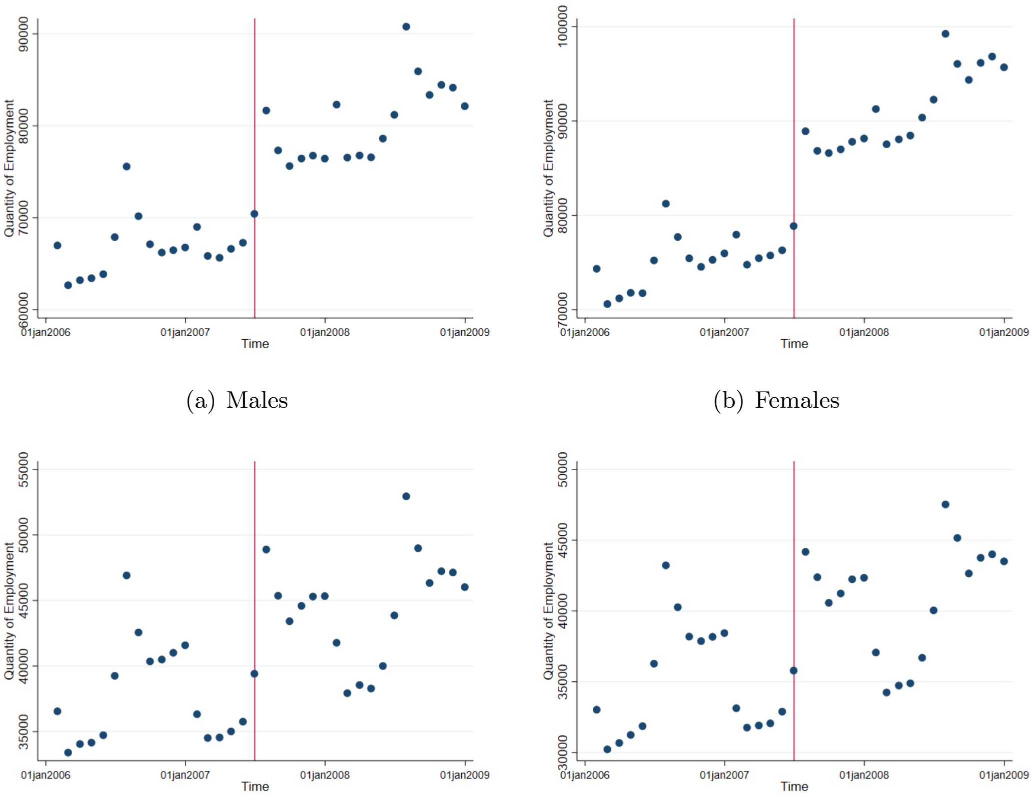
striking to the literature, in which few studies focused on the different effects between the full-time and part-time workers, see Belman et al. (2015). However, notice that because of the lack of information on hourly wage and working hours, high-wage part-time workers cannot be separated from the overall employment.[39] Hence, the employment change may be from either bound or unbound part-time workers.



(a) Males

(b) Females

(c) Males Aged 15-21

(d) Females Aged 15-21

Figure 14: The Number of Part-Time Workers

Notes: The figure shows the employment of all part-time workers from 2006 to 2008. Every bin is the number of employment at each month. The red line represents June 2007, which is the latest month before the minimum wage increase. Because of the lack of hourly information, part-time workers who had hourly wages above the new minimum prior to the change are also included in the figure.

2.4 Empirical Methodology and Data Setting

In this section, I firstly show the methodology used for the firm-level and the individual-level analysis, then show the data setting followed by the methodology. The firm-level

[39] The "Part-time Labour Employment Survey", conducted by the Ministry of Labor of Taiwan from 1 September 2007 to 15 October 2007, showed that 44.3% of part-time workers already had higher hourly wages than the new minimum.

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analysis in this study includes specifications for the minimum wage effects on the average wage, employment stock, and also the employment flow in firms; the individual-level analysis includes the wage, employment, and the earning effects of the affected workers, and also the wage difference between lowest-wage and slightly higher-wage workers. Those analyses will be presented in sequence when detecting the minimum wage effect on the wage/earning gap.[40] But considering the similarity, the strategies are listed together categorized by firm- or individual level data.

2.4.1 Empirical Methodology

The dispute on the minimum wage effect mainly results from controversy over the methodologies. Although the discontinuous boundary comparison (Dube et al., 2010; Allegretto et al., 2011) has combined the advantages of panel data analysis via the two-way fixed effect framework (Neumark and Wascher, 1992, 2007) and the quasi-experiments methodology (Card and Krueger, 1994), it has raised concerns about the technique of choosing appropriate controls (Neumark et al., 2014b; Neumark and Wascher, 2017). The synthetic controls may improve the adequacy of the counterfactual, but they also generate concerns about the difficulty to rule out groups actually affected by minimum wage from the controls when analyzing a long-term panel (Belman et al., 2015). In addition, because of different techniques of forming controls, the estimated results now more rely on controls (Jardim et al., 2017) rather than the real effect. To address this dilemma, this study employs the approach of the regression discontinuity design and is based on monthly administrative data and a sole increase in the nationwide minimum wage in Taiwan.

2.4.1.1 Models for Firm-level Analysis

Previous studies analyzing the firm-level effects employed the variations between firms of outcomes and the wage changes due to minimum wage increases (Card and Krueger, 1994; Hirsch et al., 2015). One limit from this method is the need to take the wage change as an exogenous variable, hence it cannot be used to specify the actual wage changes firms had. Besides, it may lead to biased results if there are endogenous correlations between the change of wages and the outcomes, even if taking a counterfactual as a control. The method this study uses employs the implementation time as the instrument variable reflecting the

[40] In this study, the wage gap means the wage difference between employed workers; the earning gap adds the employment effects on them.

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shock from the minimum wage increase to rule out the endogeneity, and uses it to evaluate the wage effect on firms. In other words, I form a sharp regression discontinuity design by taking the time periods as the running variables. The model is as follows.

$$y_{jt} = \alpha_0 + \alpha_1 D_{jt} + \alpha_2 Time_t + \alpha_3 (D_{jt} \cdot Time_t) + \theta X_{jt} + \epsilon_{jt} \tag{6}$$

On the left hand side of Eq. (6), y_{it} indicates outcomes in firm j at time t, such as the average wage, employment stock of low-wage workers, and the employment flow which can be separated into the separation rate and hire rate. To be a supplement to knowing the firms' potential adjustments to the minimum wage workers, the occupational injury rate is also included in the outcomes.[41]

On the right hand side of the equation, D_{jt} is the dummy variable representing the discontinuity. It is equal to 1 if observations are on the right hand side of the cutoff of time, which is the implementation time of the new minimum wage, and is equal to 0 otherwise. $Time_t$ controls the time trend. The term of $D_{jt} \cdot Time_t$ allows observations on the right hand side to have different trends from the left hand side. X_{jt} contains characteristics of firms like their scales, regions and industries.[42] α_1 is the coefficient showing the discontinuous change of outcomes after the cutoff time.

The only assumption needed to evaluate the causal effect of the minimum wage increase is that the firms' outcomes should continuously change over time when the policy has no effect. It means that D_{jt} has no correlation with other unobservable factors that have influence on the outcomes. In a more simple way: $Cov(D_{jt}, \epsilon_{jt}) = 0$. Even when controlling the time trend, in the labor market the specific years and months may still have influences on firms' outcomes. Hence, Eq. (7) is the model excluding the influences from years and months.

$$y_{jt} - y_t^p = \alpha_0 + \alpha_1 D_{jt} + \alpha_2 Time_t + \alpha_3 (D_{jt} \cdot Time_t) + \theta X_{jt} + \epsilon_{jt} \tag{7}$$

[41] All the outcomes are analyzed by values rather than log values as to be consistent to the analysis of the individual level, in which results are needed to be calculated with proportions of wage groups. This part will be discussed later.

[42] Firm scale as variable changes across time and set by levels, which are less than 30 employees, 30 to 99 employees, 100 to 499 employees, 500-999 employees, and above 1000 employees. On the other hand, the variables of region and industry of a firm are the same over time. The former is a dummy variable defined by being located in the capital area, Taipei, or not; the latter is a dummy variable defined by belonging to the service industry or not. Variables of region and industry can be used to see the composition in the firms.

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In this model, the outcome variable is replaced by the residual of the outcome. y_t^p is the predicted value and is only estimated by the year and month fixed effects, and the difference between the actual value and the predicted value excludes the potential effects related to specific years or months.

2.4.1.2 Models for Individual-level Analysis

In the very small literature of analyzing the employment effects on the affected, most of the studies followed the approach of difference-in-difference with the additional difference between affected and unaffected individuals, which were distinguished by workers' prior wages (Connolly and Gregory, 2002; Stewart, 2004; Clemens and Wither, 2014). However, taking workers with higher wages as controls neglects the potential substitution effect, and hence leads to biased estimates (Neumark, 2018). Although Neumark et al. (2004) separated effects on the different wage groups, the method they used is based on the traditional panel data analysis, which may just reflect the correlation rather than causal effect of minimum wage increases (Dube et al., 2010; Allegretto et al., 2011). To estimate the causal effects on the bound and also the unbound workers at the same time and to avoid the dispute about controls, the method used on the individual-level is also based on the approach of sharp regression discontinuity design.

The methodology is similar to the settings of the firm-level analysis, but with two different groups of workers.[43] One is the directly affected workers and the other is the indirectly affected workers. The former is workers bound by the new minimum wage, that is, they had lower wages than the new minimum prior to the increase. Hence, their wages would be at least mechanically raised to the new minimum wage level. The latter is workers unbound by the new minimum but just had slightly higher wages. If there is a substitution effect, their employment should be increased and also possibly their wages; if there is a equity effect, their wages should be increased without employment change. The basic model is as follows.

$$y_{it}^l = \gamma^l + \beta^l D_{it} + \theta^l Time_t + \eta^l (D_{it} \cdot Time_t) + \epsilon_{it}^l \tag{8}$$

[43] There are two differences between the settings of the firm- and the individual-level analysis. First, there is no characteristic variable for the individual level. As only employed workers can be observed, adding covariates means restricting the population to the employed. Second, the indivudual-level data is set as the balanced panel. If workers are missing in the record at some periods,then they are out of employment during those periods. To track their employment status, the missing observation is replaced as a status of non-employment.

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$$y^h_{kt} = \gamma^h + \beta^h D_{kt} + \theta^h Time_t + \eta^h (D_{kt} \cdot Time_t) + \epsilon^h_{kt} \tag{9}$$

The characters of l and h represent the directly affected and indirectly affected workers, respectively. y^l_{it} is the outcome of the directly affected workers i at time t. On the other hand, y^h_{kt} is the outcome of the indirectly affected workers k at time t. D_{it} and D_{kt} are dummy variables that reflect whether they are on the right hand side of the time cutoff. The setting of $Time_t$ and the interaction between D and $Time_t$ is the same as the firm-level setting. ϵ^l_{it} and ϵ^h_{kt} are residuals of the outcome that cannot be captured by this model. β^l and β^h are our interests, they represent the discrete change of the outcome after the new minimum wage is implemented. Notice that the analysis would be conditional on employed workers if the personnel characteristics are added into the model, so those variables are not included. But the exclusion of personnel characteristics should have no influence on the results, since they have no variation in a short period. Instead, characteristics like age and gender are used to set the data group.

The underlying assumption is also that under controlling the overall time trend and the time trend after the implementation, the D has no correlation with the ϵ. To rule out the potential influence of specific month and year effects, the models are improved as follows,

$$y^l_{it} - y^p_t = \gamma^l + \beta^l D_{it} + \theta^l Time_t + \eta^l (D_{it} \cdot Time_t) + \epsilon^l_{it} \tag{10}$$

$$y^h_{kt} - y^p_t = \gamma^h + \beta^h D_{kt} + \theta^h Time_t + \eta^h (D_{kt} \cdot Time_t) + \epsilon^h_{kt} \tag{11}$$

To rule out specific year and month effects on the low-wage workers, including both groups, rather than the specific year and month effects on the directly or the indirectly affected workers, the predicted value by the month and year fixed effects is estimated from the mix of these two kinds of workers, as presented as y^p_t, rather than from their own groups. Otherwise, the difference of policy effects between these two groups may be eliminated if the specific time effects of specific groups are fixed.

The goal in this study is to find out the minimum wage effect on the wage/earning gap. The main strategy is to estimate the effects on individual groups of workers. Except for that, estimating the outcome difference between different groups of workers will also provide some evidence to the effects on inequality. Following Eqs. (10) and (11), their

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difference can be shown by the following equation.

$$\overline{y_t^h} - y_{it}^l = \gamma^h - \gamma^l + (\beta^h \overline{D_t} - \beta^l D_{it}) + (\theta^h - \theta^l) Time_t + (\eta^h \overline{D_t} - \eta^l D_{it}) \cdot Time_t + \overline{\epsilon_t^h} - \epsilon_{it}^l \quad (12)$$

On the left hand side of Eq. (12), the outcome difference at time t between the two groups is calculated by the difference between the actual observation of the directly affected group and the mean outcome value of the indirectly affected group at time t, shown by $\overline{y_t^h}$. As there are two panels for estimation, differences cannot be calculated between individuals of different groups. On the right hand side of the equation is the difference of models of their own groups. As the value of $\overline{D_t}$ depends on the time t, it is the same as D_{it}. That is, $\overline{D_t}$ equals to D_{it} at time t. Hence, Eq.(12) can be rewritten to be the model for estimating the gap.

$$\overline{y_t^h} - y_{it}^l = \Delta\gamma + \Delta\beta D_{it} + \Delta\theta Time_t + \Delta\eta (D_{it} \cdot Time_t) + \Delta\epsilon_{it} \quad (13)$$

In which, $\Delta\gamma = \gamma^h - \gamma^l$; $\Delta\beta = \beta^h - \beta^l$; $\Delta\theta = \theta^h - \theta^l$; $\Delta\eta = \eta^h - \eta^l$; $\Delta\epsilon_{it} = \overline{\epsilon_t^h} - \epsilon_{it}^l$.

$\Delta\beta$ is the coefficient representing the effect on the gap between these two groups. Under the same assumption of Eqs. (10) and (11), D would be correlated to neither ϵ_{kt}^h nor ϵ_{it}^l, hence $\Delta\beta$ would also be unbiased. Furthermore, the only assumption Eq. (13) needs is weaker, which is $Cov(D_t, \overline{\epsilon_t^h} - \epsilon_{it}^l)$ equals to 0. It means that whether observations are in the period after the policy change or not has no correlation with other factors influencing the difference. As the effects from specific months and years are on both groups, the difference estimation automatically rules out those effects. In addition, other unobservable factors which may influence the two groups at the same time period are also automatically excluded in Eq. (13) but may still exist in Eqs. (10) and (11).

If the indirectly affected workers received null effect from the minimum wage increase, then the model of Eq. (13) is more like the method of difference in difference that controls the specific time trend, $D_{it} \cdot Time_t$. However, the assumption is very strong and not reliable in reality.

2.4.2 Data Setting

The data used for the firm-level and the individual-level analyses will be described separately in this section, including the method to form the dataset, the descriptive statistics of data, and the figures showing the outcome's pattern related to the analytic methodol-

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ogy. The strategy used to address the challenge of classifying workers by using observable wages (Neumark et al., 2004; Neumark, 2018), and the following adjustment strategy for estimation for the individual-level analysis are also shown in this section.

2.4.2.1 Data Setting for Firm-level Analysis

In the monthly records of the labor insurance and employment insurance, every insured employee is linked to the identification number of their firm. During the research period, Jan. 2004 to Dec. 2010, the number of firms is about 400 thousand in every month. Through the firm's ID number, I generate a matched employee-employer dataset that contains the composition of workers, the average and total wages, the rate of employment flow, and also the characteristics of the firms. As minimum wage may have less effect on large scale firms, a subsample composed of firms with less than 30 employees, is also generated in this study. Table 12 shows the firms' descriptive statistics.

The total number of observations for all firms is about 35 million, which is composed over 84 months with about 400 firms in each month. Interestingly, small scale firms share 95% of the total number of observations. Due to this fact, the employment of all firms has a mean of 13.26 workers but with a high S.D. of 117.35. Comparatively, small scale firms have a mean of 6.28 with S.D. of 14.69. The scale difference also influences other variables related to employment, including employment compositions and total wages.

Compared to others, small scale firms are more likely to be directly and indirectly affected by the minimum wage increase. On average, a small scale firm has 6.12 full-time workers with up to 2.21 (35%) bound by the new minimum wage, and 0.62 (10%) with wages slightly above the new minimum. On the other hand, when including full time workers from larger scale firms, the proportions for bound workers and slightly higher-wage workers drops to 23% and 8%. However, it is not the case when it comes to the part-time workers. In the whole sample, 3.2% of employees are part-time workers, but the corresponding percentage for small scale firms is only 2.5%. Since the share of part-time workers is small, firms with small scale should be more expected to be affected by the minimum wage increase.

Average wages are similar between the total and subsample, while total wages are obviously different. Other variables including the separation rate, hire rate, and occupational injury rate are also similar across different scales. The separation and hire rates are defined

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by the number of separations/hires at time t divided by the number of employees at time $t-1$. As expected, those rates are higher among part-time workers than full-time workers, which are 10%-12% and 3%-4%, respectively. The occupational injury rate is calculated by the total number of employees experiencing occupational injury divided by the total number of employees. It is only about 1‰ overall and 1.4‰ in the most risky industries (manufacturing, construction, water supply and remediation activities). To avoid larger scale firms from attenuating the estimated results, small scale firms are the main target analyzed in this study.

Density and Composition

Following the methodology of regression discontinuity design, estimation may be problematic due to two reasons. First, the observations endogenously move around the cutoff. To the study of firms' outcomes, the possibility of this case would be firms leave the market and hence cause the estimation only on survivals. If it is the case, then the density of the observations would change around the cutoff. Second, even if the density has no change, observations around the cutoff may have different characteristics due to the policy change. The case here would be that the minimum wage increase changes the composition of firms, so the results may just reflect the change of composition rather than the real effects on the same firms.

Figure 15(a) shows the number of small scale firms across 84 months. It is hard to tell any obvious change in the number of firms. It means that although the small scale firms have 35% of employees directly affected by the minimum wage increase, they are not forced to leave the market right after the policy change. Hence, the overall change of a firm's outcome would not result from the firm's closure. Figures 15(b) and 15(c) show the ratio of small scale firms in the service sector and the restaurant industry, respectively. As the service sector and especially the restaurant industry have high ratios of low-wage workers, the minimum wage increase may raise their costs much higher than other industries. The potential mechanism that keeps the whole number of firms the same is firms from other sectors entering the market while firms of such vulnerable industries do withdraw from the market. However, it is obviously not the case shown in Figures 15(b) and 15(c). Other than the industry, a similar situation may occur related to the location. As firms in the Taipei area may be more influenced by the minimum wage increase because of a higher service sector ratio and higher enforcement, the ratio of firms in Taipei may decrease due

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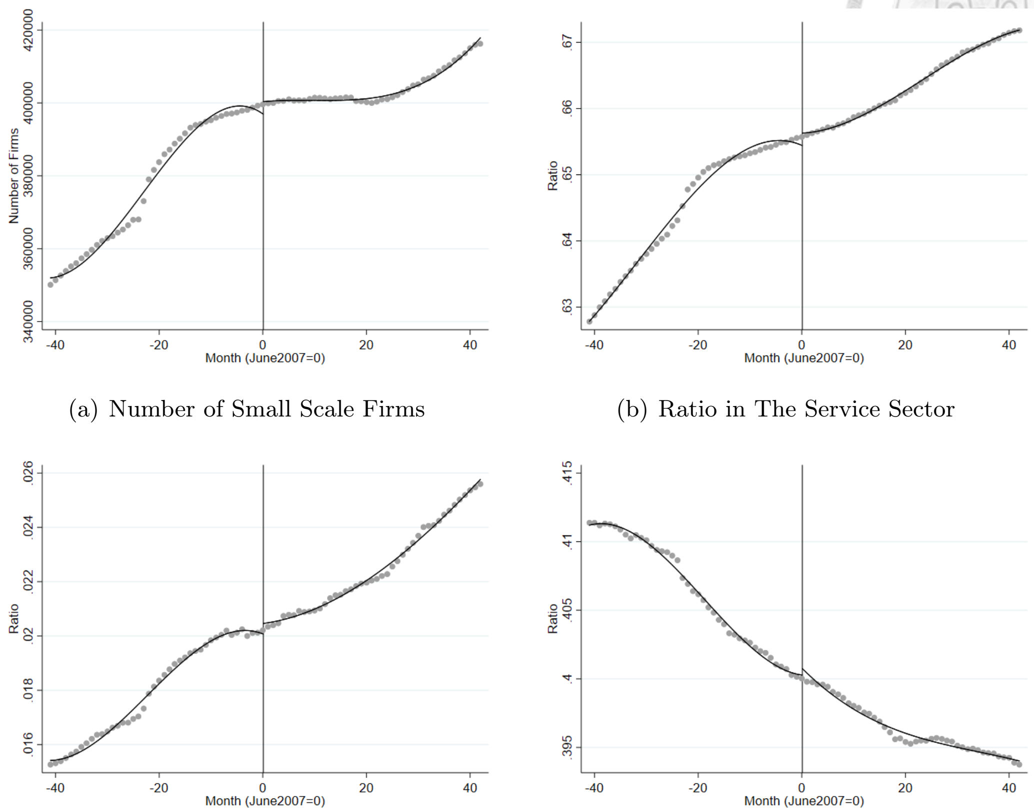
to the policy. However, Figure 15(d) cannot prove that hypothesis either.



(a) Number of Small Scale Firms

(b) Ratio in The Service Sector

(c) Ratio in The Restaurant Industry

(d) Ratio in Taipei

Figure 15: The Density and Composition of Small Scale Firms

Notes: This figure shows the number and the compositions of firms with less than 30 employees. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The whole time period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is There is no weighting to all observations. It is hard to find obvious decreases for the number of firms and for ratios for the service sector, the restaurant industry, and firms in Taipei area.

By ruling out the possibilities of the density change and composition change, the change of firms' outcomes around the cutoff time should reflect the changes within firms. The rest of this section shows the patterns over time of the outcomes of interest.

Employment Stock

To test the hypothesis of equity effect and also the substitution effect, the variables of interest include the employment stocks of different wage levels and also the average wages. If it is the case of the substitution effect, the lowest-wage jobs would go down along with an increase in the employment of higher-wage workers. Besides, wage spillovers existing or

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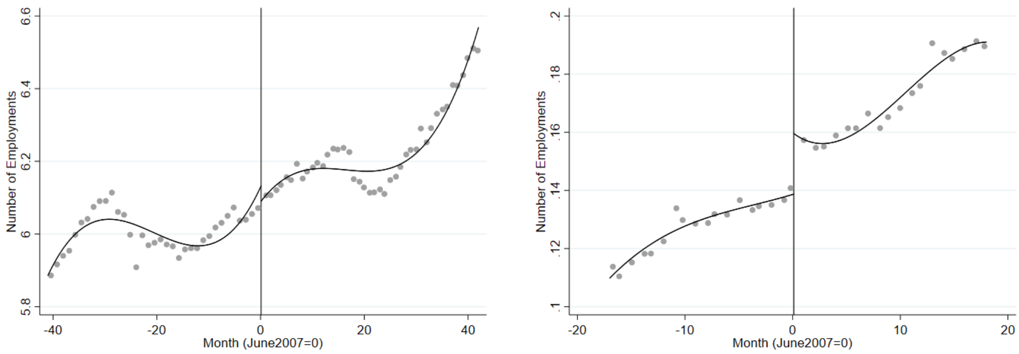
not depends on the demand and supply for the higher-skilled workers. On the other hand, if it is the case of equity effect, employment would stay the same while the wage would have spillovers.

The average number of full-time workers and part-time workers in small scale firms are shown in Figure 16. As the information on part-time workers is not available until 2006, the time period for the part-time employment is shorter, which is from January 2006 to December 2008, while the period for full-time is from January 2004 to December 2010. The change among full-time workers is not obvious, but there is an obvious jump from 0.14 to 0.16 for part-time workers. It means that without considering any other factors, the data shows that the part-time employment of small scale firms has a 14% increase after the hourly minimum wage is raised by 44%.



(a) Full-Time Employment (b) Part-Time Employment

Figure 16: Average Number of Employees in Small Scale Firms

Notes: This figure shows the average number of employment on both full-time and part-time workers in small scale firms. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period for full-time employment is from Jan. 2004 to Dec. 2010. As the information on part-time employees is not available until 2006, the graph for part-time employment only shows the period between Jan. 2006 to Dec. 2008. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

The composition of full-time workers is shown in Figure 17. The full-time employment with bound wages (17(a)) is decreasing over time, but a negative result around the cutoff cannot be easily identified.[44] Correspondingly, a change in the employment of workers

[44] Here the wage level 17,400 NTD is used to represent the new monthly minimum wage, as this level is the closest level to the new minimum wage and exists on both sides of the cutoff time. Prior to the

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with unbound wages (17(b)) is not easily observed either. However, the employment of those with wages one level higher (17(c)), and one to three levels higher than the new minimum (17(d)) seem to have increased after the policy change.[45] As those wages are higher than the new minimum wage, bound workers' wages would not be raised to those levels. Hence, a possible explanation would be some other workers are transited to those levels, including from non-employment, unbound wage workers from lower wage levels, and even higher-wage workers.

Compared to full-time employment, part-time employment has a obvious increase after the policy change. But unfortunately, the data has no hourly information that can be used to separate part-time workers on different wage levels. However, since the proportion of unbound part-time workers is only 44.3%, all the increases in part-time workers would be contributed to them only if there is a dramatic employment increase on them. Especially, workers who are expected to gain the substitution benefit are of a lower proportion than the total unbound.

Average Wage

The average nominal wages for full-time and part-time workers are shown in Figure 18. Both the full-time and the part-time workers in the small scale firms seem to have nominal wage increases after the policy change. But when considering the real value, those increases seem to be dramatically eliminated by the consumer price index (CPI).[46] This interesting outcome may reflect the hypothesis of the equity effect, in which firms raise both the wage and also the product price, hence nullifying all the influences from the wage increases.

However, the average real wage mixes wages of directly and indirectly affected workers, who should be separated by their prior wages. As it is impossible for the firm-level analysis using wages to separate workers and observe their wage changes at the same time, strong assumptions are needed to use the firm-level outcome to make a conclusion.

In addition, the results of real wages may be influenced by the pattern of the CPI. Figure 19 shows the monthly CPI.[47] Rather than increasing right after the minimum wage

increase, insured wage levels under 17,400 NTD are 15,840 and 16,500 NTD. If workers with insured wages lower than the new minimum are laid off because of the increase, then the total number of workers with wages not greater than 17,400 NTD would decline after the cutoff time.

[45] The level one level higher than the new minimum is 18,300 NTD, and the level three levels higher than the new minimum is 20,100 NTD, which is about 16% higher than the new minimum.

[46] The real value is deflated by the monthly consumer price index of Taiwan.

[47] The vertical axis is the relative ratio to the CPI of Sept. 2015.

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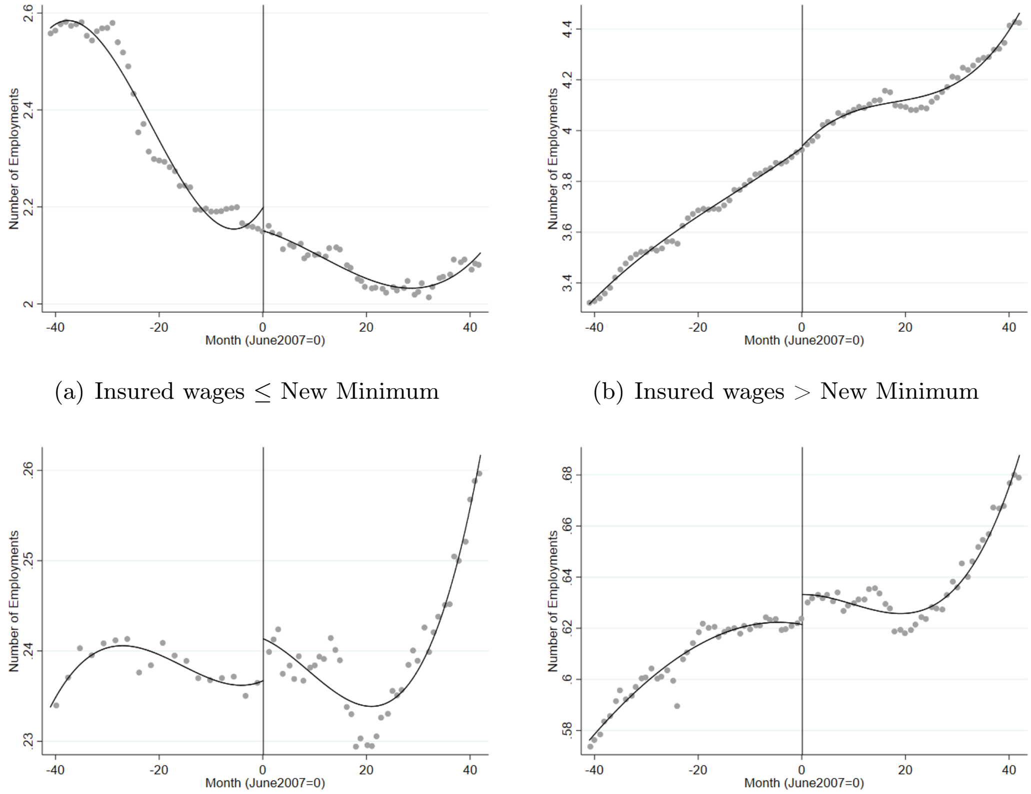

(a) Insured wages ≤ New Minimum

(b) Insured wages > New Minimum

(c) One Level Higher than New Minimum

(d) One Level Higher to Three Levels Higher

Figure 17: Small Scale Firm's Full-Time Employment Divided by Wages

Notes: This figure shows the average number of full-time employees for various wage levels. As there was no insured wage level on the new monthly minimum wage before the policy change, here I use the closest level of 17,400 NTD to represent it. The level which is one level higher than the new minimum is 18,300 NTD, and the level three levels higher than the new minimum is 20,100 NTD. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

increase, the CPI of July 2007 is lower than the value of June 2007. But after that, the CPI seems to have an upward shift compared to previous months. As the dataset in this study has no information on product prices of firms, the minimum wage effect on the prices is not allowed to be identified in the firm-level analysis.

Although I can assume that there is no other event besides the minimum wage increase occurring around the cutoff time to affect the CPI, the assumption is too strong in this micro analysis. Instead, I take the CPI as a price level firms face to, and they can choose

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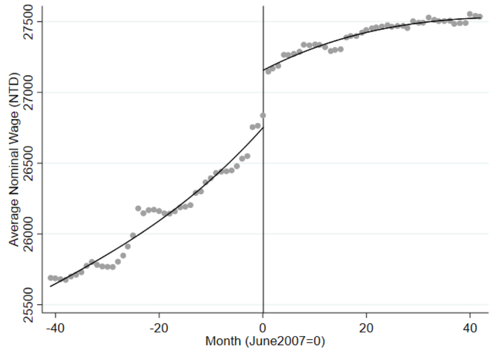
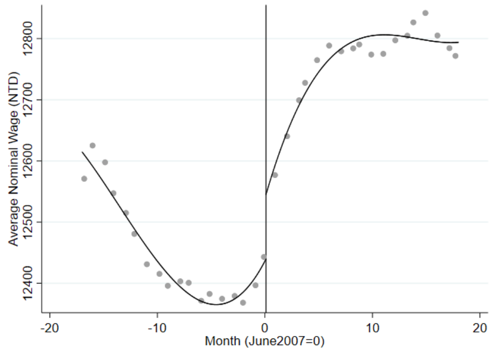
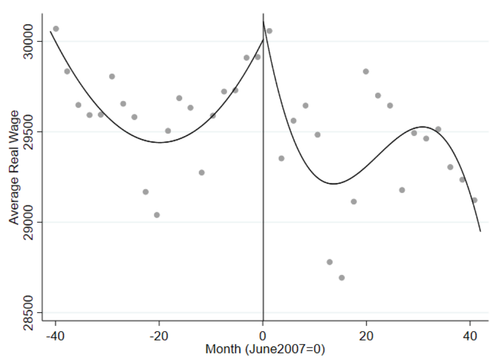


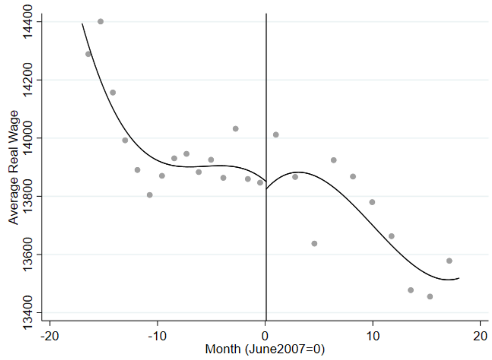
(a) Nominal Value – Full-Time



(b) Nominal Value – Part-Time



(c) Real Value – Full-Time



(d) Real Value – Part-Time

Figure 18: Average Wages in Small Scale Firms

Notes: This figure shows the average wages in the nominal and real value on the whole full-time and the whole part-time employment in small scale firms. The real value is deflated by the monthly consumer price index. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. As the information on part-time employees is not available until 2006, the graphs for part-time employment only show the period between Jan. 2006 and Dec. 2008. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

the nominal wage payment to form the real wage for workers. In other words, the CPI is a confounder that may affect estimated results of real wages. To understand the influence the CPI may have, Figure 20 shows graphs of the CPI in the RD form.

Figure 20(a) shows that the CPI may have no obvious change under the RD plotting in the whole time period, as the CPI fell rather than increased in July 2007. But, if the cutoff is adjusted from June 2007 to July 2007, as shown in Figure 20(b), the CPI would be raised. It means that if we estimate in the global scale, we may neglect the rise of the CPI because it fell right after the minimum wage increase. On the other hand, even in the

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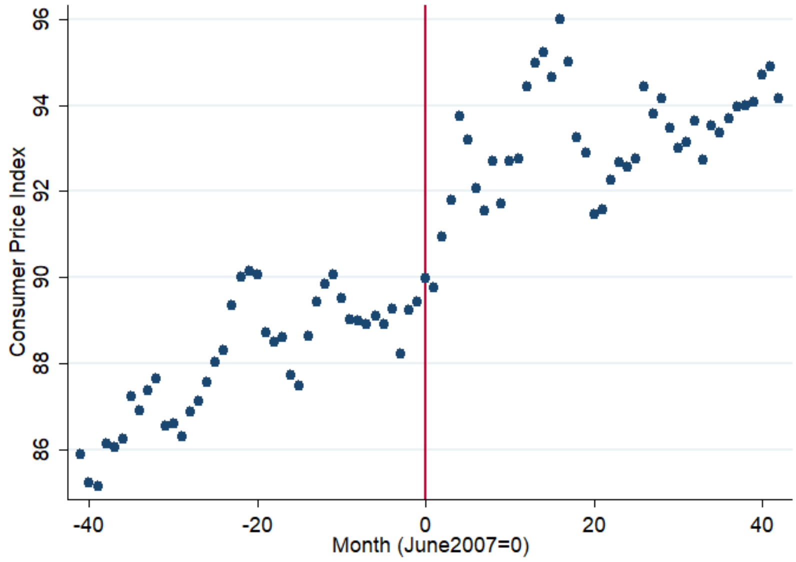

Figure 19: Monthly Consumer Price Index

This figure shows the monthly consumer price index (CPI) of Taiwan. The vertical axis is the relative ratio to the CPI of Sept. 2015. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. Every bin is the CPI for the corresponding month.

local scale, the selection of bandwidth can also lead to different results. Figure 20(c) uses a short bandwidth with five months before and after the cutoff. As there is no weighting, the shorter bandwidth leads to magnifying the influence of the fall in July 2007, hence it seems to cause a decline on the CPI rather than an increase. On the contrary, when extending the bandwidth to ten months before and after the cutoff time, as shown in Figure 20(d), the influence of the fall is weakened and may lead to a more reliable result. This finding will be considered in the following estimation.

Total Wage

As a combination of the employment stock and the average wage, the total wage of firms can be used to observe whether firms' labor costs are raised due to the minimum wage increase. Figure 21 shows the nominal and the real value of total wages on the full-time and part-time employment in small scale firms. As the employment and average nominal wage seem to increase for both full-time and part-time employment, it is not surprising that the total wages of those two kinds of employment seem to increase too (21(a) and 21(b)). However, although the total real wage for the full-time workers (21(c)) has an

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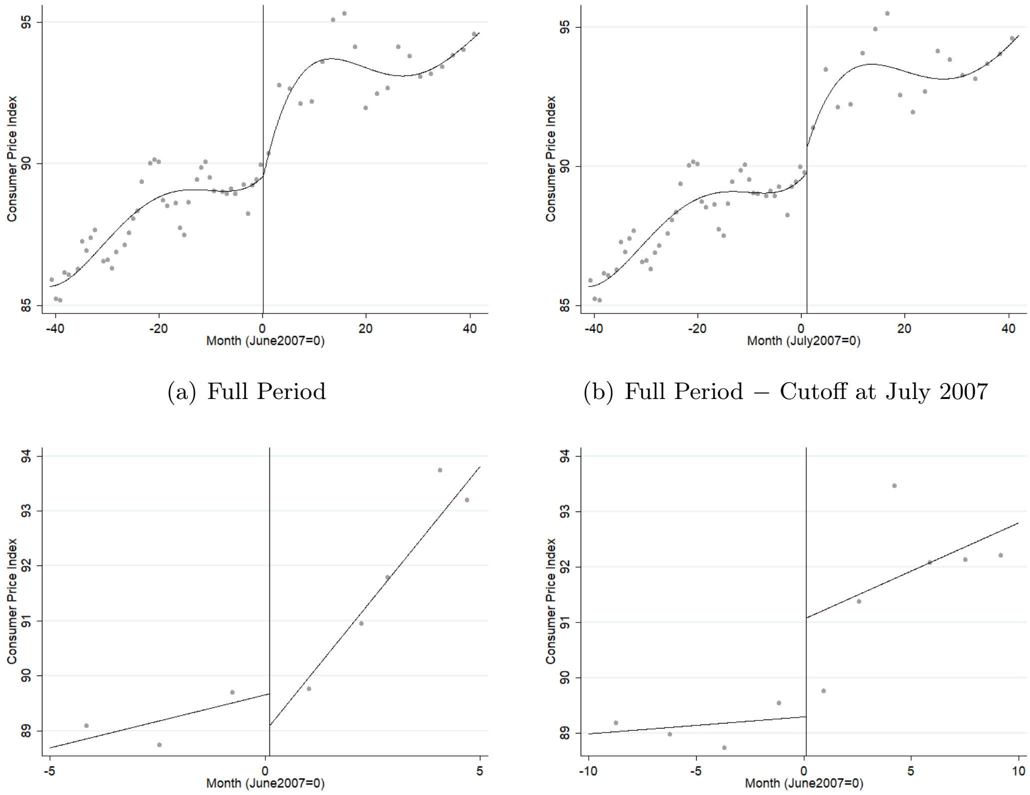

(a) Full Period

(b) Full Period – Cutoff at July 2007

(c) Five Month Bandwidth

(d) Ten Month Bandwidth

Figure 20: CPI in Regression Discontinuity Design

Notes: This figure shows the CPI in a form of regression discontinuity design. The vertical axis is the relative ratio to the CPI of Sept. 2015. The horizontal axis shows the number of months from June 2007 in graphs (a), (c), and (d), but from July 2007 in graphs (b). Hence, there is a vertical line at June 2007 in the three graphs, and at July 2007 in graph (b). The data period in graphs (a) and (b) is from Jan. 2004 to Dec. 2010; the data period is from Jan. 2007 to Nov. 2007 in graph (c) and from Aug. 2006 to Apr. 2008 in graph (d). In each graph, the number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. In graphs (a) and (b), the black curve is the global polynomial fit with the order of 4 for each side of the cutoff; in graphs (c) and (d), it is the local polynomial fit with the order of 1 for each side of the cutoff. There is no weighting to all observations.

ambiguous effect because of the CPI change, the total real wage for the part-time workers (21(d)) still has an obvious upward shift right after the minimum wage increase. It means that the employment effect on the part-time workers outweighs the increase of the CPI.

Employment Flow

To more understand the minimum wage effect on workers, a developing literature has focused on the employment flow (Brochu and Green, 2013; Dube et al., 2016) and has found evidence in line with theories of search frictions in the labor market. Through firm-level

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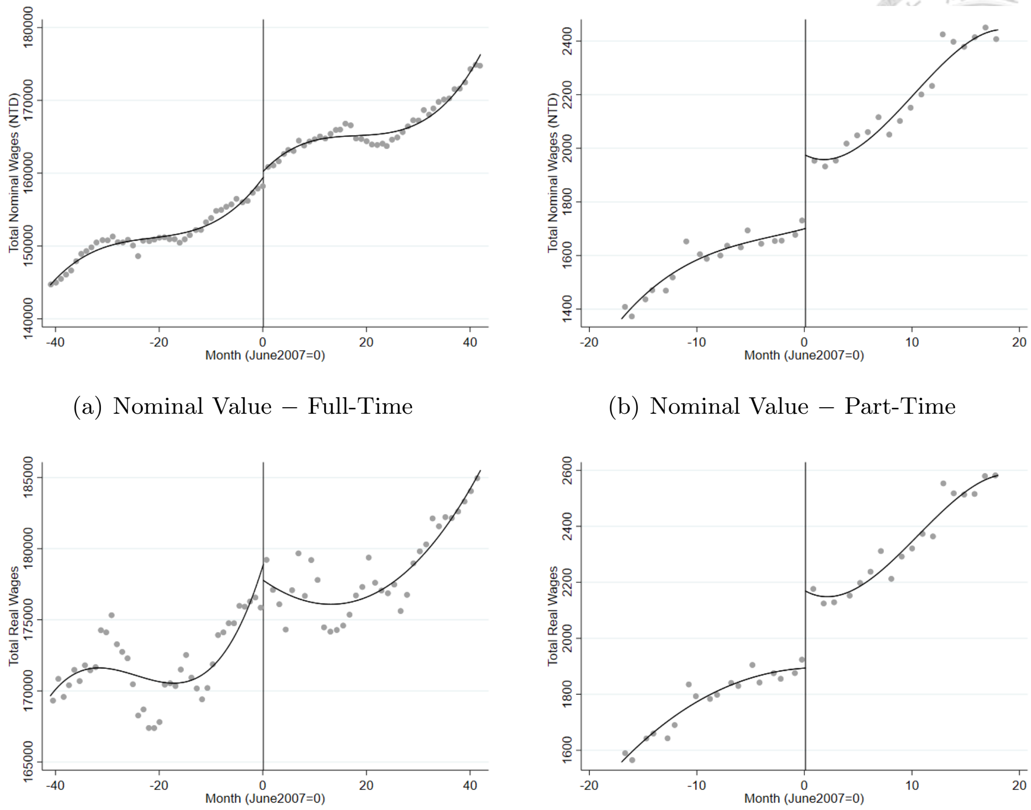

(a) Nominal Value – Full-Time

(b) Nominal Value – Part-Time

(c) Real Value – full-time

(d) Real Value – part-time

Figure 21: Total Wages in Small Scale Firms

Notes: This figure shows the total wages in the nominal and real value on the whole full-time and the whole part-time employment in small scale firms. The real value is deflated by the monthly consumer price index. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. As the information on part-time employees is not available until 2006, the graphs for part-time employment only show the period between Jan. 2006 and Dec. 2008. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

analysis, the employment flow is also focused on and used to see whether the search frictions can explain the findings in this study. Two variables are generated by using the matched employee-employer dataset: the separation rate and the hire rate. They are defined by the number of separations/hires at time t divided by the number of employees at time $t-1$. Figure 22 shows the two rates for full-time and part-time employment.

The rates are very divergent on both full-time and part-time employment, which can also be seen through the statistics.[48] This phenomenon may be due to the small scale

[48] For the full-time employment, the mean of the separation rate is 0.027 with a S.D. of 0.094 and

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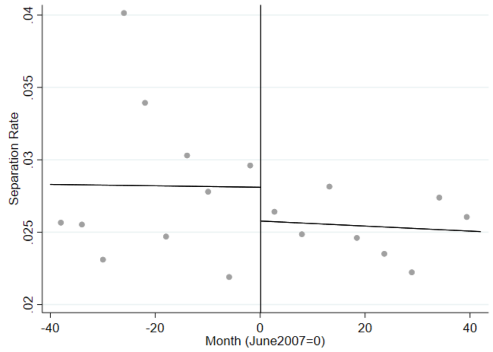
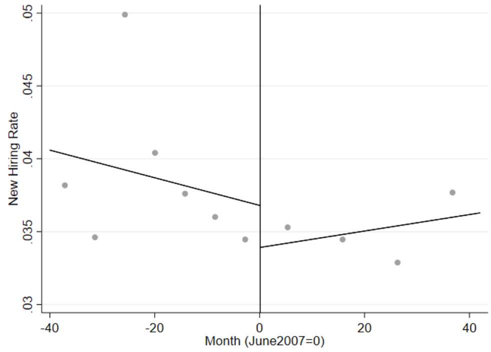
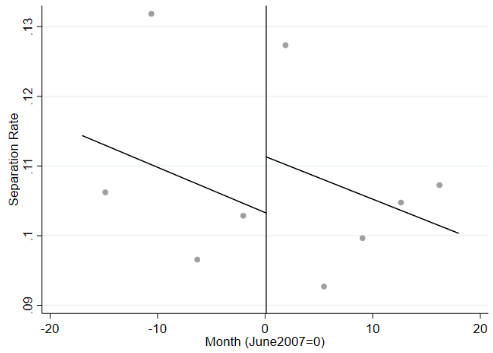


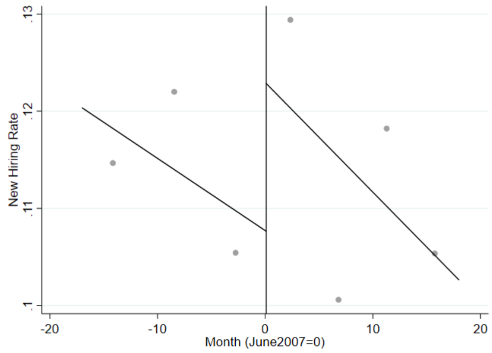
(a) Separation Rate – Full-Time



(b) Hire Rate – full-time



(c) Separation Rate – Part-Time



(d) Hire Rate – Part-Time

Figure 22: Employment Flow in Small Scale Firms

Notes: This figure shows the separation and hire rates for the whole full-time and the whole part-time employment in small scale firms. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. As the information on part-time employees is not available until 2006, the graphs for part-time employment only show the period between Jan. 2006 and Dec. 2008. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. As the value is divergent, the global polynomial fit is with the order of 1 for each side of the cutoff. There is no weighting to all observations.

of these firms. To avoid overfitting, the order of 1 for the global polynomial fit curve. Although it is hard to tell the true result, bins jumping after the cutoff in the graphs of part-time employment seems to not be in line with the findings of previous studies. As seasonal factors may influence the employment flow, they are needed to be ruled out in estimation.

Occupational Injury

the mean of the hire rate is 0.037 with a S.D. of 1.093. For the part-time employment, the mean of the separation rate is 0.099 with a S.D. of 0.24 and the mean of the hire rate is 0.11 with a S.D. of 0.714.

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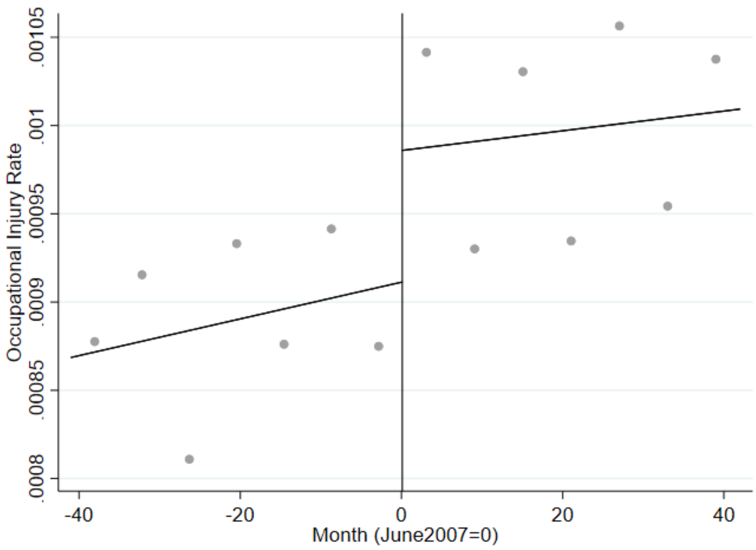

Figure 23: Occupational Injury Rate in Small Scale Firms

Notes: This figure shows the occupational injury rate in small scale firms. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. As the value is divergent, the global polynomial fit, the black curve, is with the order of 1 for each side of the cutoff. There is no weighting to all observations.

As firms may lower other costs to mitigate the wage hike, one possibility is to cut the cost on maintaining safety, see Hradil (2018). This adjustment may also affect the unbound workers without being reflected on their compensation. Figure 23 shows the occupational injury rate in small scale firms, which is calculated by the number of occupational injuries divided by the total number of employees in the firms. I use the order of 1 for the fit curve as the rate is divergent. But even though the rate is fluctuating, it seems to have an obvious upward shift.

2.4.2.2 Data Setting for Individual-level Analysis

Due to the lack of the information on hourly wages and working hours, the individual-level analysis is only on the full-time workers. Nevertheless, as shown in the descriptive statistics of the firm-level dataset, part-time employment only constitutes about 3% of the whole employment. Thus, the main mechanism through which the minimum wage affects the inequality should be the full-time workers.

The challenge to estimating effects on the affected workers is how to classify workers

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through observable wages (Neumark et al., 2004; Neumark, 2018). Using observations of workers with wage information right before the minimum wage change equals to doing estimation conditional on employed workers. It may neglect the entry effect from non-employment into employment and perhaps can be solved by a longer tracking data (Neumark, 2018). Considering that a previous method that uses average wages of individuals to classify workers may mix the direct effect and the indirect effect (Clemens and Wither, 2014, 2017), the employment substitution and the wage spillovers, using workers' latest wage information prior to the policy change may lead to a more correct result.

Through this intuition, two ways of classification are tried firstly. One is based on the information of the last wage prior to the increase; the other uses the whole wage information prior to the increase. That is, by the former method, full-time workers who had wages lower than the new minimum right before the policy change, or those with lower wages months prior to the policy change but were then out of work or turned to part-time jobs are classified as the directly affected workers. By the latter method, only full-time workers who only earned wages lower than the new minimum when they were employed during the whole prior period will be classified as the directly affected workers. Once workers are included into the dataset, they will be set on the balanced panel, in which their missing periods on the record are set as non-employment. This setting helps to generate the employment status of workers out of work. The employment probability is calculated as the ratio of the employees to the whole individuals in the dataset. As the hourly minimum wage has a much higher increase range, the employees include part-time workers. Figure 24 shows the employment probability and the nominal wage of directly affected workers classified by these two methods.

From the graphs, no matter which method is used, the patterns for both employment and wage are obviously different on both sides of the cutoff time. Turning to the method of using the last wage information, Figure 24(b) shows that workers who were having decreasing wages are actually included in the dataset. As workers out of employment after having low wages are included, the employment probability should decrease over time during the prior period. But workers with low wages right before the policy change and having no work earlier are also included. Hence, Figure 24(a) shows the employment probability keeps steady in the prior period. But after the cutoff time, the selection condition does not hold anymore, and the employment probability keeps decreasing over time. On the other

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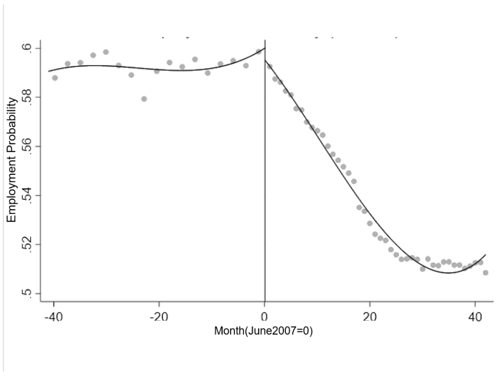
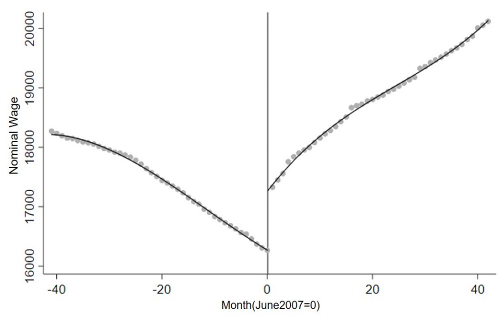
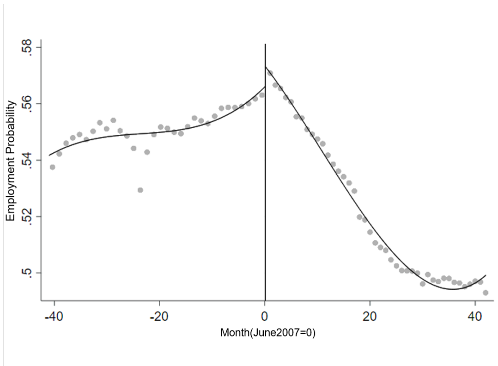


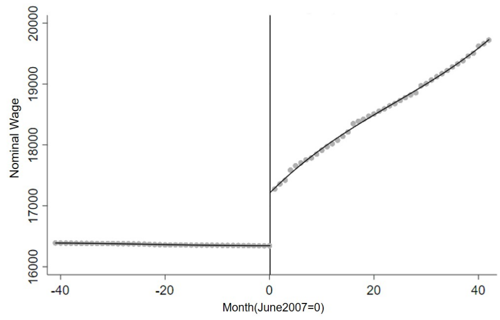
(a) Employment Probability – by Last Information



(b) Nominal Wage – by Last Information



(c) Employment Probability – by Whole Information



(d) Nominal Wage – by Whole Information

Figure 24: Employment and Wage of Directly Affected Classified by Two Methods

Notes: This figure shows the employment probability and nominal wage of directly affected workers classified by two methods. The first one uses the last wage information prior to the minimum wage increase. Full-time workers having wages lower than the new minimum right before the policy change, or months prior to the policy change but were then out of work or turned to part-time jobs are included. The second one uses the whole wage information prior to the minimum wage increase. Only full-time workers earning wages lower than the new minimum when they were employed during the whole prior period are included. Once included, they are set in the balanced panel, in which their missing periods on the record are set as non-employment. The employment probability is the ratio of employment, including part-time jobs, relative to all included workers. The nominal wage does not include the wage for non-employed workers, which should be 0. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 3 for each side of the cutoff. There is no weighting to all observations.

hand, using the rule of the whole information method leads the nominal wage to be very steady prior to the cutoff time (24(d)), and leads the employment to increase during the prior period (24(c)), as workers who had jobs much earlier are more likely to have higher wages. Same as the previous method, when the condition does not hold, the employment

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decreases.

Rather than reflecting the true effects from the minimum wage increase, the changes of variables around the cutoff time highly rely on the selection condition. Therefore, the classification method needs to be less restrictive, which means loosening the wage restriction close to the cutoff time. One possibility is to use the earliest information to classify workers. However, as workers' wages may increase over time, the dataset will mix workers with different wages rather than merely low wages. Hence, it is a tradeoff between the sample selection problems and the sample purity. The only way to provide an unbiased result is to bear the cost of mixing workers firstly and then fix it. This strategy will be shown later in this section.

Individual-level Dataset

To avoid the highly restrictive classification, the directly and also the indirectly affected workers are classified by the wage information at the beginning of the data period, which is January 2004. In other words, the dataset is conditional on workers employed in that month. More specifically, the group of directly affected workers (which is a lower-wage group compared to the other) contains full-time workers who had wages lower than the new minimum; the group of indirectly affected workers (which is a higher-wage group compared to the other) contains full-time workers who had wages between one level to three levels higher than the new minimum in January 2004.[49] Although the substitution or spillover effect may be on workers with much higher wages, the lower levels should be more likely to experience such effects.

A few points for setting the dataset are listed as follows. First, to avoid the influence of schooling, mandatory military service, and retirement, an age restriction is employed in this dataset. The goal in this study is not to focus on teens, and it has already been shown in Figures 13 and 14 that the employment effects on teens may not be more distinctive than others. The age range is ages 22 to 54 for females and ages 24 to 54 for males in January 2004. During the data period, 2004 to 2010, the oldest age in the dataset is 60. Second, the dataset is a balanced panel. Individuals' missing periods on the record are set as non-employment. This helps to evaluate the employment change. Third, the status of employment includes part-time employment. As the percentage increase of the hourly

[49] Insured wage levels lower than the new minimum includes 15,840 and 16,500 NTD. As the level of 17,400 NTD contains workers with actual wages between 16,500 and 17,400 NTD, which may be higher than the new minimum of 17,280 NTD, it is not included into the directly affected worker group. The indirectly affected workers are from levels of 18,300, 19,200, and 20,100 NTD.

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minimum wage is higher than that of the monthly minimum wage, transferring to part-time jobs may be an optimal choice. If this is the case, then the employment effect would be underestimated. Table 13 shows the descriptive statistics of the individual-level dataset. The genders are separated to see if there is any difference between them.

All the observations are individuals times the 84 months, from January 2004 to December 2010. In other words, there are 410,552 male and 515,893 female individuals in the lower-wage group; there are 57,412 male and 86,636 female individuals in the higher-wage group. The number of females is higher in both groups, meaning that workers potential affected by the minimum wage mainly are female.

The age compositions between gender and two groups are very similar. Individuals aged 30 to 49 compose a large part of both groups. The lower-wage group has lower ratios in the Taipei area, higher ratios in the small scale firms, slightly lower employment probabilities, slightly higher part-time employment, and still lower wages compared to the higher-wage group. Between genders, the female workers have slightly higher employment probabilities and higher part-time employment, but lower wages in both groups. Wages of females are about 6%-7% lower than males'. To notice that, information including working location, sector, firm scale, and the wages are all conditional on employment. However, *the real earnings* equals 0 if the observation is under non-employment, and equals *the real wage* when there is employment. Hence, the change of the real earnings would combine minimum wage effects on both employment and wage.

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Table 12: Descriptive Statistics of Firm-level Dataset

	All firms		Small scale firms[1]	
	Mean (1)	SD (2)	Mean (3)	SD (4)
Panel A. Firm charateristics				
Total employment	13.26	117.35	6.28	14.69
Service sector	0.64	0.48	0.65	0.48
Location *(Taipei)*	0.40	0.49	0.40	0.49
Panel B. Employment composition				
Full-time *(FT)*	12.82	110.40	6.12	13.96
FT (*wage ≤ new MW*)[2]	3.01	13.45	2.21	4.42
FT (*wage > new MW*)	9.81	105.30	3.91	12.69
FT (*new MW +1 wage level*)[3]	0.40	5.30	0.24	1.31
FT (*new MW +1 to +3 wage levels*)[4]	1.06	9.17	0.62	2.35
Part-time *(PT)*	0.43	24.23	0.16	2.59
Panel C. Nominal wage				
Avg. wage *(FT)*	26,933	7,543	26,790	7,536
Avg. wage *(PT)*	13,091	4,239	12,649	3,570
Total wage	383,705	4,113,022	161,254	454,963
Total wage *(FT)*	377,828	4,035,899	159,299	448,033
Total wage *(PT)*	5,877	337,245	1,955	34,894
Panel D. Real wage				
Avg. wage *(FT)*	29,673	8,278	29,513	8,269
Avg. wage *(PT)*	14,263	4,632	13,755	3,870
Total wage	422,921	4,524,666	177,471	492,969
Total wage *(FT)*	416,493	4,441,134	175,347	485,578
Total wage *(PT)*	6,429	367,185	2,124	37,606
Panel E. Others				
Separation rate *(FT)*	0.027	0.094	0.027	0.095
Separation rate *(PT)*	0.101	0.239	0.099	0.240
Hire rate *(FT)*	0.037	1.067	0.037	1.093
Hire rate *(PT)*	0.120	0.908	0.110	0.714
Occu. injury rate	0.00094	0.02589	0.00095	0.02653
Occu. injury rate *(restricted)*[5]	0.00141	0.03081	0.00145	0.03203
Obs.	34,676,834		32,860,719	

Notes:

[1] Firms with less than 30 employees.

[2] As the new minimum is not a wage level prior to the policy change, here using the level of 17,400 NTD to represent the new MW. This level is the closest level to the new minimum.

[3] 18,300 NTD

[4] 18,300-20,100 NTD, which is 6% to 16% more than the new minimum.

[5] Restricted to the three industries (manufacturing, construction, water supply and remediation activities) having the highest occupational injury rates before the treatment.

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Table 13: Descriptive Statistics of Individual-level Dataset

	Group of directly affected				Group of indirectly affected			
	Male		Female		Male		Female	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Individual backgrounds								
Age between 22-29	0.335	0.472	0.320	0.467	0.335	0.472	0.352	0.472
Age between 30-39	0.338	0.473	0.305	0.461	0.327	0.469	0.295	0.456
Age between 40-49	0.253	0.435	0.291	0.454	0.257	0.437	0.272	0.445
Age between 50-60	0.074	0.262	0.084	0.277	0.082	0.274	0.081	0.273
Work in Taipei	0.332	0.471	0.356	0.479	0.391	0.488	0.405	0.491
Work in service sector	0.528	0.499	0.572	0.495	0.521	0.500	0.479	0.500
Firm with <30 employees	0.756	0.429	0.706	0.456	0.573	0.495	0.456	0.498
Firm with 30-99 employees	0.143	0.350	0.155	0.362	0.176	0.381	0.207	0.405
Firm with 100-499 employees	0.068	0.252	0.089	0.285	0.141	0.348	0.186	0.392
Firm with 500-999 employees	0.012	0.108	0.016	0.124	0.044	0.205	0.057	0.231
Firm with 1000+ employees	0.021	0.145	0.034	0.182	0.066	0.248	0.091	0.288
Panel B. Labor market outcome								
Employment probability	0.719	0.449	0.737	0.440	0.742	0.438	0.765	0.424
Part-time employment	0.0051	0.0716	0.0098	0.0986	0.0041	0.0640	0.0070	0.0836
Nominal wage	22,062	8,250	20,336	6,663	23,652	7,923	22,074	6,326
Real wage	24,358	8,830	22,467	7,102	26,118	8,470	24,389	6,739
Real earning	17,521	13,262	16,551	11,622	19,370	13,562	18,649	11,907
Obs.	34,444,368		43,335,012		4,822,608		7,277,424	

Notes:
The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. All the male individuals were aged 24 to 60; all the female individuals were aged 22 to 60 during the data period, from 2004 to 2010. The dataset is a balanced panel with observations labeled as non-employment when they were out of the insurance record. The employment probability is reflecting the dummy variable of employment, which includes part-time employment. Working information including location, sector, firm scale, the nominal wage, and the real wage are conditional on employment. The real wage is deflated by monthly CPI. The real earning equal 0 if there is non-employment

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Compared to the general RD design, the density test and the randomization test are not available here. As the dataset is a balanced panel, the number of the observation stays the same during the time period. On the other hand, the personnel working characteristics are conditional on employment, which may also be the outcome resulting from the minimum wage increase. Hence, how to classify groups to form the dataset plays a key role. The following shows the variables of interest in RD form from the two groups.

Individual Employment, Wage, and Earnings

Compared to most of the previous studies providing evidence of employment effects on specific demographic groups (Belman et al., 2015; Neumark, 2018), separating the groups of directly and indirectly affected workers can help to clarify the true effects on both sides. Figure 25 shows the employment probability of the two groups. Different from the graphs of more restrictive settings, the employment probability shown here is more smooth with no distinctive patterns on both sides of the cutoff time. As those groups are classified by the information of January 2004, the employment probability equals to 1 in the beginning of the data period and keeps decreasing afterwards. Around the cutoff time, there seems to be no obvious change and the graphs are similar between the two groups and also between the genders. But of course, the specific month and year effects and the large scale of the bandwidth may influence what is shown here.

The individual nominal wages of the two groups are shown in Figure 26. Because the wages at the beginning of the time period are used to classify groups, their nominal wages are lower or slightly higher than the new minimum wage of 17,280 NTD. As the nominal wage is conditional on employment, so the wage is increasing over time. As expected, the group of the directly affected workers has an obvious increase of their nominal wage (26(a) and 26(b)). Comparatively, the pattern for the group of the indirectly affected workers is unclear (26(c) and 26(d)). But if the real value of the wage is considered, as shown in Figure 27, the lower-wage group seems to still have an increase (27(a) and 27(b)), but the higher-wage group may have had their real wages decrease (27(c) and 27(d)) rather than be stable or increase as the hypothesis of equity effect expects.

As the nominal and real wages are conditional on employment, only if there is no employment effect can they be used to infer the effect on the two groups. Hence, the real earnings, combining the wage and employment effects, is the key variable to understand the effect on the inequality. Figure 28 shows the real earnings of the two groups. The

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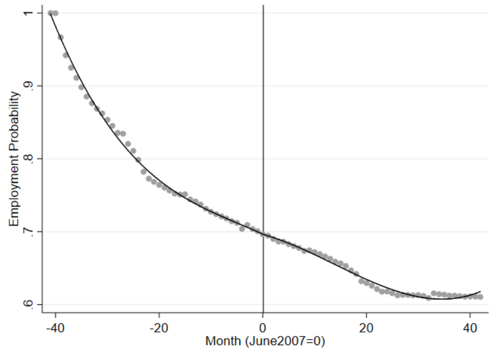
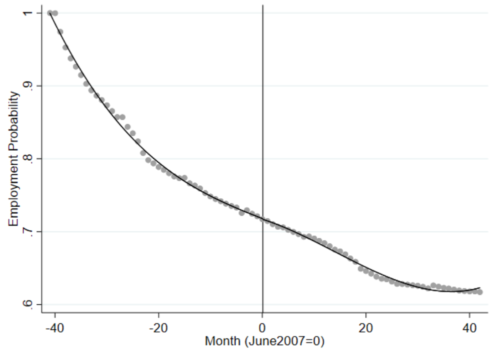
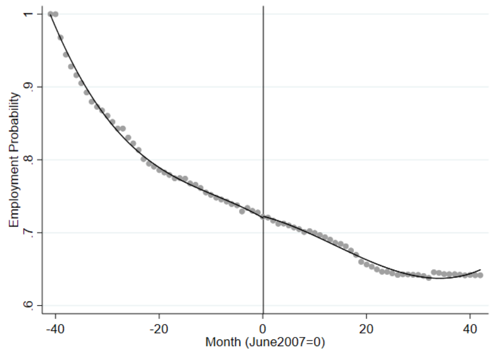


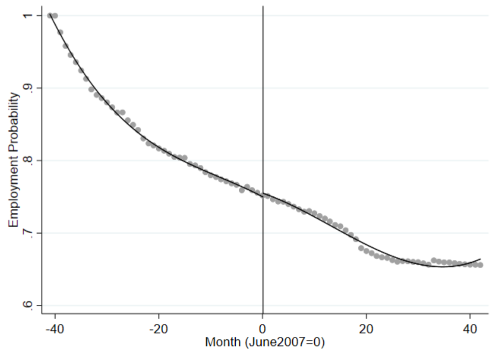
(a) Group of Directly Affected − Male



(b) Group of Directly Affected − Female



(c) Group of Indirectly Affected − Male



(d) Group of Indirectly Affected − Female

Figure 25: Individual Employment Probability

Notes: This figure shows the individual employment probabilities from groups of the directly and indirectly affected workers. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. Non-employment is labeled if observation is not in the insurance record. As a result of this setting, the employment probability equals 1 in Jan. 2004. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

lower-wage group still seems to have an increase after the policy change (28(a) and 28(b)); the change on the higher-wage group is not clear, although there seems to be no change on the female side (28(d)) and a decrease on the male side (28(c)).

Difference between Groups

The interest in this study is to see how the inequality is affected by the minimum wage increase. According to the hypothesis of the substitution effect, the bound workers may

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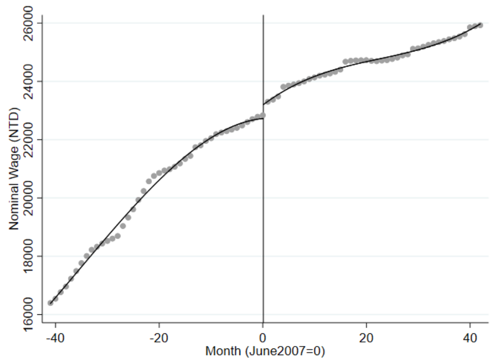
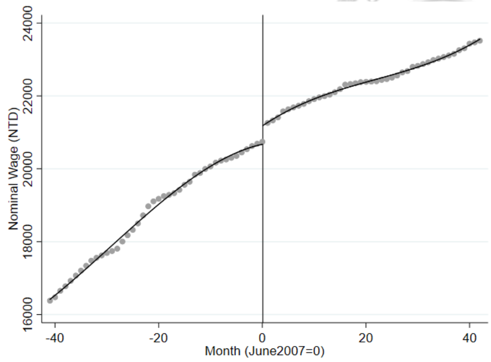
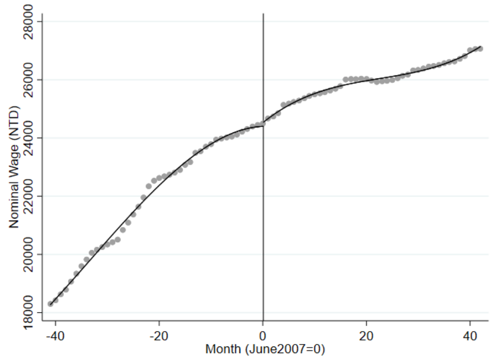


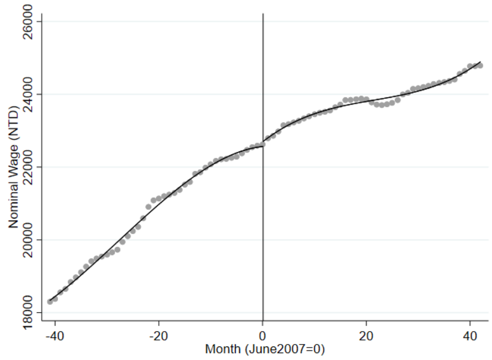
(a) Group of Directly Affected − Male



(b) Group of Directly Affected − Female



(c) Group of Indirectly Affected − Male



(d) Group of Indirectly Affected − Female

Figure 26: Individual Nominal Wage

Notes: This figure shows the individual nominal wage from groups of the directly and indirectly affected workers. The nominal wage is conditional on employment. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. As a result of this setting, the nominal wage of the former group in Jan. 2004 is lower than the new minimum wage, while it is slightly higher than the new minimum in the latter group. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

suffer due to the increase; on the contrary, the unbound workers may gain benefits from more employment and may even get higher wages. The inequality may be raised then. On the other hand, the hypothesis of the equity effect expects that the unbound workers will have their wages raised when the lowest-workers have higher wages, thus the inequality between the lower tail in the wage distribution may have no distinctive change. To briefly

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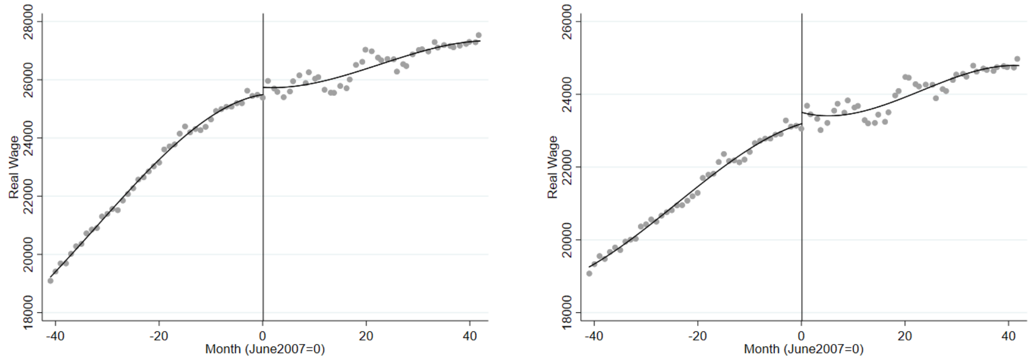
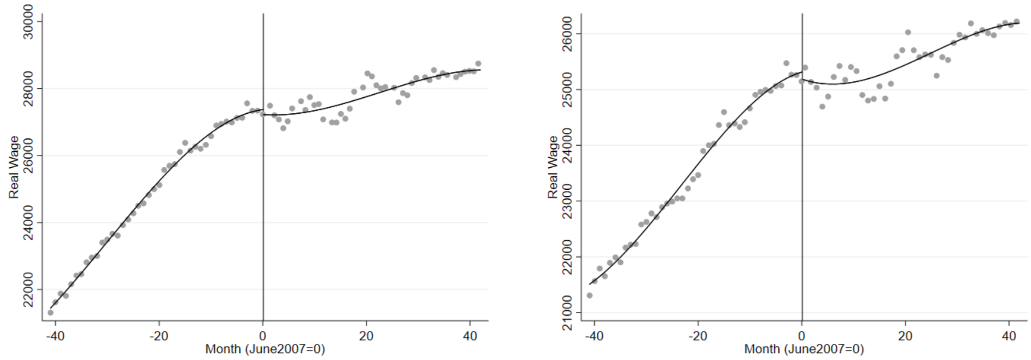


(a) Group of Directly Affected − Male (b) Group of Directly Affected − Female



(c) Group of Indirectly Affected − Male (d) Group of Indirectly Affected − Female

Figure 27: Individual Real Wage

Notes: This figure shows the individual real wage from groups of the directly and indirectly affected workers. The real wage is conditional on employment and be deflated by the monthly CPI. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

see if those expectations can be shown in the data, the following figures show the difference of employment, wages, and earnings between the two groups of workers.

Figure 29 shows the difference of employment probability between the two groups. To reflect the concept of inequality, the difference is calculated by the mean of the indirectly affected at time t minus the value of the individuals of the directly affected group. As individuals were all employed in January 2004, the difference is 0 at that time. The

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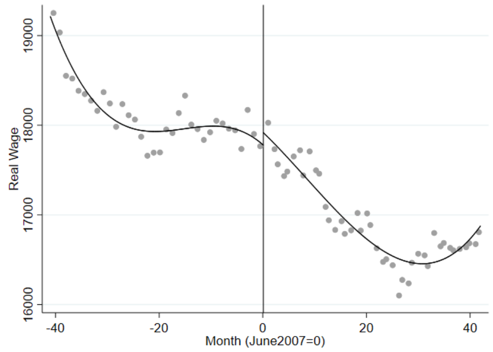
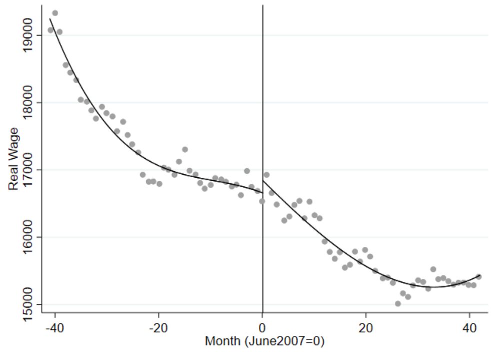
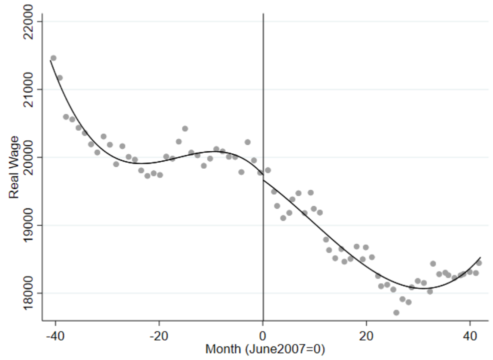


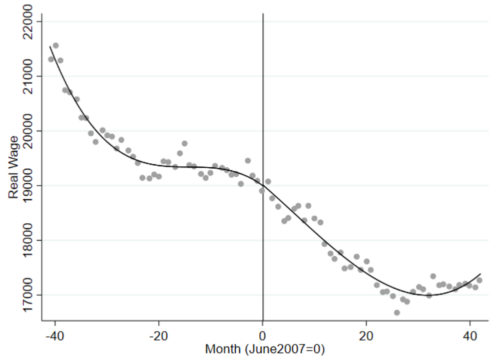
(a) Group of Directly Affected − Male



(b) Group of Directly Affected − Female



(c) Group of Indirectly Affected − Male



(d) Group of Indirectly Affected − Female

Figure 28: Individual Real Earnings

Notes: This figure shows the individual real earnings from groups of the directly and indirectly affected workers. The real earnings equal 0 if observation is out of work, and equal the real wage otherwise. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

increasing difference shows that the lower-wage workers are more likely to be out of work. Around the cutoff time, there seems to be no obvious change on the male workers (29(a)), but the difference between the female higher- and lower-wage workers seems to be wider (29(b)). A quick intuitive guess is that the lower-wage females suffered from disemployment because of the minimum wage increase. But if we go back to see Figures 25(b) and 25(d), it is more possible that the widened difference is due to the increased employment of the

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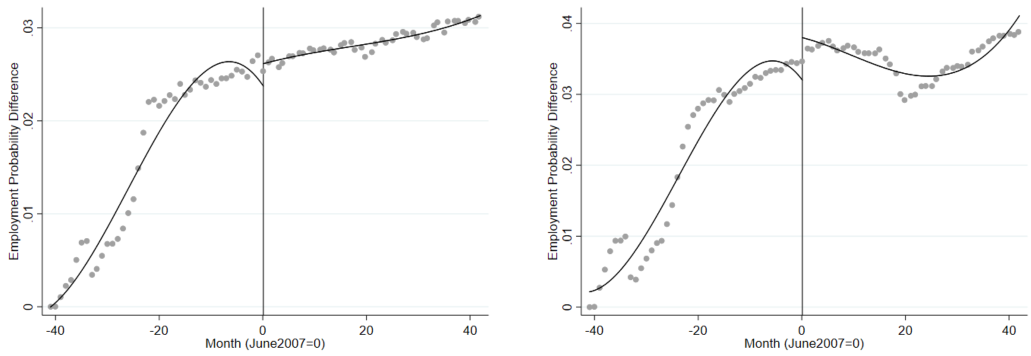
female higher-wage workers instead. However, if we assume that there is no effect on the higher-wage workers, then the result estimated from an approach of difference in difference will tell us that the female workers suffer because of the increase.



(a) Male

(b) Female

Figure 29: Difference of Employment Probability between Two Groups

Notes: This figure shows the difference of employment probability between the groups of directly affected and the indirectly affected workers. The difference equals the mean of the indirectly affected at time t minus the value of the individuals of the directly affected group. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. By this setting, all the individuals were employed in Jan. 2004, so the difference of the employment probability is 0 at that time. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

Figure 30 shows the differences between the nominal and real wages between the two groups. One of the interesting parts is that the wage gap for the males has a decreasing pattern on both sides of the cutoff time, but it seems to be steady for the females prior to the policy change. The minimum wage increase leads to distinctive reductions on wage gaps among different groups and different genders. This is not close to expectation of he equity effect, unless we assume that the wage spillovers only happen on workers with previous wages 16% higher than the new minimum.[50]

The difference of the real earnings between two groups is shown in Figure 31. When

[50] The group of the indirectly affected workers is classified by the wage levels of 18,300 to 20,100 NTD, which is about 6% to 16% higher than the new minimum wage of 17,280 NTD.

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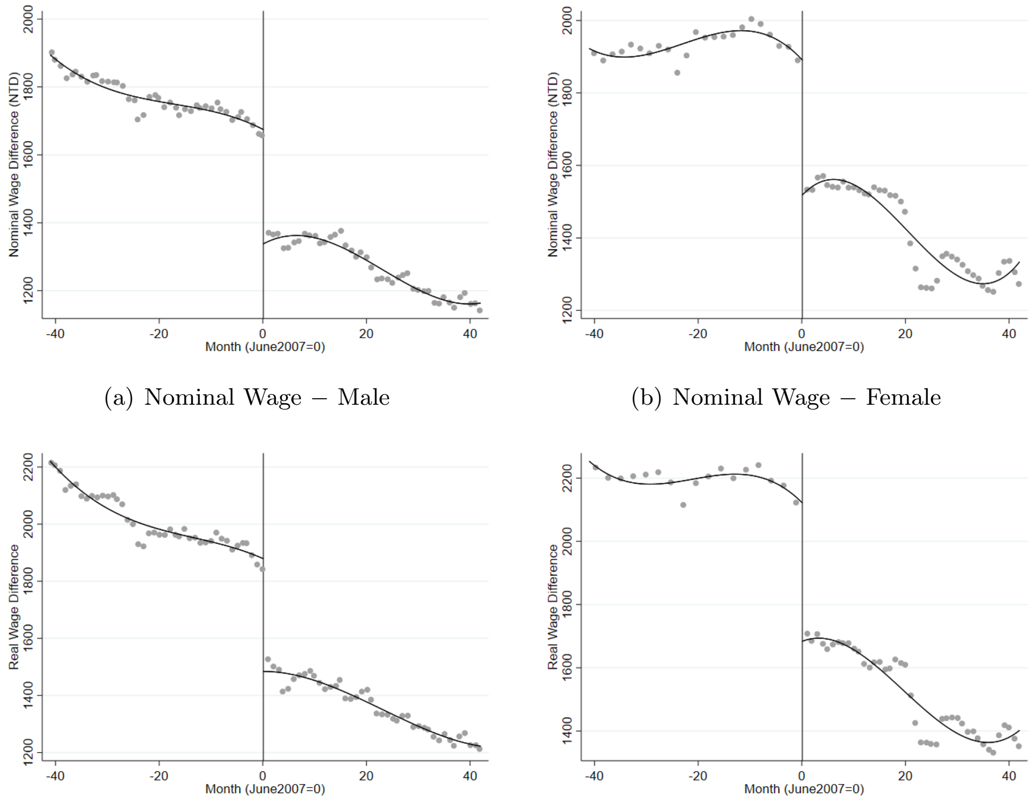

(a) Nominal Wage – Male

(b) Nominal Wage – Female

(c) Real Wage – Male

(d) Real Wage – Female

Figure 30: Difference of Wages between Two Groups

Notes: This figure shows the differences of the nominal and real wages between the groups of directly affected and the indirectly affected workers. The difference equals the mean of the indirectly affected at time t minus the value of the individuals of the directly affected group. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. By this setting, all the individuals were employed in Jan. 2004, so the difference of the employment probability is 0 at that time. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

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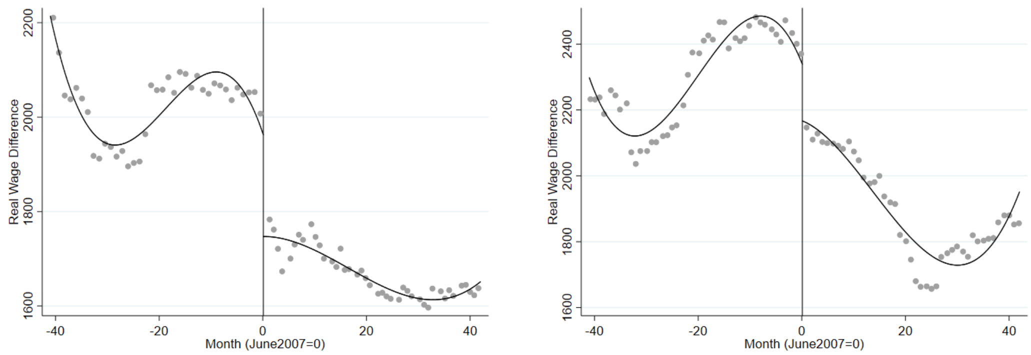
combining the effects on employment and wages, both genders seem to have reductions in their earnings gaps after the minimum wage increase. This phenomenon is not in line with the hypotheses of the substitution effect and the equity effect, which state there should be a wider wage gap and a stable wage gap, respectively.



(a) Real Earnings − Male

(b) Real Earnings − Female

Figure 31: Difference of Real Earnings between Two Groups

Notes: This figure shows the difference of the real earnings between the groups of directly affected and the indirectly affected workers. The difference equals the mean of the indirectly affected at time t minus the value of the individuals of the directly affected group. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. By this setting, all the individuals were employed in Jan. 2004, so the difference of the employment probability is 0 at that time. The horizontal axis shows the number of months from June 2007, which is the latest month before the minimum wage increase. Hence, June 2007 is set as 0 on the horizontal axis. The data period is from Jan. 2004 to Dec. 2010. The number of bins is selected by the data-driven procedure of specifying the mimicking variance evenly-spaced method using spacing estimators. The black curve is the global polynomial fit with the order of 4 for each side of the cutoff. There is no weighting to all observations.

2.4.2.3 Adjustment for Individual-level Analysis

The drawback of the classification using the wage information of the earliest information in the data period is mixing workers with different wages before the policy change. Figure 32 shows the insured wage distribution of the two groups in June 2007, right before the minimum wage increase. The blue bar is for the lower-wage group, and the red bar is for the other. From the graphs, we can tell that even after 42 months, workers having bound wages in January 2004 were still concentrated in the bound wage levels in June 2007. More specifically, 36% of the male workers and 41% of the females in the lower-wage group were

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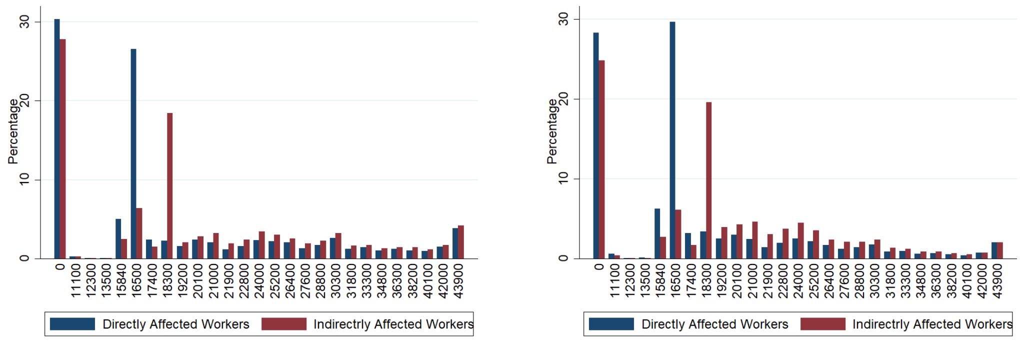
still bound by the new minimum in June 2007. Correspondingly, 6% and 9% of them were raised to one to three levels higher than the new minimum. On the other hand, in the higher-wage group 24% of males and 29% of females were in the range of one to three levels higher than the new minimum. The proportion that fell into the bound levels is 11% for both males and females. However, both groups have high proportions of non-employment, which is valued as 0 in the figure.



(a) Male (b) Female

Figure 32: Insured Wage Distribution of Two Groups in June 2007

Notes: This figure shows the insured wage distribution of the groups of directly affected and the indirectly affected workers in June 2007. The wage equals to 0 for non-employment. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004.

Regardless of the high proportion of non-employment, as both groups still concentrated of their previous levels, the simple method from the literature is that the results are more reflective of effects on the directly and the indirectly affected workers rather than others. However, the ambiguity is exactly what this study wants to address. In addition, the high proportion of non-employment also makes the results more ambiguous. To address this problem, two assumptions for estimation are listed as follows.

One assumption is that effects of the minimum wage increase are only a direct effect and an indirect effect, which are on workers with wages lower than the new minimum and wages between one to three levels higher than the new minimum. The other assumption is that the unemployment rate in the whole labor market is applied to workers on all wage levels. Although the second assumption explains parts of the non-employment, there are still unexplained parts left. Considering that the wage may increase or decrease over time

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due to some unobservable factors, like the setting in the Mincer equation. Just as some workers have their wages raised to higher levels, correspondingly, some workers have their wages decreased due to some reasons. But those workers with decreased wages will be truncated by the minimum wage and be labeled as non-employment.[51] As their potential wages are already lower than the existing minimum wage, the minimum wage increase would not have an effect on them. By these two assumptions, the results of the two groups can be derived as the combination of the actual direct and indirect effects. In the following, the adjustment for the individual outcome and the difference are described separately.

First, the individual outcome when the two groups are classified by the earliest information could be described as Eqs. (14) and (15)

$$y^l = r_L^l y_L + r_H^l y_H + O^l \tag{14}$$

$$y^h = r_L^h y_L + r_H^h y_H + O^h \tag{15}$$

y^l and y^h are as the setting before, representing individual outcomes from the group of directly affected workers (group l) and the group of directly affected workers (group h), respectively. As the groups mix workers with different wages, these two outcomes actually are combinations of outcomes from different kinds of workers. y_L is the outcome of workers who are *actually* bound by the new minimum wage before the policy change (worker L); y_H is the outcome of workers who are *actually* one to three levels higher than the new minimum before the policy change (worker H). r_L^l is the ratio of the *actually* bound workers in the lower-wage group, and r_H^l is the ratio of workers *actually* having slightly higher wages than the new minimum in the lower-wage group. Correspondingly, in a simplified way, r_L^h is the ratio of the L workers in the h group, and r_H^h is the ratio of H workers in the h group.[52] O^l and O^h are the weighted average outcome of other kinds of workers in groups l and h. The ex post outcome could be shown as Eqs. (16) and (17),

[51] The context here seems to acknowledge the disemployment effect caused by the minimum wage, but actually it is different between the effect of having a minimum wage and a minimum wage increase. An example for the context is a minimum wage worker losing his working capability due to an accident or a family event are unable to maintain his original job, hence he is out of employment or transfers to other informal or unpaid work. It is obvious not like the situation where the employer needs to raise the wage of that minimum wage worker, which is the case that this study is focusing on.

[52] Ratios of workers unemployed but belonging to such wage levels are included in r.

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as Δ means the change after the policy change.

$$y^l + \Delta y^l = r^l_L(y_L + \Delta y_L) + r^l_H(y_H + \Delta y_H) + (O^l + \Delta O^l) \tag{16}$$

$$y^h + \Delta y^h = r^h_L(y_L + \Delta y_L) + r^h_H(y_H + \Delta y_H) + (O^h + \Delta O^h) \tag{17}$$

Based on the unique trait of the RD design of this study, Δy^l and Δy^h are exactly the estimates β^l and β^h shown in Eqs. (8) and (9). Now based on the first assumption, minimum wage increases only have effects on workers L and H, hence, both ΔO^l and ΔO^h are 0. Then the original estimates can be shown as in Eqs. (18) and (19).

$$\beta^l = r^l_L\beta_L + r^l_H\beta_H \tag{18}$$

$$\beta^h = r^h_L\beta_L + r^h_H\beta_H \tag{19}$$

β_L and β_H represent the effects on workers L and H, the same as Δy_L and Δy_H. Therefore, the actual direct and indirect effects can be derived by the estimated results, β^l and β^h, and the ratios, r^l_L, r^l_H, r^h_L, and r^h_H. One thing to notice is that to derive the actual effects, the change of the outcome variables need to be the change of values rather than the percentage change. If we replace Eqs. (16) and (17) with the log value of the outcome, then effects on different parts of workers cannot be easily separated. That is the reason why this study estimates the value changes rather than percentage changes. In addition, based on Eqs. (18) and (19), the variance of the change of the groups l and h can be shown as in Eqs. (20) and (21).

$$V(\beta^l) = (r^l_L)^2V(\beta_L) + (r^l_H)^2V(\beta_H) + 2r^l_Lr^l_H cov(\beta_L, \beta_H) \tag{20}$$

$$V(\beta^h) = (r^h_L)^2V(\beta_L) + (r^h_H)^2V(\beta_H) + 2r^h_Lr^h_H cov(\beta_L, \beta_H) \tag{21}$$

Hence, the variance of actual effects can be derived by the variance of estimated results, $V(\beta^l)$ and $V(\beta^h)$, the ratios, r^l_L, r^l_H, r^h_L, and r^h_H, and $cov(\beta_L, \beta_H)$. As β_L and β_H are derived by Eqs. (18) and (19), the covariance between the two effects cannot be identified. However, considering that these two kinds of workers have similar wages and hence are both low-skilled workers compared to the whole distribution of workers, the minimum wage effects on them may vary in the same directions. For example, a recession in a specific

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industry may amplify the disemployment effect and mitigate the substitution effect on those workers. If the direct wage effect is over the mechanical adjustment, then the direct wage effect and also the wage spillovers would be mitigated by a recession. If the direct wage effect is just reflecting the mechanical adjustment, then there would be no correlation with the indirect wage effect. Therefore, the $cov(\beta_L, \beta_H)$ should be 0 or positive. That is, the variance of the actual effects would be overestimated when setting the covariance as 0. If the actual estimates are still significant under this setting, they would also be significant under the true variance.

Except for the individual outcome, the model for the difference between two groups, Eq. (13), needs to be rewritten as Eq. (22).

$$\begin{gathered}\overline{y_t^h} - y_{it}^l = \Delta\gamma + \Delta\beta D_{it} + \Delta\theta Time_t + \Delta\eta(D_{it} \cdot Time_t) + \Delta\epsilon_{it} \\ \text{where} \quad \Delta\beta = (r_H^h - r_H^l)\beta_H - (r_L^l - r_L^h)\beta_L\end{gathered} \tag{22}$$

It means that the estimated result is not exactly the effect on the difference after the minimum wage increase, which should be the difference of two actual effects, $\beta_H - \beta_L$. The necessary condition to derive the actual effect on the difference is $(r_H^h - r_H^l) = (r_L^l - r_L^h)$. Unfortunately, the ratios in the dataset cannot support this condition.[53] Nevertheless, as the key question that this study want to solve is whether the spillovers exist, we can estimate it through the difference of nominal wages. That is, if the direct effect on the nominal wage is just reflecting the mechanical adjustment, we can know the β_L for the nominal wage, hence we can estimate whether the β_H for the nominal wage is positive or not.

2.5 Regression Discontinuity Estimate

To thoroughly know the minimum effects on the inequality, the firm-level and individual-level estimates are shown in sequence. First, providing the firm-level estimates of the changes on the employment stock and the wage payment help to see if the substitution and the equity effects exist. The former one expects the opposite employment effects on lower- and higher-wage workers; the latter expects a wage increase for the higher-wage workers. Second, with the individual-level data, I can obtain the separated estimates of

[53] In this dataset, the difference of H worker ratio between groups h and l is 18% for the males and 20% for the females; the difference of L worker ratio between groups l and h is 25% for the males and 30% for the females.

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the employment and wage effects on the directly and indirectly affected workers. Third, I can look directly into the difference between the lower- and higher-wage workers. Fourth, the firm-level data helps me to see the employment flow that may reflect the reason behind the results; I can use individual transitions as a supplement. Fifth, I provide the estimate of the other channel of a spillover effect, the safety effect.

All the estimates are based on the local variation rather than the global variation. Considering that different bandwidths may affect the estimated results, the results here are estimated by two optimal bandwidths, which are based on the minimization of mean square error (MSE) and the minimization of coverage error rate (CER). The bandwidths vary on different models, but in general, the MSE bandwidth is about 10 months before and after the cutoff time, which is higher than the CER bandwidth, which is about 5 months before and after the cutoff time. In addition, as the traditional estimation of minimizing the error value widens the choice of bandwidth and hence leads to bias, bias−corrected estimates are needed. However, this can cause the variance of the estimated value to become enlarged. Hence, this study uses the robust estimation provided by Calonico et al. (2014), which is bias−corrected and has a more stable variance.

2.5.1 Estimate of Employment Stock and Wage Payment Effects

Previous studies about the minimum wage effects on the wage distribution all found wage spillovers, although some are huge and some are moderate (Dinardo et al., 1996; Lee, 1999; Autor et al., 2016; Cengiz et al., 2018). Combined with the elusive results of the employment effect, studies tend to infer that these spillovers are a result of the equity effect (Giupponi and Machin, 2018; Dube et al., 2019) rather than the substitution effect. Based on the hypothesis of the equity effect, higher-wage workers have their wages raised due to employers wanting to maintain the wage structure in order to keep their workers' effort (Gramlich, 1976; Grossman, 1983; Akerlof and Yellen, 1990; Katz and Krueger, 1992). Hence, this effect should be easily found in the firm level. However, until today, there has been sparse evidence from adequate firm-level data to support the spillovers. Although Dube et al. (2019) recently found wage spillovers in a large US retailer, more evidence is needed in a broader scale. As the two competing hypotheses lead to different results on firms' employment and wage payments, I try to provide evidence through firm-level analysis by using a employee-employer matched administrative dataset.

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2.5.1.1 Estimates for Employment Stock

The effect on the firms' employment stock is shown in Table 14. As the small scale firms have a higher proportion of the bound workers, a subsample of them is formed to rule out the potentially moderate effect on the larger scale firms. Columns (1) and (3) are estimates following the model shown by Eq. (6) without controlling the specific month and year effects; the column (2) and (4) are estimates following the models shown by Eq. (7), which controls the specific month and year effects. All the estimates reflect the change on the dependent variables' value rather than percentage changes.

First, when analyzing all firms, no significant effect shows up when the specific year and month effects are not controlled. After controlling those effects, the number of full-time jobs bound by the new minimum wage has a significant decrease of 2.5% compared to the mean value before the policy change. But the estimate under the CER bandwidth is positive although not significant. As the CER bandwidth is shorter than the MER bandwidth, it means that those jobs were not decreased but even sightly increased in the short run and then decreased in a longer period. However, since the total full-time employment under the MER bandwidth has no significant change, the decrease may just mean some lowest-wage jobs move to higher-wage levels.

Second, in the analysis of small scale firms, effects are more clear. Even without controlling the specific year and month effects, we can find a more significant increase on the full-time binding-wage jobs in the short run, although it is just about 1.1% compared to the mean value before the policy change. Besides, the part-time jobs significantly increase about 11.3% under both bandwidths. After controlling specific year and month effects, under the shorter bandwidth (CER), the full-time binding-wage jobs still increase by 1.1%, and the jobs one level higher than the new minimum also increase by 2.1%, although there is no significant change on the total full-time employment. Under the longer bandwidth (MER), the negative effect on the binding-wage jobs (-1.1%) shows again, but now with an increase on the jobs one to three levels higher than the new minimum (1.9%) and also with an increase in the total full-time employment (1.7%). On the other hand, the positive effect on the part-time employment stays significant when controlling the specific time effects, and even higher under the longer bandwidth (24.1%).

Based on the results, a few things are confirmed by the estimates for small scale firms,

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which are more exposed to the minimum wage effects. One is that there is no decrease on the lowest-wage jobs in the short run; these may even be a significantly slight increase, which is in line with many related works in the literature. Another is that jobs with wages slightly higher than the new minimum have a significantly slight increase, which is in line with the finding of Neumark et al. (2004). The other is that the part-time employment increases significantly, which is in line with the finding of Giuliano (2013). It is unconfirmed whether the significant decrease in binding-wage jobs under the longer bandwidth reflects layoffs or movements to higher-wage levels.

Recalling the two hypotheses of the substitution and the equity effects, the change on the employment is not in line with the expectation of the equity effect, which should lead to a stable employment. The short term increase on the lowest-paid jobs is not in line with the substitution effects either, but the longer period estimates may reflect an employment substitution away from the lowest-paid positions to higher-wage positions. However, the total full-time employment increase in a longer period cannot be explained by the substitution effect. Furthermore, the huge increase in the part-time jobs is obviously away from the expectation of substitution effect, especially since the hourly minimum wage was hugely increased by 44%. Although no hourly information can be used to separate part-time workers, we know that the proportion of unbound part-time workers is only 44.3%. If all the employment increases come from those unbound workers, then their employment needs to be increased by at least 54.4%. Hence, a more possible explanation to these findings is that the binding-wage jobs do increase in number, at least the part-time ones do.

In addition, the change of the employment also challenges studies which focused on the wage distribution (Dinardo et al., 1996; Lee, 1999; Autor et al., 2016; Cengiz et al., 2018), in which they need to neglect the change on employment, either the bound one or unbound one.

2.5.1.2 Estimates for Average Wage

The estimated minimum wage effect on the firms' average wage payment is shown in Table 15. Previous firm-level studies usually used firms' wage payment to classify firms or to form the independent variable describing the minimum wage influence (Card and Krueger, 1994; Hirsch et al., 2015), thus they cannot estimate the actual wage effects on the firms.

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Table 14: RD Estimators of Effects on Employment Stock in Firms

	All firms		Small scale firms	
	(1)	(2)	(3)	(4)
Panel A. CER optimal bandwidth estimator				
Full-Time	-0.038	-0.063	0.015	0.024
	(0.172)	(0.173)	(0.029)	(0.029)
Full-time (*wage* $\leq$ *new MW*)	0.030	0.043*	0.024**	0.023**
	(0.024)	(0.024)	(0.010)	(0.010)
Full-time (*1 level higher*)	0.013*	0.013*	0.005*	0.005**
	(0.0077)	(0.0077)	(0.0027)	(0.0028)
Full-time (*1 to 3 levels higher*)	0.019	0.020	0.0090*	0.0075
	(0.013)	(0.013)	(0.0051)	(0.0051)
Part-time	0.059	0.062	0.016***	0.017***
	(0.068)	(0.068)	(0.0045)	(0.0045)
Panel B. MER optimal bandwidth estimator				
Full-Time	-0.024	-0.026	0.002	0.106***
	(0.173)	(0.172)	(0.030)	(0.030)
Full-time (*wage* $\leq$ *new MW*)	0.006	-0.072***	0.005	-0.024**
	(0.024)	(0.024)	(0.011)	(0.011)
Full-time (*1 level higher*)	0.013*	0.007	0.003	0.004
	(0.0078)	(0.008)	(0.003)	(0.003)
Full-time (*1 to 3 levels higher*)	0.022	0.010	0.0088*	0.012**
	(0.0144)	(0.014)	(0.0052)	(0.0052)
Part-time	0.051	0.077	0.016***	0.034***
	(0.072)	(0.071)	(0.0046)	(0.0046)
Control:				
Year & month effects	No	Yes	No	Yes
Mean value before the policy change				
Full-time	12.684		6.073	
Full-time (*wage* $\leq$ *new MW*)	2.918		2.149	
Full-time (*1 level higher*)	0.376		0.237	
Full-time (*1 to 3 levels higher*)	1.013		0.624	
Part-time	0.432		0.141	

Notes: The table reports RD treatment effect estimators. Small scale firms are firms with less than 30 employees. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. All the estimators are related to the value of dependent variables rather than percentage change. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. full-time with one level higher means full-time employment on the insured wage 18,300 NTD, and one to three levels higher include 18,300, 19,200, and 20,100 NTD. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

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It is one of the contributions of this study. As there is no weighting, the estimates between total firms and small scale firms are closer and the main difference between the large and small firms is in the part-time workers. Considering that large scale firms may give higher hourly wages to their part-time workers, I also focus on estimates for the small scale firms.

For the nominal wage, no matter whether full-time or part-time, what the bandwidth is, and whatever the specific time effects are controlled or not, the average nominal wages have significantly increased. As the nominal wage is conditional on employment, the results are not so surprising. However, the effect on the average real wage is striking. To discuss the effects on the real wage, we need to rule out the influence from the fluctuation of the CPI. As shown in Figure 20, the increase of the CPI may have an adverse result under a shorter RD bandwidth. Hence, the estimated results of the full-time real wage under the CER bandwidth (on the row with grey color) are obviously higher than the nominal one, even though we know that the increased CPI actually should mitigate the wage increase. Although the estimates on the real wage of the part-time employment under the CER are not higher than the nominal ones, they could be lower if there was no fluctuation of the CPI. Therefore, estimates under the MER bandwidth should be more reliable.

Under the MER bandwidth, all the estimates for the real wage are negative. After controlling the specific year and month effects, the average wages for the full-time employment and the part-time employment are decreased, respectively, by 1.0% and by 0.6%, although the latter is not so significant. As the nominal insured wage of full-time bound workers is at least increased by 4.7%, which is larger than the inflation 10 months after the cutoff time even without controlling the time trend, the decrease of the real wage should come from the unbound workers. If the inference is true, it means that rather than spillovers, the workers who had wages just above the new minimum before the policy change had their real wages decline, which is in line with the finding from Neumark et al. (2004) but opposite to the very specific finding on a large US retailer from Dube et al. (2019) and hence to the equity effect.

2.5.1.3 Estimates for Total Wage

Combining the employment and the average wage, Table 16 shows the estimated effects on the total wages. As the impact from the minimum wage increase may be too moderate on large scale firms, the estimates based on all the firms are insignificant but those based

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Table 15: RD Estimators of Effects on Average Wage Payments in Firms

	All firms		Small scale firms	
	(1)	(2)	(3)	(4)
Panel A. CER optimal bandwidth estimator				
Full-time (*nominal*)	242***	261***	251***	271***
	(17)	(17)	(17)	(17)
Full-time (*real*)	746***	501***	755***	485***
	(19)	(19)	(19)	(19)
Part-time (*nominal*)	72	117***	139***	285***
	(44)	(44)	(43)	(43)
Part-time (*real*)	22	-99**	40	-114**
	(49)	(49)	(48)	(47)
Panel B. MER optimal bandwidth estimator				
Full-time (*nominal*)	332***	703***	338***	708***
	(17)	(17)	(18)	(17)
Full-time (*real*)	-104***	-328***	-92***	-309***
	(19)	(19)	(19)	(19)
Part-time (*nominal*)	77*	88*	209***	339***
	(45)	(45)	(43)	(43)
Part-time (*real*)	-220***	-535***	-139***	-80*
	(50)	(50)	(49)	(48)
Control:				
Year & month effects	No	Yes	No	Yes
Mean value before the policy change				
Full-time (*nominal*)	26,986		26,837	
Full-time (*real*)	29,995		29,829	
Part-time (*nominal*)	12,958		12,443	
Part-time (*real*)	14,403		13,830	

Notes: The table reports RD treatment effect estimators. Small scale firms are firms with less than 30 employees. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The real wage is deflated by the monthly CPI. All the estimators are related to the value of dependent variables rather than percentage change. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The row highlighted in grey color shows the influence from the fluctuation of the monthly CPI, which leads to overestimating the effect on the real wage. Hence, estimates under the MER bandwidth are more reliable to reflect the effects on the real wage. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

on the small scale firms are significant. Turning to the nominal value, after controlling the year and month effects, the total wages for full-time employment is about 1% to 4% higher than the prior; for part-time employment, the total wages hugely increase 14.0% to 25.2%.

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Turning to the real value, with the concern of the monthly CPI fluctuation, the MER estimators are also more reliable here. As the employment of the part-time workers increases hugely, the inflation has less influence on the estimates and the total real wages for the part-time workers are still significantly increased by about 23.4%. However, the total real wages on the full-time workers have no significant difference from the prior. It means that the slightly positive employment effect and the slightly negative real wage effect come together to lead to a null effect on the firms' total real wage payment for the full-time employment.

This finding provides a totally different story about firms' adjustments to the minimum wage increase. Rather than to lay off lowest-wage workers or to raise other workers' wages, the firms decrease the real wages of the unbound workers to mitigate the labor cost increase, which is from the wage hike of the lowest-wage employees and even the employment increases on the bound and unbound workers. Under this adjustment, no positive *"spillovers"* exist but negative ones do. But as the results show that the employment with wages above the new minimum is raised after the minimum wage increase, the effect on the inequality is still unclear through the firm-level analysis.

2.5.2 Estimate of Employment and Wage Effects on The Affected Individuals

As the firm-level analysis is on the employment stock of different wage levels and the average wages, the individual-level analysis can look into the effects on the individual workers that are supposed to be affected. Although there is already a vast literature providing evidence on workers who might suffer from the minimum wage increase, most of them focused on particular demographic or industrial groups (Belman et al., 2015). Few studies estimated effects on the affected classified by wages, but they kept using the technique of difference in difference and hence neglected the indirect effects (Clemens and Wither, 2014; Jardim et al., 2017). Only Neumark et al. (2004) used the conventional panel data framework to provide the direct and indirect effects at the time, but they failed to estimate the entry effect. This study contributes to the literature by providing the two effects separately through the RD estimation, which did not have to rely on a perfect counterfactual.

In this section, the estimates on the two groups classified by the earliest wage information are presented firstly, followed by the adjustment of the estimates. To make sure that no serious sample selection problems exist, a robustness estimate using a different

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Table 16: RD Estimators of Effects on Total Wage Payments in Firms

	All firms		Small scale firms	
	(1)	(2)	(3)	(4)
Panel A. CER optimal bandwidth estimator				
Full-time (*nominal*)	-719	-265	1,518*	2,140**
	(6,580)	(6,577)	(884)	(878)
Full-time (*real*)	6,474	6,209	3,333***	3,282***
	(7,363)	(7,363)	(983)	(983)
Part-time (*nominal*)	825	864	206***	243***
	(920)	(920)	(58)	(59)
Part-time (*real*)	1,055	1,070	254***	274***
	(1,030)	(1,029)	(63)	(65)
Panel B. MER optimal bandwidth estimator				
Full-time (*nominal*) wage	859	4,636	2,004**	7,195***
	(6,614)	(6,602)	(902)	(901)
Full-time (*real*)	-5,267	-8,180	-218	829
	(7,330)	(7,330)	(999)	(999)
Part-time (*nominal*)	728	1,065	215***	437***
	(958)	(936)	(58)	(59)
Part-time (*real*)	659	963	236***	451***
	(1,041)	(1,040)	(67)	(65)
Control:				
Year & month effects	No	Yes	No	Yes
Mean value before the policy change				
Full-time (*nominal*)	376,487		158,219	
Full-time (*real*)	418,458		175,857	
Part-time (*nominal*)	5,853		1,731	
Part-time (*real*)	6505		1,924	

Notes: The table reports RD treatment effect estimators. Small scale firms are firms with less than 30 employees. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The real wage is deflated by the monthly CPI. All the estimators are related to the value of dependent variables rather than percentage change. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The row highlighted in grey color shows the influence from the fluctuation of the monthly CPI, which leads to overestimating the effect on the real wage. Hence, estimates under the MER bandwidth are more reliable to reflect the effects on the real wage. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

classification method is also presented. After presenting the individual effect, I provide the estimated results on the difference between two groups. Notice that because of the lack of hourly information, the individual-level estimates are only on the full-time workers, but if

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they transfer to part-time jobs they will also be identified as being employed.

2.5.2.1 Estimates for Two Affected Groups

The estimated results of the effects on the two groups are presented in Table 17. Considering the fluctuation of the monthly CPI, estimates related to the real values are just provided under the MER bandwidth.

First, consider the employment effects on the two groups of workers. The directions of the estimated employment effects on the lower-wage group depend on whether the specific time effects are ruled out. Before controlling, the employment probability of this group significantly but slightly decreased by 0.33% to 0.63% compared to the prior level and there was no obvious difference between genders. But after controlling, the estimated employment effects turn positive, with a range from 0.5% to 2.3%. The phenomenon is similar for the higher-wage group. After controlling the specific time effects, the estimates change from insignificantly negative to significantly positive with a range of increase from 0.3% to 2.5%.

This means that their employment changes even just around the cutoff time are negatively affected by some factors which can be controlled by year and month. Hence, if estimating a difference in difference with some other groups of workers who are not affected by those unobservable factors, there could be on overestimate of the disemployment effect on the low-wage workers, which is similar to the argument of Allegretto et al. (2011) and Dube et al. (2010) about the specific time effect. The estimated effects on both groups are all significantly positive; this is in line with the finding from the firm-level analysis.

Second, I will observe to the wage effects. As expected, nominal wages of the lower-wage group have been significantly increased, although the amounts are reduced by controlling the specific time effects. Before ruling out the influence related to specific times, the range of increase is between 1.2% and 2.3%, but it is reduced to 0.6% to 1.5%. This means that, even if there is no minimum wage increase, their nominal wages still tend to be raised, but the increase due to the policy change let their wages increase more. However, when including the influence from the CPI, the positive effect on the lower-wage group is reduced more and only the females can still have a significantly positive real wage effect, which is about 0.52% higher than before. Thus, after considering the original wage increase and the inflation, the male individuals in the lower-wage group actually have no wage increase

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and the females have a very slight one.

On the other hand, even when not controlling the effects of specific times, there is only weak evidence proving that the nominal wage has raised among the higher-wage group; after ruling out the original wage increase, their nominal wages decline, although very slightly and only significantly among the females. And of course, the situation in the higher-wage group gets worse; they actually experience a wage decline of about 1.5% when considering both the original wage increase and the inflation. The result proves the inference in the firm-level analysis, which says that the decrease of the average real wage should come from the unbound workers and hence be opposite to the expectation of a spillover. The result is in line with Neumark et al. (2004), which is the only study providing wage and employment effects on workers of different wage levels.

Finally, we can see that the individuals of the lower-wage group have an increase of their real earnings by about 1.6%, which is from the increases on their employment and also their real wages. Correspondingly, individuals of the higher-wage group have no change on their real earnings, which results from the positive employment effect and the negative wage effect.

Therefore, results from both the firm-level and individual-level analysis indicate that no positive *"spillovers"* exist but negative ones do. However, higher-wage workers' employment is also increased. Those two effects offset each other lead to the null change on the higher-wage group's real earnings. Hence, the minimum wage can only reduce the real earnings inequality between the lowest-wage group and others. The reduction on the inequality do not spill over to other higher-wage workers. The results are obviously different from studies estimating the wage distribution (Dinardo et al., 1996; Lee, 1999), and provide a support to Autor et al. (2016), which questioned about the existence of the spillovers.

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Table 17: RD Estimators of Effects on Two Groups of Individuals

	Group of directly affected workers				Group of indirectly affected workers			
	Male		Female		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. CER optimal bandwidth estimator								
Employment prob.	-0.0023**	0.0034***	-0.0026***	0.0164***	-0.0023	0.0022	-0.0009	0.0078***
	(0.0010)	(0.0011)	(0.0009)	(0.0009)	(0.0028)	(0.0027)	(0.0021)	(0.0021)
Nominal wage	283***	164***	419***	316***	33	-114*	51	-77**
	(23)	(23)	(16)	(16)	(58)	(59)	(37)	(37)
Panel B. MER optimal bandwidth estimator								
Employment prob.	-0.0032***	0.0160***	-0.0045***	0.0040***	-0.0038	0.0155***	-0.0024	0.0186***
	(0.0011)	(0.0011)	(0.0009)	(0.0009)	(0.0027)	(0.0027)	(0.0022)	(0.0022)
Nominal wage	433***	144***	488***	138***	109*	-70	147***	-192***
	(23)	(23)	(17)	(16)	(59)	(59)	(37)	(37)
Real wage	168***	-36	205***	120***	-219***	-386***	-266***	-398***
	(26)	(26)	(18)	(18)	(65)	(65)	(41)	(41)
Real earnings	2	281***	164***	271***	-308***	22	-78	14
	(32)	(32)	(26)	(25)	(88)	(88)	(63)	(63)
Control:								
Year & month effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean before the policy change								
Employment prob.	0.696		0.717		0.722		0.752	
Nominal wage	22,838		20,743		24,496		22,624	
Real wage	25,385		23,055		27,227		25,146	
Real earnings	17,679		16,534		19,652		18,905	

Notes: The table reports RD treatment effect estimators. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD in Jan. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD in Jan. 2004. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The real wage is deflated by the monthly CPI. Because of the influence of the monthly CPI fluctuation, only MER estimators of the real wage effects are included. All the estimators are related to the value of dependent variables rather than percentage change. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The mean value before the policy change is the mean value in June 2007.

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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2.5.2.2 Adjusted Estimates for The Affected Individuals

As the two groups actually have mixed compositions of different kinds of workers, the findings above may be undermined. To solve this problem, the adjusted estimates for the actual affected individuals by using Eqs. (18) and (19) are presented in Table 18. All the variances are calculated by using Eqs. (20) and (21) with a 0 value for the covariance between the direct and indirect effects. As discussed before, the variances should be overestimated under this setting, hence the estimates would be actually significant if they already are in the table.

First, regarding the employment effect, after ruling out the unobservable factors, the actual bound workers have their employment increased in the shorter bandwidth, and the effect on the females (3.8%) is higher than the effects on the males (0.9%). However, when using the longer bandwidth, the males have a larger increase (3.8%) and the females have a moderate decrease (-0.5%). On the other hand, the effects on the unbound workers with slightly higher wages than the new minimum are not so significant when using the CER bandwidth, but are significantly positive under the MER bandwidth (4.9% for the males and 6.9% for the females). This means that even after adjusting the estimates, results still show that there are moderately negative and even positive effects on the bound workers, and positive effects on the unbound workers, although different bandwidths lead to strong or weak estimated results.

Second, turning to the wage effect, the adjusted results clearly separate the opposite effects on the two kinds of workers. If the specific time effects are not considered, the bound workers' nominal wages are increased by 5.1% to 7.5%. The effects still remain at a range from 3.0% to 5.6% when cutting potential increases related to specific years and months. Even deflated by the CPI, the effects are still positive, although reduced to 1.1% to 3.6%.[54] On the contrary, even before controlling specific time effects, the unbound workers' nominal wages have no significant change, and are significantly changed by -2.8% to -4.7% when cutting off the wage increases they should have. When the inflation is also considered, their wages are changed by -7.9% to -8.1%.

The clearer results reflect that the estimates on the two groups actually mix the two opposite effects and make the story more definite: the lower-wage workers have their wages

[54] Although the derived estimate of the real wage effect on the females is higher than the nominal one, it only happens when controlling the specific time effects, and does not necessarily reflect the influence from the fluctuation of the monthly CPI.

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increase higher than the potential increase and the inflation, while the higher-wage workers have their wages stagnated and even decreased in the real value.

Finally, as to the real earnings, the adjusted estimates still show that there is no significant change on the unbound workers, but a significant increase on the bound workers by 4.1% to 4.8%. Therefore, it proves again that there are no spillovers on the inequality effect of the minimum wage increase.

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Table 18: Adjusted Estimates for The Affected Individuals

	The directly affected individuals				The indirectly affected individuals			
	Male		Female		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. CER optimal bandwidth estimator								
Employment prob.	-0.005**	0.0087***	-0.0062***	0.0373***	-0.0074	0.005	-0.0006	0.0124*
	(0.0021)	(0.0021)	(0.0015)	(0.0015)	(0.0112)	(0.0113)	(0.0074)	(0.0074)
Nominal wage	838***	596***	1078***	909***	-245	-742***	-250*	-629***
	(48)	(48)	(28)	(28)	(240)	(241)	(130)	(130)
Panel B. MER optimal bandwidth estimator								
Employment prob.	-0.0068***	0.0362***	-0.0099***	-0.005***	-0.0125	0.0475***	-0.0043	0.067***
	(0.0021)	(0.0021)	(0.0015)	(0.0016)	(0.0112)	(0.0113)	(0.0076)	(0.0075)
Nominal wage	1235***	499***	1180***	531***	-112	-517**	46	-883***
	(48)	(47)	(29)	(28)	(243)	(242)	(130)	(130)
Real wage	692***	205***	773***	655***	-1222***	-1687***	-1238***	-1651***
	(53)	(53)	(31)	(31)	(269)	(269)	(144)	(144)
Real earnings	257***	841***	503***	713***	-1389***	-294	-474**	-235
	(62)	(62)	(39)	(39)	(361)	(360)	(220)	(220)
Control:								
Year & month effects	No	Yes	No	Yes	No	Yes	No	Yes
Derived mean value before the policy change								
Employment prob.	0.9619		0.9715		0.9619		0.9715	
Nominal wage	16,418		16,284		18,597		18,705	
Real wage	18,248		18,100		20,670		20,790	
Real earnings	17,552		17,584		19,882		20,198	

Notes: The table reports adjusted RD treatment effect estimators. The directly affected individuals are workers who actually have insured wage levels of 15,840 and 16,500 NTD in June 2007, and also include individuals who may be at those levels but are out of employment at that time. The indirectly affected individuals are workers with insured wage levels at 18,300, 19,200, and 20,100 NTD with the same setting. The estimates and variances are derived from estimates and variances in Table 17 and the workers compositions of two groups in June 2007. The variances should be overestimated, as the covariance of the two actual effects should be positive. The derived mean value before the policy change is derived by the worker composition in June 2007.

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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2.5.2.3 Robustness Test by Adjusted Classification

Considering that only one month of information is used to classify workers, results may be influenced by some particularity at that time. To make a further confirmation of the results, this part uses three additional months of information. Workers only with wages lower or just slightly higher than the minimum wage from January 2004 to April 2004 are included in the respective groups. In other words, the new groups add workers out of work in January 2004 but employed in any of the other three months, and subtract workers who have higher wages in the additional three months. Compared to the original classification, this method loosens the restriction of the time of employment but is more restrictive on the wages.

Table 19 shows the estimated results for the two groups, and Table 20 shows the adjusted estimates on the actual affected. The comparisons between the adjusted estimates from the original and the adjusted classifications are shown in Figures 33, 34, and 35. To simply sum up the comparisons, the adjusted classification method also leads to consistent results; in particular, it has almost the same estimates for real earnings.

First, Figure 33 shows the adjusted estimates of the employment effects. Results from the adjusted classification further confirm the positive employment effects on the bound and unbound workers. By observing the 95% confidence intervals, the only obvious difference of results between the two classifications is the employment effect on the female bound workers (33(a)). Those workers classified by the original classification have a high employment increase under the CER bandwidth and a slight decrease when using the MER one. But the results turn to all positive and similar to the male's when they are classified by the adjusted classification. Considering that the original method is more restrictive on the employed time, the result from the adjusted one should be closer to the fact. Another difference is that the unbound workers classified by the adjusted classification have their employment significantly increased in the shorter bandwidth, although the confidence intervals of results from the two classifications have overlapped.

Second, Figure 34 shows the adjusted estimates of the nominal wage effects. It also shows opposite effects on the two kinds of workers when using the adjusted classification. Besides, the wage increase on the bound workers are higher when using the MER, the longer bandwidth.

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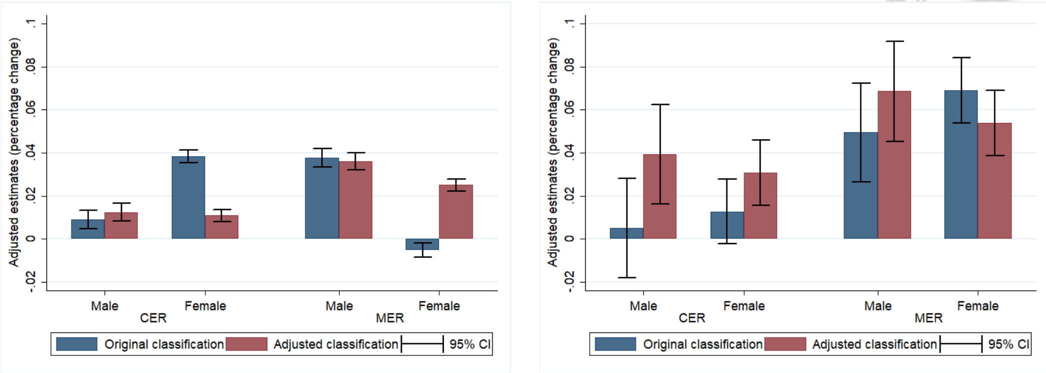
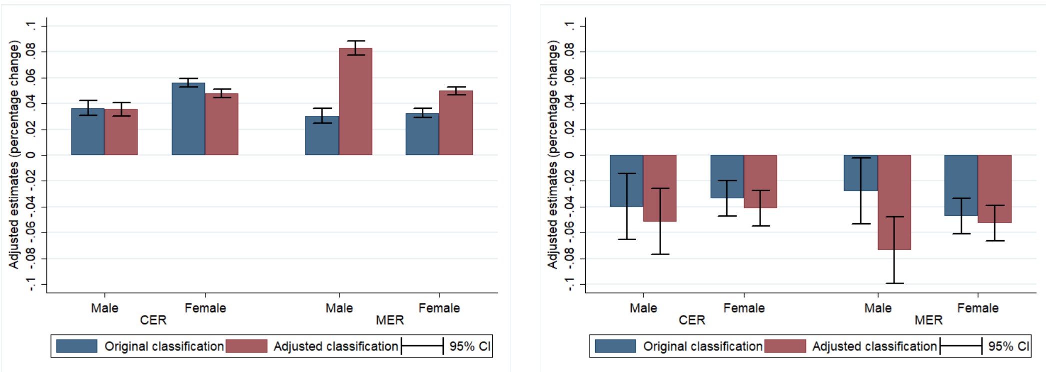


(a) Direct Effect (b) Indirect Effect

Figure 33: Adjusted Estimates of Employment Effect

Notes: This figure shows the adjusted estimates of the employment effects on the directly affected and indirectly affected individuals. All the estimates are made while controlling the specific year and month effects. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The vertical axis shows the ratio of the adjusted estimates to the mean value in June 2007. The original classification uses the wage information in Jan. 2004 to classify groups, while the adjusted classification uses the wage information from Jan. 2004 to April 2004.



(a) Direct Effect (b) Indirect Effect

Figure 34: Adjusted Estimates of Nominal Wage Effect

Notes: This figure shows the adjusted estimates of the nominal wage effects on the directly affected and indirectly affected individuals. All the estimates are made while controlling the specific year and month effects. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The vertical axis shows the ratio of the adjusted estimates to the mean value in June 2007. The original classification uses the wage information in Jan. 2004 to classify groups, while the adjusted classification uses the wage information from Jan. 2004 to April 2004.

Finally, Figure 35 shows the adjusted estimates of the real wage and real earnings effects. As expected, the real wage effects on the two kinds of workers are still opposite. In addition, the estimates of the real earnings effect from the two classifications are nearly the

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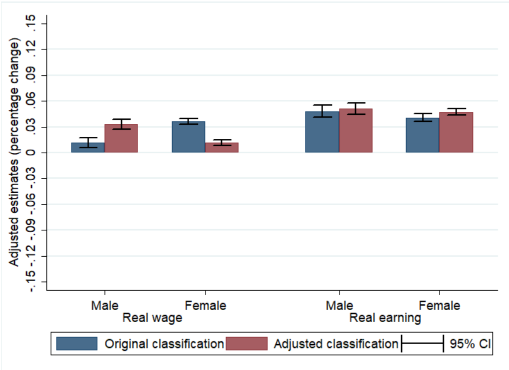
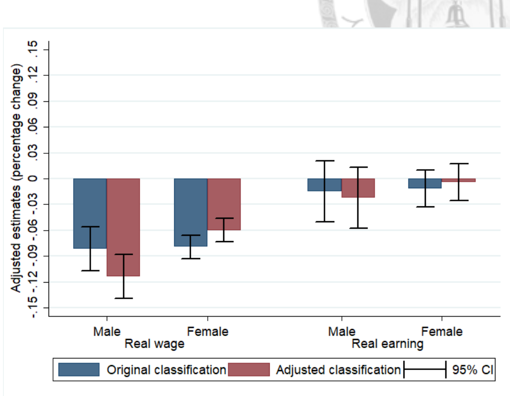
same. The fact that there are no spillovers on the earning inequality due to the minimum wage increase is confirmed once again.



(a) Direct Effect



(b) Indirect Effect

Figure 35: Adjusted Estimates of Real Wage and Earning Effect

Notes: This figure shows the adjusted estimates of the real wage and real value effects on the directly affected and indirectly affected individuals. All the estimates are made while controlling the specific year and month effects. Because the fluctuation of the monthly CPI may let the results be overestimated in a shorter bandwidth, the estimates in this figure are all estimated from MER bandwidth, which is about 10 months before and after the cutoff time. The vertical axis shows the ratio of the adjusted estimates to the mean value in June 2007. The original classification uses the wage information in Jan. 2004 to classify groups, while the adjusted classification uses the wage information from Jan. 2004 to April 2004.

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Table 19: RD Estimators of Effects on Two Groups of Individuals (Adjusted Classification)

	Group of directly affected workers				Group of indirectly affected workers			
	Male		Female		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. CER optimal bandwidth estimator								
Employment prob.	-0.0026***	0.0065***	-0.0025***	0.0069***	-0.0025	0.010***	-0.0016	0.0095***
	(0.0010)	(0.0010)	(0.0009)	(0.0009)	(0.0026)	(0.0026)	(0.0021)	(0.0021)
Nominal wage	325***	137***	383***	238***	10	-157***	57	-125***
	(22)	(22)	(16)	(16)	(56)	(56)	(36)	(36)
Panel B. MER optimal bandwidth estimator								
Employment prob.	-0.0033***	0.016***	-0.0032***	0.014***	-0.0048*	0.019***	-0.0036*	0.017***
	(0.0010)	(0.0010)	(0.0009)	(0.0010)	(0.0026)	(0.0026)	(0.0021)	(0.0021)
Nominal wage	433***	373***	466***	230***	135**	-168***	159***	-183***
	(21)	(22)	(16)	(16)	(56)	(56)	(36)	(36)
Real wage	102***	55**	157***	-31*	-267***	-476***	-267***	-321***
	(24)	(24)	(17)	(17)	(62)	(62)	(40)	(40)
Real earnings	-17	274***	24	319***	-294***	-5	-268***	70
	(30)	(30)	(24)	(24)	(84)	(84)	(60)	(60)
Control:								
Year & month effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean value before the policy change								
Employment prob.	0.675		0.703		0.704		0.738	
Nominal wage	22,976		20,838		24,611		22,703	
Real wage	25,537		23,161		27,355		25,234	
Real earnings	17,247		16,271		19,259		18,613	

Notes: The table reports RD treatment effect estimators. The group of directly affected workers contains workers who were full-time employed with insured wage levels of 15,840 and 16,500 NTD between Jan. 2004 and Apr. 2004. The group of indirectly affected workers contains workers who were full-time employed with insured wage levels of 18,300, 19,200, and 20,100 NTD between Jan. 2004 and Apr. 2004. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The real wage is deflated by the monthly CPI. Because of the influence of the monthly CPI fluctuation, only MER estimators of the real wage effects are included. All the estimators are related to the value of dependent variables rather than percentage change. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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Table 20: Adjusted Estimates for The Affected Individuals (Adjusted Classification)

	The directly affected individuals				The indirectly affected individuals			
	Male		Female		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. CER optimal bandwidth estimator								
Employment prob.	-0.0061***	0.012***	-0.0055***	0.0106***	-0.0079	0.0378***	-0.0036	0.03***
	(0.002)	(0.002)	(0.0014)	(0.0014)	(0.0112)	(0.0113)	(0.0075)	(0.0075)
Nominal wage	1046***	582***	1017***	778***	-451*	-956***	-202	-769***
	(45)	(45)	(26)	(26)	(242)	(242)	(131)	(131)
Panel B. MER optimal bandwidth estimator								
Employment prob.	-0.0066***	0.0347***	-0.0056***	0.0244***	-0.0177	0.0659***	-0.0107	0.0522***
	(0.002)	(0.002)	(0.0014)	(0.0014)	(0.0114)	(0.0114)	(0.0076)	(0.0075)
Nominal wage	1284***	1359***	1156***	809***	-18	-1370***	111	-989***
	(46)	(46)	(26)	(26)	(242)	(243)	(131)	(131)
Real wage	570***	602***	680***	204***	-1430***	-2356***	-1242***	-1248***
	(51)	(52)	(29)	(29)	(269)	(269)	(145)	(145)
Real earning	209***	892***	311***	828***	-1379***	-444	-1098***	-81
	(58)	(58)	(34)	(34)	(362)	(362)	(218)	(218)
Control:								
Year & month effects	No	Yes	No	Yes	No	Yes	No	Yes
Derived mean value before the policy change								
Employment prob.	0.9619		0.9715		0.9619		0.9715	
Nominal wage	16,408		16,267		18,602		18,712	
Real wage	18,237		18,080		20,675		20,798	
Real earning	17,541		17,565		19,887		20,205	

Notes: The table reports adjusted RD treatment effect estimators. The directly affected individuals are workers who actually have insured wage levels of 15,840 and 16,500 NTD in June 2007, and also include individuals who may be at those levels but are out of employment at that time. The indirectly affected individuals are workers with 18,300, 19,200, and 20,100 NTD with the same setting. The estimates and variances are derived from estimates and variances in Table 17 and the workers compositions of two groups in June 2007. The variances should be overestimated, as the covariance of the two actual effects should be positive. The derived mean value before the policy change is derived by the worker composition in June 2007.
* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

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2.5.3 Estimate of Effects on The Difference

Even as the effects on the bound and unbound workers can be used to determine the difference, a more direct way is to estimate their difference instead. Based on Eq. (22), the RD estimates of the effects on the differences between the two groups classified by both the original and adjusted methods are shown in Table 21. The advantage of estimating the gap directly is to move out all the unobservable factors that influenced the two kinds of workers at the same time, although the estimates for the individuals already control some factors related to specific years and months. As the effects from the fluctuation of the monthly CPI can be eliminated by the difference of the two groups, the estimates under the CER bandwidth for the real value are also provided here.

The estimates are very robust under different classifications and under different bandwidths. Compared to the prior levels, the employment gap between the female groups is significantly widened by about 7% under the CER bandwidth and 10% under the MER bandwidth, while the male employment gap is only significantly widened by 9% under the MER bandwidth. If we used the prior assumption that the unbound workers are not affected by the minimum wage increase, we would be mislead to a wrong conclusion that the bound workers experienced a significant disemployment effect, especially the females. The misleading result is in line with the finding from studies using the triple difference, such as Clemens and Wither (2014). However, based on the above findings, the results here actually reflect the higher employment growth among the higher-wage group.

The gaps of the nominal wage and the real wage between the two groups are decreased by about 20%. Based on the individual-level analysis above, the reduction should be not only from the wage increase in the lower-wage group but also from the decrease in the higher-wage group. Finally, combining the employment and wage effects, the gap of real earnings decreases by 10%; there is not much difference between genders.

Unfortunaely, since the two groups actually contain workers on different wage levels, the estimates here are not unbiased. Based on Eq. (22), the estimates here are the $(r^h_H - r^l_H)\beta_H - (r^l_L - r^h_L)\beta_L$. As the first ratio difference is 18% for the males and 20% for the females in the dataset, and the second ratio difference is 25% for the males and 30% for the females, the estimates actually have a higher weighting on the effects on the bound workers. Since the employment effect and the wage effect on the bound workers are all

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positive, it means that the widening employment gap is underestimated, and the reduction on the wage gap is overestimated.

Table 21: RD Estimates of Effects on Gaps Between Two Groups

	Original classification		Adjusted classification	
	Male	Female	Male	Female
Panel A. CER optimal bandwidth estimator				
Employment prob.	-0.0001	0.0024***	0.0006	0.0020**
	(0.0010)	(0.0009)	(0.0010)	(0.0009)
Nominal wage	-287***	-351***	-283***	-339***
	(23)	(16)	(22)	(16)
Real wage	-323***	-413***	-319***	-400***
	(26)	(18)	(24)	(17)
Real earnings	-202***	-249***	-239***	-244***
	(32)	(25)	(30)	(23)
Panel B. MER optimal bandwidth estimator				
Employment prob.	-0.00056	0.0038***	0.0025**	0.0036***
	(0.0011)	(0.0009)	(0.0010)	(0.0009)
Nominal wage	-320***	-372***	-319***	-365***
	(23)	(17)	(22)	(16)
Real wage	-378***	-442***	-376***	-431***
	(26)	(19)	(24)	(18)
Real earnings	-285***	-243***	-205***	-233***
	(32)	(25)	(31)	(24)
Mean value before the policy change				
Employment prob.	0.0253	0.0347	0.0287	0.0351
Nominal wage	1,657	1,881	1,636	1,864
Real wage	1,842	2,090	1,818	2,072
Real earnings	1,973	2,371	2,012	2,342

Notes: The table reports estimates of the changes on the differences between the directly and indirectly affected workers, which are classified by the wage information in Jan. 2004 or between Jan. 2004 and Apr. 2004. The difference is calculated by the mean value of the higher-wage group at time t minus every single value of individuals from the lower-wage group. Since the influence of the CPI's variation is eliminated by the difference of the two groups, the estimates under the CER bandwidth for the real value are also provided. The table reports RD treatment effect estimators. All the estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Regardless of the biased estimates between the gap, the framework of the gap estimation can provide a way to test if the unbound workers have any nominal wage increase due to the minimum wage increase. Since the increased amount of the nominal wage to the bound

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workers β_L is decided by the new minimum wage, as long as all the ratios are known, the only unknown part of Eq. (22) for the nominal wage gap is the β_H. Hence, we can form a null hypothesis that $\beta_H = 0$ to test whether the actual estimates can reject the hypothesis.[55]

Table 22 shows the difference between our estimates and the expected value. The results are robust. No matter which classification and bandwidth are used, the nominal wage gaps for both the males and the females actually are significantly reduced more than expected. This means that rather than wage spillovers, the unbound workers had wage decreased due to the minimum wage increase. It once again proves the above finding, in line with the finding of Neumark et al. (2004) and opposite to the findings of Dube et al. (2019) and Cengiz et al. (2018).

2.5.4 Effects on The Employment Flow

The above findings are robust to show that there are no spillovers on the earning inequality effect due to the minimum wage increase. The result is from the employment and wage increase on the bound workers, and the employment increase and the wage decrease on the unbound workers. It raises a question of how those phenomena occur at the same time. Although the finding on the unbound workers is the same as the finding of Neumark et al. (2004), they also found a disemployment effect on the bound workers and inferred that to the labor supply increase of the higher-wage workers mitigated the disemployment effect on their family members. As the bound workers are not found to suffer in this study, the inference of Neumark et al. (2004) does not apply here.

On the other hand, the effects on the bound workers are in line with recent studies related to the search frictions in the labor market, such as Giuliano (2013) and Dube et al. (2016). But those studies have not estimated the effects on the unbound workers. Hence, a simple thinking is that if the dataset can also provide evidence of the traits of the search friction in the labor market, then the effects on the unbound workers may be able to be explained by the frictions. According to Dube et al. (2016), the related phenomenon to the

[55] Even if there may be some unobservable factors affecting the two kinds of workers at the same time, they are eliminated by differencing. Only the time trend can affect the estimate here. However, since the time trend is reducing the wage gap, see Figure 30, the estimate should be the reduction from the nominal minimum wage increase minus the time trend. In other words, if the time trend is strong and we only subtract the mechanical minimum wage increase from the nominal wage gap, we actually overestimate the increase of the unbound workers' nominal wages. Hence, if the result still shows that the unbound workers have no wage increase, then it would be true.

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Table 22: Test for The Indirect Nominal Wage Effect

	Original classification		Adjusted classification	
	Male	Female	Male	Female
Panel A. CER optimal bandwidth estimator				
Nominal wage gap	-287***	-351***	-283***	-339***
	(23)	(16)	(22)	(16)
Difference from expectation	-69***	-85***	-78***	-84***
(t value)	-2.98	-5.24	-3.61	-5.41
Panel B. MER optimal bandwidth estimator				
Nominal wage gap	-320***	-372***	-319***	-365***
	(23)	(17)	(22)	(16)
Difference from expectation	-101***	-107***	-114***	-110***
(t value)	-4.37	-6.44	-5.22	-6.95
Expected change by the null hypothesis	-218	-266	-205	-255

Notes: The table reports the test results of the indirect nominal wage effect. The wage gap is the estimate of the change in the differences between the directly and indirectly affected workers, which are classified by the wage information in Jan. 2004 or between Jan. 2004 and Apr. 2004. The expected change by the null hypothesis is calculated by the bound workers' mechanical wage increase due to the minimum wage increase, the ratios of composition, and the zero indirect wage effect. The t value of the difference from the expectation is calculated by the variance of estimates.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

search friction is the reduction on the employment flow after the minimum wage increase, which is due to a decrease of layoffs and employees being less likely to transition to better jobs. That is, due to the same reason, it is possible that the higher-wage workers may be more likely than before the minimum wage increase to stay in the same firms even if their nominal wages have not increased. To see if the hypothesis can be supported by the dataset, the following parts provide estimates of the effects on the firms' separation and hire rates and the individual transitions between months.

2.5.4.1 Employment Flow on Firm Level

Based on the matched employee-employer dataset, the estimate on the separation and hire rates of firms are shown in Table 23. Results for all firms and small scale firms are close. Although there is a difference in the hire rate of full-time workers before controlling the specific year and month effects, it is wiped out when controlling the time effects.

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Considering that the small scale firms are more exposed to the impacts of the minimum wage increase, the following discusses the estimates on the subsample.

Table 23: RD Estimates of Effects on Employment Flow

	All firms		Low scale firms	
	(1)	(2)	(3)	(4)
Panel A. CER optimal bandwidth estimator				
Separation rate *(FT)*	0.00026	5.87e-06	0.00015	8.74e-05
	(0.00022)	(0.00022)	(0.00023)	(0.00021)
Hire rate *(FT)*	0.013***	0.009***	0.001	0.013***
	(0.003)	(0.002)	(0.002)	(0.003)
Separation rate *(PT)*	0.029***	0.030***	0.037***	0.037***
	(0.003)	(0.003)	(0.003)	(0.003)
Hire rate *(PT)*	0.089***	0.089***	0.056***	0.051***
	(0.009)	(0.009)	(0.009)	(0.008)
Panel B. MER optimal bandwidth estimator				
Separation rate *(FT)*	0.000966***	-0.000614***	0.000864***	-0.000887***
	(0.00018)	(0.00017)	(0.00019)	(0.00017)
Hire rate *(FT)*	0.0044**	-0.0018	0.0025	-0.0014
	(0.0018)	(0.0039)	(0.0017)	(0.0040)
Separation rate *(PT)*	0.043***	0.045***	0.037***	0.033***
	(0.002)	(0.002)	(0.002)	(0.002)
Hire rate *(PT)*	0.072***	0.067***	0.057***	0.043***
	(0.007)	(0.007)	(0.007)	(0.007)
Control:				
Year & month effects	No	Yes	No	Yes
Mean value before the policy change				
Separation rate *(FT)*	0.027		0.027	
Hire rate *(FT)*	0.033		0.033	
Separation rate *(PT)*	0.107		0.105	
Hire rate *(PT)*	0.134		0.119	

Notes: The table reports RD treatment effect estimators. Small scale firms are firms with less than 30 employees. The separation rate at time t is the number of separations at time t divided by the total number of workers observed at time $t-1$. The hire rate at time t is the number of new hires at time t divided by the total number of workers observed at time $t-1$. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. The real wage is deflated by the monthly CPI. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

First, regarding full-time employment, different bandwidths lead to different results. Under the shorter bandwidth, the CER bandwidth, the separation rate has no significant

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change no matter whether the specific time effects are controlled or not. But the hire rates is significantly raised by about 39% compared to the prior level after controlling the specific time effects. However, the results are opposite when using the longer bandwidth, the MER bandwidth. After ruling out unobservable factors related to the specific year and month, the separation rate is significantly reduced by about 3%, but the hire rate has no significant change.

Even the results are opposite under different bandwidths, both of them lead to an increase of employment, which is in line with the finding above. But they lead to an interesting fact that in the shorter period more full-time workers are newly hired, while in a longer period there is no significant change on the new hires but the incumbents are less likely to leave their jobs. Although the longer time effect is as expected by the model of search frictions, the shorter period evidence gives questions to it.

Second, turning to part-time employment, estimates are all robust and strongly positive on different settings. Compared to the prior level, their separation rates are increased by a percent in the low 30s, and their hire rates are increased by a percent in the high 30s to low 40s. In addition, the value of the change on the hire rate is higher than the one on the separation rate, indicating employment growth, which is in line with above findings. As the hourly minimum wage is increased by 44%, which is much higher than the monthly minimum wage's 9% increase, effects on the part-time employment should be more obvious. But they show a stronger evidence against the findings from Dube et al. (2016).

2.5.4.2 Individual Transition

As the separation and hire rates are very divergent as shown in Figure 22, the RD estimates may be questioned. Besides, the total employment flow in firms may mix the effects on different levels of workers. To understand the actual transitions of different kinds of workers, Figures 36, 37, 38, and 39 show transition matrixes of the males and females.

The transition matrix is composed of five employment statuses: full-time employed with bound wages (FT bound), full-time employed with wages one to three levels higher than the new minimum (FT above), full-time employed with much higher wages (FT high), part-time employed (PT), and out of employment (Out). Between every two time periods, the transitions can be classified to 25 kinds. As the adjusted classification is less restrictive to the employed time compared to the original one, here I use the group of the directly

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affected workers classified by the adjusted classification.[56] All the percentages show the probability of transiting to a status at time $t+1$ conditional on a status at time t.[57] Different from the graphs before, these graphs show the changes between statuses. Every bin contains different individuals. To notice that, compared to the RD graph, once workers transit to another status, they will be counted as in that status in the next period, so it would be just a one month change rather than a level shift.

The rows of the matrix show where the workers go. Workers in the status of "FT bound", "FT above", and "FT high" at time t only have an obvious increased probability to go to the status of "PT" right after the policy change.[58] Workers in the status of "PT" have increased probabilities to the three "FT" statuses and the status of "Out" at the second month after the policy change, and a decreased probability to stay in "PT" correspondingly. Workers in the status of "Out" seem to have no obvious change. Based on these findings, a few inferences are as follows.

First, no evidence shows the separation is declining, but the entry from non-employment to full-time low-wage jobs may increase. There is no obvious separation change on the full-time bound workers or the workers with wages slightly above the new minimum after the policy change, as the "FT bound to FT bound" and "FT above to FT above" have no obvious increase after the cutoff time. Hence, the above findings of the employment growth of those two kinds of workers should reflect the entry from non-employment, which is in line with the short-run findings in Table 23. Unfortunately, the inference cannot be directly proved here, as the "Out" contains different kinds of workers and thus cannot show the entry effect on potential low-wage workers.

Since the minimum wage increase raises the bound workers' wages, the potentially increased entry to them should reflect an increased labor supply. On the other hand, the increased entry to the "FT above" may be from an increased labor demand because of lower relative wages, but also may be from an increased labor supply. Neumark et al. (2004) found similar results but inferred the upward shift of the labor supply to be a complement for their lower-wage family members' disemployment. As the disemployment effect is not found in this study, even if the increased entry to the "FT above" is really from an increase

[56] Actually, the transition matrix is not so different if the original classification is used.

[57] For example, Figure 36(e) shows the probability of being "Out" at time $t+1$, given they are "FT bound" at time t. At any time, the sum of probabilities shown in Figure 36(a), 36(b), 36(c), 36(d), and 36(e) equals to 1.

[58] There are three months at which "FT bound" transits to "FT above" or "FT high", and "FT above" transits to "FT high". They reflect the wage increase.

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of the labor supply, the reason is unknown.

Second, it is clear that the much higher increase in the hourly minimum wage leads full-time workers to transit to part-time jobs. As the hourly minimum wage has a 44% increase and the monthly minimum wage only has a 9% change, the transition is more likely to reflect that the part-time jobs become more appealing to some low-wage full-time workers, rather than the workers being forced to become part-time employed by their employers.[59] But actually, the amount is too small to affect the whole employment change of full-time workers, as the probabilities of maintaining the same full-time statuses have no correspondingly huge change.

Third, the increased flow from part-time workers to full-time jobs at the second month should reflect some full-time workers transiting back. To the employers, as the new hourly minimum wage is the same as the monthly minimum wage divided by standard working hours, and the part-time employment still gives employers some flexibility, employers should have no incentive to force part-time workers to be full-time employed. On the other hand, the original part-time workers have higher wages than before, the policy change would not give them higher incentive to transit to full-time jobs. Hence, the transition should reflect the movement of workers who just transit to part-time jobs.

Therefore, all the results indicate that the minimum wage increase drives the employment flow rather than reduces it. In addition, the increased employment flow along with the finding of employment growth should reflect that the minimum wage increase raises the labor supply of part-time and full-time bound-wage jobs. Hence, the results are more in line with the theory of the monoposony, in which the employment is constrained by the upward labor supply Card and Krueger (1994). However, the increase in the number of full-time jobs with wages slightly above the new minimum could be driven by the labor demand or labor supply.

[59] The new hourly minimum wage is the same as the monthly minimum wage divided by the standard working hours, so the transition would not lead to higher hourly wage. Moreover, the transition may have other benefits of part-time employment, such as working flexibility.

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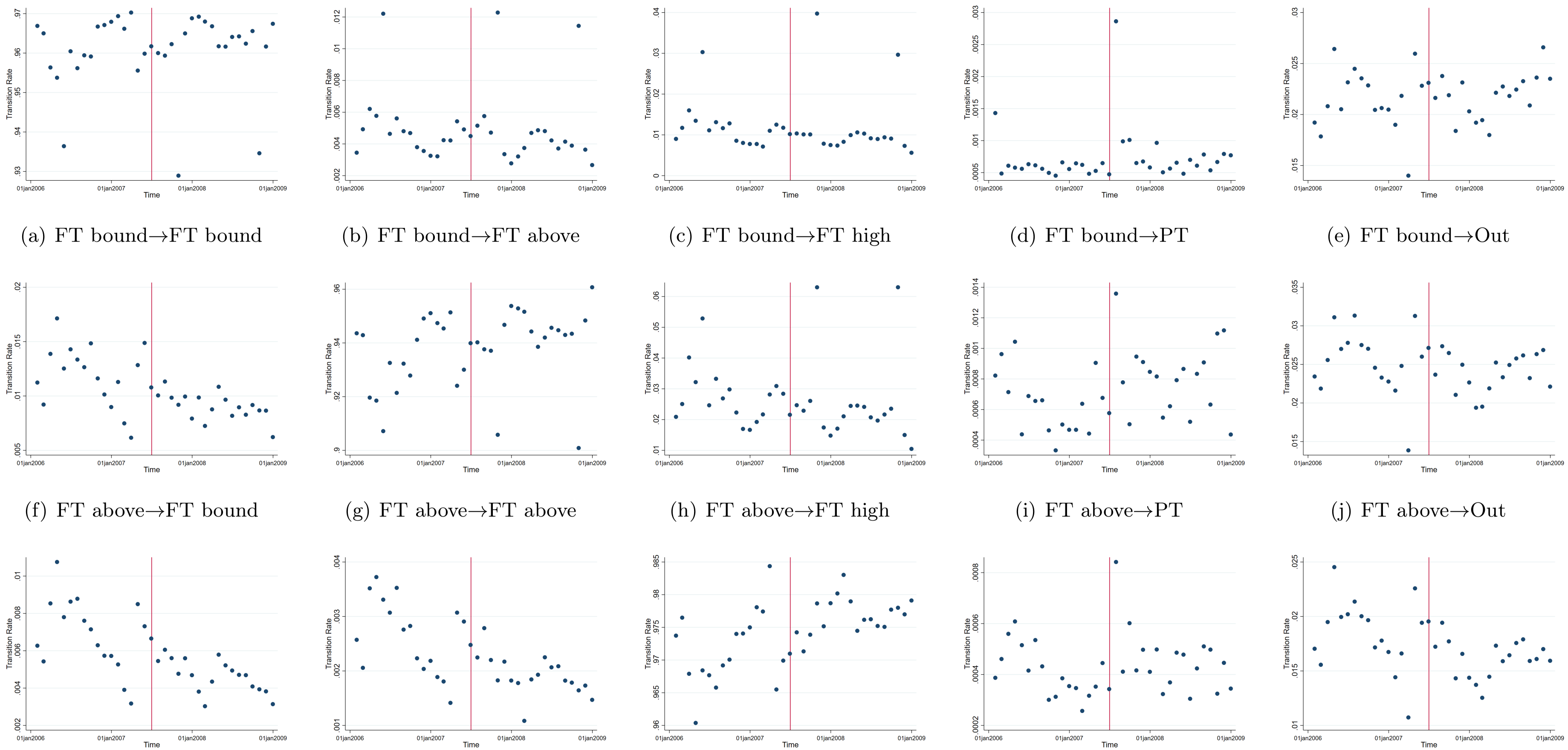

(a) FT bound→FT bound
(b) FT bound→FT above
(c) FT bound→FT high
(d) FT bound→PT
(e) FT bound→Out
(f) FT above→FT bound
(g) FT above→FT above
(h) FT above→FT high
(i) FT above→PT
(j) FT above→Out
(k) FT high→FT bound
(l) FT high→FT above
(m) FT high→FT high
(n) FT high→PT
(o) FT high→Out

Figure 36: The Transitions of Male Workers in The Group of Directly Affected (Adjusted Sample)

Notes: The figure shows the transition matrix of the workers in the group of directly affected, which is classified by the wage information between Jan. 2004 and April 2004. The transition matrix is composed of five employment statuses: full-time employed with bound wages (FT bound), full-time employed with slightly above wages than the new minimum (FT above), full-time employed with much higher wages (FT high), part-time employed (PT), and out of employment (Out). The percentages show the probability of transiting to a status at time $t+1$ conditional on a status at time t. Every bin is the mean value in the corresponding month. The red vertical line is set on June 2007. Due to the part-time data not being available until 2006, the period shown here is from Jan. 2006 to Dec. 2008.

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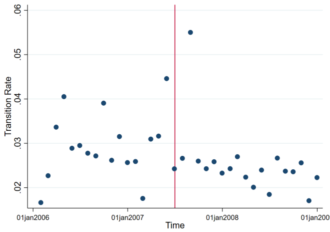
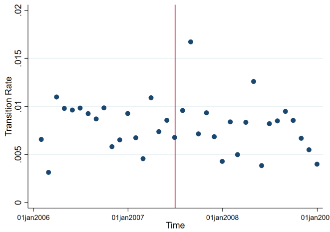
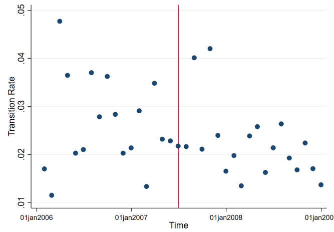


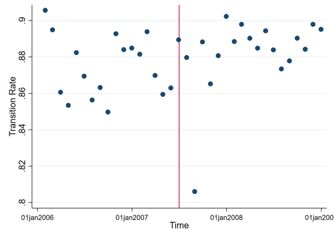
(a) PT→FT bound



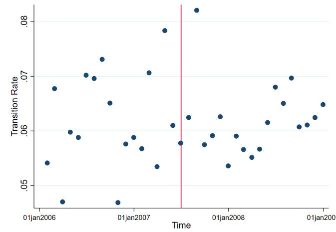
(b) PT→FT above



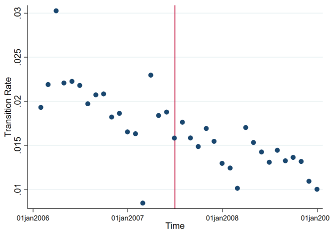
(c) PT→FT high



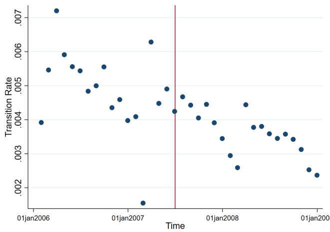
(d) PT→PT



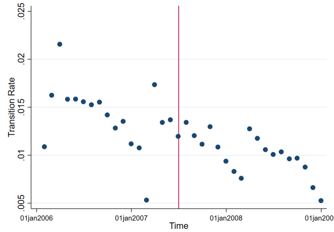
(e) PT→Out



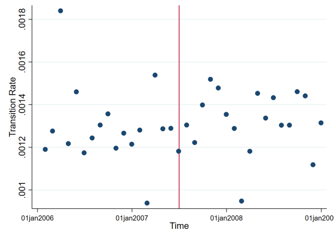
(f) Out→FT bound



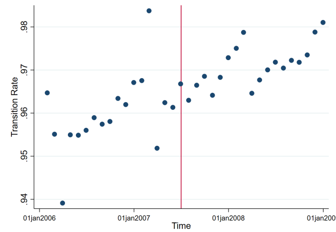
(g) Out→FT above



(h) Out→FT high



(i) Out→PT



(j) Out→Out

Figure 37: The Transitions of Male Workers in The Group of Directly Affected (Adjusted Sample) (Continued)

Notes: The figure shows the transition matrix of the workers in the group of directly affected, which is classified by the wage information between Jan. 2004 and April 2004. The transition matrix is composed of five employment statuses: full-time employed with bound wages (FT bound), full-time employed with slightly above wages than the new minimum (FT above), full-time employed with much higher wages (FT high), part-time employed (PT), and out of employment (Out). The percentages show the probability of transiting to a status at time $t+1$ conditional on a status at time t. Every bin is the mean value in the corresponding month. The red vertical line is set on June 2007. Due to the part-time data not being available until 2006, the period shown here is from Jan. 2006 to Dec. 2008.

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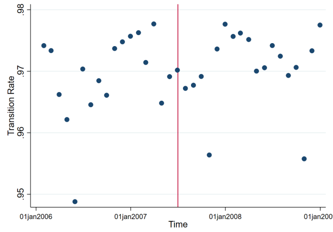
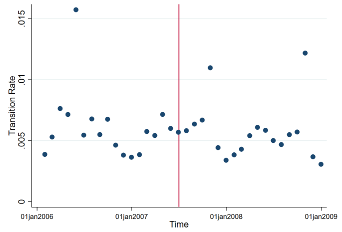
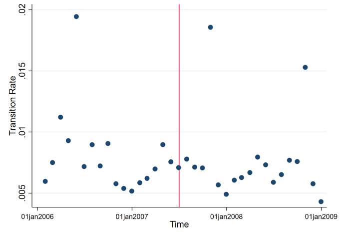


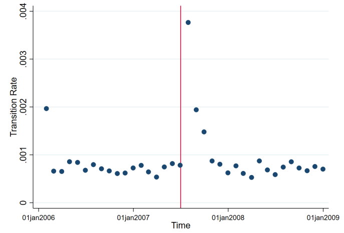
(a) FT bound→FT bound



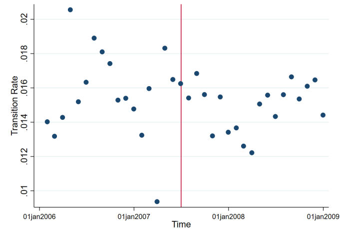
(b) FT bound→FT above



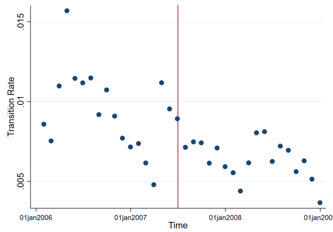
(c) FT bound→FT high



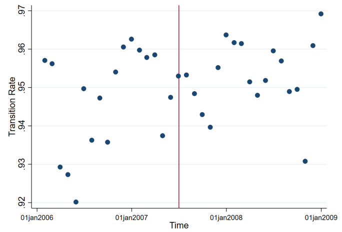
(d) FT bound→PT



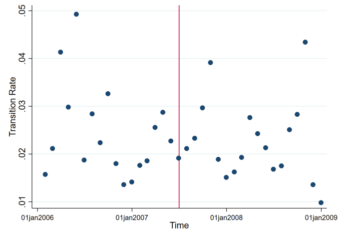
(e) FT bound→Out



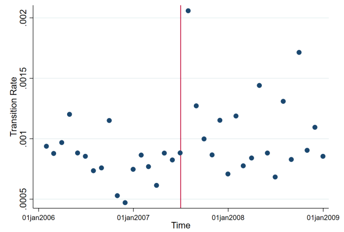
(f) FT above→FT bound



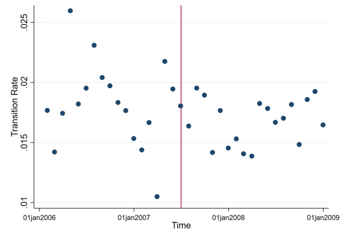
(g) FT above→FT above



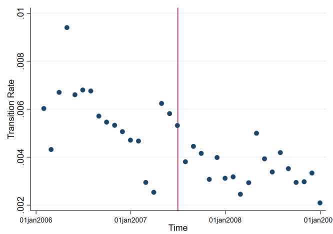
(h) FT above→FT high



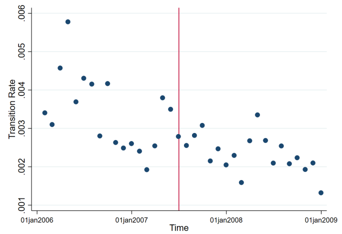
(i) FT above→PT



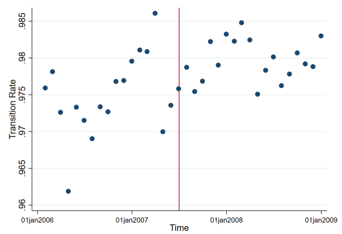
(j) FT above→Out



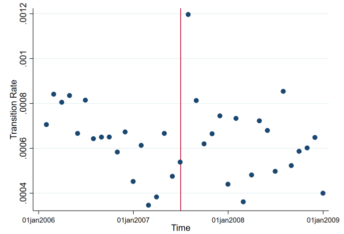
(k) FT high→FT bound



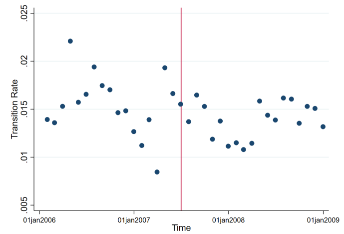
(l) FT high→FT above



(m) FT high→FT high



(n) FT high→PT



(o) FT high→Out

Figure 38: The Transitions of Female Workers in The Group of Directly Affected (Adjusted Sample)

Notes: The figure shows the transition matrix of the workers in the group of directly affected, which is classified by the wage information between Jan. 2004 and April 2004. The transition matrix is composed of five employment statuses: full-time employed with bound wages (FT bound), full-time employed with slightly above wages than the new minimum (FT above), full-time employed with much higher wages (FT high), part-time employed (PT), and out of employment (Out). The percentages show the probability of transiting to a status at time $t+1$ conditional on a status at time t. Every bin is the mean value in the corresponding month. The red vertical line is set on June 2007. Due to the part-time data not being available until 2006, the period shown here is from Jan. 2006 to Dec. 2008.

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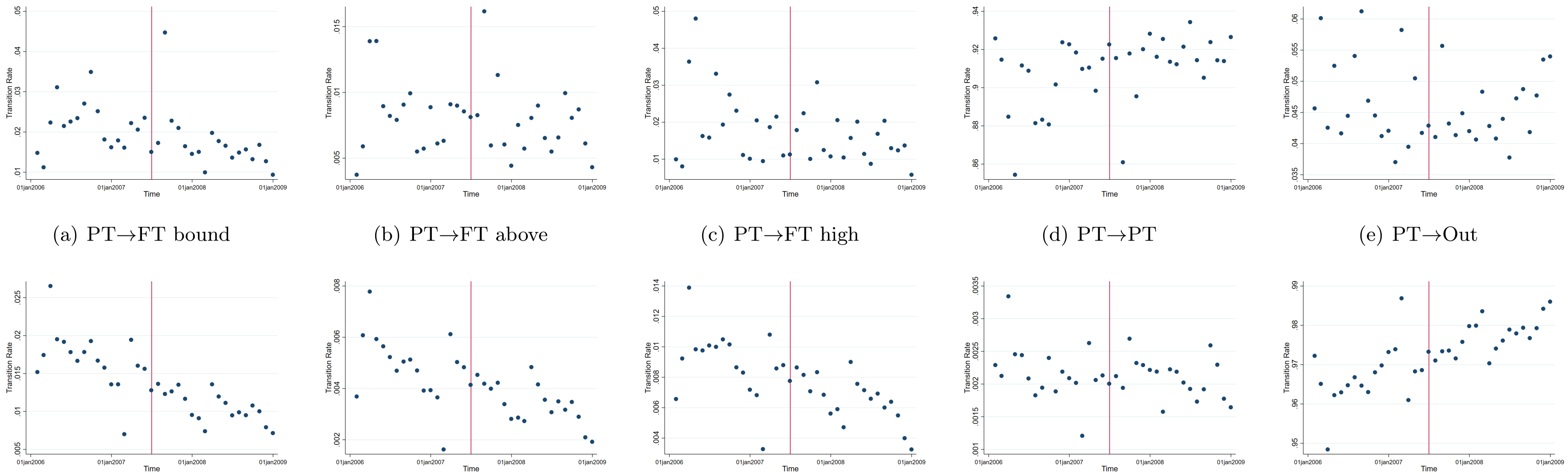

(a) PT→FT bound (b) PT→FT above (c) PT→FT high (d) PT→PT (e) PT→Out

(f) Out→FT bound (g) Out→FT above (h) Out→FT high (i) Out→PT (j) Out→Out

Figure 39: The Transitions of Female Workers in The Group of Directly Affected (Adjusted Sample) (Continued)

Notes: The figure shows the transition matrix of the workers in the group of directly affected, which is classified by the wage information between Jan. 2004 and April 2004. The transition matrix is composed of five employment statuses: full-time employed with bound wages (FT bound), full-time employed with slightly above wages than the new minimum (FT above), full-time employed with much higher wages (FT high), part-time employed (PT), and out of employment (Out). The percentages show the probability of transiting to a status at time $t+1$ conditional on a status at time t. Every bin is the mean value in the corresponding month. The red vertical line is set on June 2007. Due to the part-time data not being available until 2006, the period shown here is from Jan. 2006 to Dec. 2008.

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2.5.5 Estimate of Effects on Occupational Injury

In addition to changing on the workers' employment and compensation, it is possible that firms have some other adjustments to mitigate the minimum wage hike. In a working paper, Hradil (2018) found that workplace safety is reduced because of the minimum wage increase. As the workplace safety would not only affect the workers who are bound by the minimum wage, the safety effect would be another "spillover" effect on the workers who already had higher wages. As shown in Figure 23, although the occupational injury rate is around 0.1 percentage point and fluctuates between different months, it seems to have a level shift after the minimum wage increase. To more accurately know the actual effects, the RD estimate results are shown in Table 24.

Table 24: RD Estimates of Effects on Occupational Injury

	All firms		Small scale firms	
	(1)	(2)	(3)	(4)
Panel A. CER optimal bandwidth estimator				
Occu. injury rate	3.73e-05	4.20e-05	3.33e-05	3.29e-05
	(4.95e-05)	(4.67e-05)	(5.17e-05)	(5.19e-05)
Occu. injury rate (*restricted*)	0.00015	0.00015	0.00015	0.00014
	(0.00011)	(0.00011)	(0.00012)	(0.00012)
Panel B. MER optimal bandwidth estimator				
Occu. injury rate	0.00020***	0.00018***	0.00019***	0.00021***
	(4.10e-05)	(3.84e-05)	(4.08e-05)	(4.25e-05)
Occu. injury rate (*restricted*)	0.00042***	0.00044***	0.00043***	0.00042***
	(8.72e-05)	(8.72e-05)	(9.43e-05)	(9.27e-05)
Control:				
Year & month effects	No	Yes	No	Yes
Mean value before the policy change				
Occu. injury rate	0.00090		0.00091	
Occu. injury rate (*restricted*)	0.00135		0.00139	

Notes: The table reports RD treatment effect estimators. Small scale firms are firms with less than 30 employees. The restricted industries include manufacturing, construction, water supply and remediation activities. The CER bandwidths are around 5 months and the MER bandwidths are around 10 months before and after the cutoff time. Estimators are bias−corrected RD estimates with robust variance under controls for the industry sectors and location. The mean value before the policy change is the mean value in June 2007.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

The results between all firms and the small scale firms are similar. Due to the fluc-

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tuation across months, the estimates under the shorter bandwidth, the CER bandwidth, are not significant. But, no matter whether the specific time effect is controlled or not, the occupational injury rate of firms from all industries is significantly increased by about 0.02 percentage point under the longer bandwidth, the MER bandwidth. Compared to the mean value in June 2007, which is 0.09 percentage point, it is increased by about 23%. Considering the difference between industries, the estimates could be attenuated by some less risky industries. After restricting to the most risky industries (manufacturing, construction, water supply and remediation activities), the estimates become about 0.04 percentage point. Compared to the previous mean value of 0.14 percentage point, it is increased by about 30% after the minimum wage increase. Hence, employers may cut some security cost or force some unsafe production process due to the minimum wage increase.

2.6 Conclusion and Discussion

Different from previous studies using the wage distribution, this study provides the firm-level and individual-level evidences to pin down the minimum wage effect on the earning inequality. Considering all the controversial settings in the minimum wage literature, including the methodology and the studied target, this study proposes a novel approach that uses the regression discontinuity design to estimate effects on firms and specific workers of different wage groups. This approach is based on exploiting the advantage of the monthly personnel administrative data of Taiwan during a seven-year period in which there was only one nationwide minimum wage increase. The key advantage of this approach is that it allows clarification of the employment and wage effects on different kinds of workers at the same time, which are neglected or mixed under the analysis of wage distribution, without assuming any counterfactual. To examine the hypotheses related to the minimum wage impacts, this study implements the proposed approach in a few parts. First, the effects on the employment stocks and average wages in firms are estimated to initially see if the equity effect or the substitution effect exists. Second, looking into the individual level, the employment and wage effects are estimated on the bound and unbound workers classified by the long period information. Third, the effects on the employment and wage gaps between workers with different wage levels are estimated to directly see the effects on inequality. Fourth, he employment flow in firms and the individual transition matrix is used to infer the possible explanation of the estimated results. Finally, as a supplement,

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the effect on the occupational injury is estimated to see if there is another channel of "spillover" effect.

The firm-level estimates show that firms actually have their employment increased after the minimum wage increase, especially a 12% to 24% increase of the part-time employment; although the average nominal wage is increased, the real value goes down. As the minimum wage increases higher than the CPI, the wage decrease can only be from the unbound workers. The opposite employment and wage effects lead the total payments for the full-time employees to stay constant, while the part-time payments increased 14% to 23%. As the hypothesis of the equity effect expects wage spillovers, our findings obviously opposes it. On the other hand, the employment increase is also opposite to the expectation of the substitution effect. Although the finding is striking, the evidence from the individual level confirms it even more. My best estimates show that the employment of the full-time workers bound by the minimum wage is significantly increased by 1% to 4% due to the minimum wage increase, while their real wage is increased by 1% to 3%. On the other hand, the employment of the workers who had wages slightly higher than the new minimum wage is also increased by 3% to 7%, but their real wage is decreased by 6% to 11%. The decrease reflects that they lose their potential nominal wage increase and suffer because of the hike of the CPI rather than having their wages cut. As a combining result of the employment and wage effects, the bound workers have their real earnings increased by 4% to 5% but the unbound workers have no significant change on their real earnings. Hence, there are no spillovers to the slightly higher-wage workers. Furthermore, through the estimate of the effects on the employment and wage differences, we can once again make sure that the wage and earning gaps between these two groups of workers are reduced. In addition, the nominal wage gap is reduced even more than the mechanical contribution from the minimum wage increase, meaning that the unbound workers' nominal wages are lower than they whould be if there was no minimum wage increase.

Obviously, neither the hypothesis of the equity effect nor the substitution effect can explain those findings. Moreover, the hypothesis of search frictions is also rejected by the further analysis. The estimates of the effects on the employment flow show that under the shorter bandwidth, the new hire rate on the full-time employment even has an increase by 39%. No matter what the bandwidth is, the separation rate and the hire rate on the part-time employment are hugely increased by 31% to 35% and 36% to 43% respectively. Those

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results are opposite to the expectation of the search frictions, in which the employment flow should be reduced by the minimum wage increase. Furthermore, the individual transition matrix also shows that the full-time employment growth is not from reduced separation but may be from the higher employment entry, and the high employment flow on the part-time employment can be explained by that the full-time workers move to part-time jobs due to the much higher increase in the hourly minimum wage, but some workers move back to full-time jobs afterwards.

The higher entry to the full-time binding-wage jobs and the part-time jobs should reflect the change on the labor supply rather than the labor demand, since to employers they are more expensive than before. This finding matches the expectation of the traditional monopsony model, in which the labor market is constrained by the upward labor supply, hence higher wages lead to higher employment. However, the higher entry to the full-time unbound jobs along with their declining wages still cannot be explained by the monopsony. As one of the findings is that the firms' total full-time wage payment has no significant change, one possible explanation is that the employer suppresses the higher-wage workers' wages to mitigate the cost pressure; at the same time the employer's demand for the higher-wage worker is still increased because they become relatively cheaper, but the elasticity of labor supply may be huge and hence there is no wage increase even if the labor demand is raised. Another explanation is the upward shift in the higher-wage workers' labor supply, but the reason is unknown. These two possible explanations should be studied more in the future.

From this study, it is clear that there is no spillover effect. Another interesting finding is that there is even a negative "spillover" due to the reduced workplace safety after the minimum wage increase. The estimates show that firms in all the industries have their occupational injury rate raised by 23% compared to the previous rate, and the firms in more risky industries have their occupational injury rate raised by 30%. This effect also applies to workers who are not bound by the minimum wage.

As the findings of this study are different from previous studies, the question is why does it happen. The methodology should be a key source to the answer. Previous studies used wage distributions to estimate. If we used the methodology introduced by Cengiz et al. (2018), we would also find a slight spillover effect as shown in Figure 12. However, the increased frequency on levels higher than the new minimum is not from wage spillovers

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but from higher employment of those kinds of workers, and actually they have a lower wage than they would have had if there was no minimum wage increase. In addition, studies using the density of wage distribution (Dinardo et al., 1996; Lee, 1999) need to assume the medium wages are not affected by the minimum wage increase, but the medium wage may be decreased when the low-wage workers has higher employment; This may lead to a estimated result of the spillover effect on the wage inequality.

Second, as this study is based on Taiwanese data, the difference may just be because of different countries. However, if this is the case, then it means that the minimum wage effect is not universally the same but needs to be estimated on different backgrounds. Therefore, the important question is "what kinds of conditions would cause what kinds of outcomes?". It is beyond the question of "what is the effect of the minimum wage?".

A concern about this study is that the insurance data used in this study may be problematic. It is possible that the employer makes the employee's insured wage lower than their actual wage to save the insurance fee. That is, there may be some workers having high wages but being classified as low-wage workers, so the estimated results would be mixed with others. More specifically, the estimates on the bound workers may include effects on the unbound workers; the estimates on the slightly higher-wage workers may include effects on the workers with much higher wages. As workers with much higher wages should not be affected by the minimum wage increase, the effect on the slightly higher-wage workers would be underestimated.

On the other hand, the wage effects on the bound workers should also be underestimated because the estimate mixes the effect on the slightly higher-wage workers, which is opposite to the estimate. However, it is not clear when it comes to the employment effect, since the estimate has the same direction as the effect on the slightly higher-wage workers. It could be the case that the actual employment effect on the bound workers is negative, but because there is a large proportion of workers actually having slightly higher wages, the estimated result becomes positive. However, this is less likely to happen due to two reasons. First, employers have higher incentive to underreport the employee's insured wage if their wages are higher. If the actual wage of the employee is just slightly higher than the minimum wage, the insurance fee that they can save would be too moderate to prompt the employer to violate the law and ruin the relationship with the employee.[60] Second, even if

[60] The insurance fee for the labor and employment insurance is 6.5% of employees' monthly insured wage, and the employer has to pay 70% of the fee. For example, the total insurance fee for a minimum wage

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every wage level has the same probability to be underreported, workers who are reported as the bound workers are more likely to be from other much higher wage levels, as the slightly higher-wage workers just has a very small part in the wage distribution. Due to the two reasons, the estimate of the employment effect on the bound workers is more likely to be attenuated by the workers with much higher wages. Therefore, even if the data has the problem of wage underreporting, it would just underestimate findings in this study.

worker earning 15,840 NTD per month is 1,030 NTD per month, the employer has to pay 721 NTD; the total insurance fee for a worker with a wage of 18,300 NTD, which is slightly higher than the new minimum, is 1,190 NTD, the employer has to pay 833 NTD. The difference between the two for the employer is only 112 NTD per month, which is less than 1% of the minimum wage.

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