

NBER WORKING PAPER SERIES

WORKER MOBILITY IN A GLOBAL LABOR MARKET:
EVIDENCE FROM THE UNITED ARAB EMIRATES

Suresh Naidu
Yaw Nyarko
Shing-Yi Wang

Working Paper 20388
<http://www.nber.org/papers/w20388>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2014

We thank Thomas Joseph, UAE Exchange, Labor Minister H.E. Saqr Ghobash, Alex Zalami and the UAE Ministry of Labor for help accessing the data sets and learning about the UAE labor market. This paper has benefited from conversations with or comments from Daron Acemoglu, Santosh Anagol, Michael Clemens, Alan de Brauw, Arindrajit Dube, Ann Harrison, Alan Manning, Todd Sorenson, Eric Verhoogen and seminar participants at Barcelona GSE Summer Forum, Berkeley, Boston University, Center for Global Development, CIFAR, ETH-Zurich, LSE, MIT, NBER Summer Institute Labor/Personnel, Warwick, and Wharton. Afshan Aman, Victor Archavski, Michelle Han, Minkwang Jang, Goran Lazarevski, and Stefanie Gude provided excellent research assistance. The authors acknowledge financial support from the New York University in Abu Dhabi Research Institute, the Center for Technology and Economic Development and Wharton Global Initiatives. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2014 by Suresh Naidu, Yaw Nyarko, and Shing-Yi Wang. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Worker Mobility in a Global Labor Market: Evidence from the United Arab Emirates
Suresh Naidu, Yaw Nyarko, and Shing-Yi Wang
NBER Working Paper No. 20388
August 2014
JEL No. J42,J6,O15,O53

ABSTRACT

In 2011, a reform in the United Arab Emirates allowed any employer to renew a migrant's visa upon contract expiration without written permission from the initial employer. We find that the reform increased incumbent migrants' earnings and firm retention of these workers. This occurs despite an increase in employer transitions, and is driven by a fall in country exits. While the outcomes of workers already in the United Arab Emirates improved, our analysis suggests that the reform decreased demand for new migrant workers and lowered their earnings. These results are consistent with a model in which the reform reduces the monopsony power of firms.

Suresh Naidu
Columbia University
420 West 118th Street
New York, NY 10027
and NBER
sn2430@columbia.edu

Shing-Yi Wang
The Wharton School
University of Pennsylvania
3620 Locust Walk
Philadelphia, PA 19104
and NBER
was@wharton.upenn.edu

Yaw Nyarko
Department of Economics
New York University
19 W. 4th Street, 6th Floor
New York, NY 10012
yaw.nyarko@nyu.edu

1 Introduction

Labor market competition and worker mobility are important for the efficient allocation of resources and have major implications for productivity and growth. Among the most important barriers to worker mobility are international borders, which prevent workers from migrating to countries with better institutions and job opportunities. A growing literature considers the effects of international mobility on workers' outcomes.¹ However, much less attention has been paid to the labor market restrictions that migrants face in their destination countries.² Understanding the economic consequences of these restrictions is potentially valuable for policy as restrictions on the labor market mobility of migrant workers exists in virtually all countries (Ruhs 2013). Ruhs (2013) shows that countries that allow the most inflows of international migrants impose stricter restriction, via employer-specific visas, on migrant mobility across employers. Such visa policies, by restricting job-to-job transitions, can result in substantial monopsony power for firms.

This paper examines how relaxation of these restrictions on employer transitions affects the labor market outcomes of migrant workers in the United Arab Emirates (UAE). Prior to the reform, migrant workers in the UAE were under a labor system based on sponsorship by firms, called the *kafala* (sponsorship) system. One component of this system was that each worker was tied to one employer for the duration of their multi-year contracts. Workers had two options for remaining in the UAE when their employment contracts expired. They could renew the contract with their existing employer or they could transition to a new firm only if the existing firm provided a No-Objection Certificate (NOC). If the employer did not renew the contract and did not provide the NOC, the visa system required workers return to their home countries for at least 6 months. In January 2011, the UAE government implemented a policy reform that allowed migrant workers to transition to new employers without approval from their previous employer, but only after their previous contract expired. We examine whether this translates into more competitive labor markets for both workers and employers when contracts are renegotiated. To our knowledge, this is the first paper that provides causal estimates of the impact of a reform of a visa system that ties migrant workers to employers.

While we exploit a reform that occurred in the UAE, the policy debates over the implications of employer-specific visas extend well beyond this particular context. Similar NOC requirements

¹See for example, Clemens 2012 and 2013, Gibson, McKenzie and Stillman 2011, McKenzie, Stillman and Gibson 2010.

²We are aware of one such paper. McKenzie, Theoharides, and Yang (2014) find that labor market distortions, in the form of minimum wage requirements, amplify the effect of output shocks on migrant employment.

are common in most other Gulf countries, where more than 15 million migrants work. In the United States, H2-B visas, for example, also tie low-skilled seasonal workers to particular employers, and do not allow immediate job-to-job transitions after a contract expires. Immigration reformers have asked for a clause allowing a 6-month interval whereby workers can search for a new employer without having to return to their source country. These types of visas are often criticized by advocates for restraining labor market competition, lowering migrant wages, and facilitating endemic labor rights violations.³

This paper addresses the question of how increasing labor market competition affects workers outcomes. The visa reform in the UAE provides a unique source of exogenous variation in the monopsony power of firms vis-a-vis workers. We present a simple model of monopsony power with two sources of labor. Firms in the UAE not only face a within-country labor market for incumbent migrants but also have the option of hiring from the pool of potential migrants from other countries. The model demonstrates that increasing labor market competition will lead to higher wages *and* higher employment for incumbent migrants. This combination of increased wages and increased employment for incumbent migrants is a distinctive signature of reducing the market power of firms. By introducing the potential of hiring outside of the country, the model also shows that labor demand for new entrants to the UAE falls leading to fewer new entrants and lower earnings for them. Thus, the model emphasizes a trade-off between ex-ante openness to migrant labor and ex-post restrictions on worker mobility.

Since Robinson (1933), applying the economic analysis of imperfect competition to the labor market has been an important thread in economics. The recent literature on imperfect competition in labor markets is summarized in Manning (2011). Some of the common approaches in this literature differ substantially from our approach, and results vary widely across studies. For example, Falch (2010) and Staiger, Spetz, and Phibbs (2010) use wage regulations to measure monopsony power by looking at the impact of changes in wages on employment. Similarly, Matsudaira (2014) uses regulations stipulating minimum employment levels for nurses as exogenous change in employment to measure monopsony power through the accompanying change in wages. Isen (2013) uses employee deaths at small U.S. firms to estimate gaps between marginal products and wages. The bulk of the literature examines formal labor markets in advanced economies, yet the importance of job mobility and labor market competition in developing countries and immigrant labor markets is likely even greater, given lack of formal information sharing or institutionalized wage setting.

³e.g. <http://www.epi.org/publication/2b-employers-congressional-allies-fighting>

The labor restrictions in the UAE can also shed light on similar institutions of indentured contracts and bonded labor. Historically, restrictive labor market contracts were commonplace for indentured migrant workers (Galenson 1984, Abramitzky and Braggion 2006) and existed in domestic labor markets (Naidu and Yuchtman 2013, Naidu 2010). More recently in developing countries, bonded labor arrangements, where workers are tied to particular employers for long periods of time, have been studied both theoretically and empirically (Bardhan 1983, von Lilienfeld-Toal and Mookherjee 2010). Similar arrangements have persisted in modern labor markets in developed countries. For example, restrictions on mobility have been studied in the context of professional sports (Scully 1974). A recent literature on non-compete clauses in labor contracts in the U.S. is also related (Marx 2011). Non-compete clauses restrict the ability of employees to work for firms that compete in the same sector, and have become more frequently used in recent years, particularly in high-tech, high-skill sectors with substantial firm-specific knowledge.

Our primary empirical strategy uses the timing of the reform together with individual-level variation in the expiration dates of labor contracts to estimate the impact of the easing of mobility restrictions on earnings, firm retention, country exits and employer transitions of incumbent workers in the UAE. This approach exploits the fact that the benefits of the reform only applies to workers after their contract expires post-reform. Standard contracts were uniformly three years in length, so the timing of individuals' contract expirations is likely to be exogenous to the timing of the reform and to other contemporaneous labor market conditions.

Given that we are also interested in the effects of the reform on potential migrants, we present a different empirical strategy to look at the impact of the reform on new workers entering the UAE. We combine variation in the number of contracts that are expiring at a firm with a before and after reform comparison. This approach uses the idea that firms with more contracts expiring after the reform experience a greater impact of the reform. This allows us to examine how the reform affects the number of new entrants from other countries are hired by firms and the initial earnings paid to new entrants.

To implement these empirical strategies, we match two high quality administrative data sets. The first data set is UAE Ministry of Labor data on the terms of the contracts signed between workers and firms. The second data set is from a large, private payroll processing firm that provides monthly payroll disbursement for migrant workers employed at thousands of firms in the UAE. The administrative payroll data minimizes measurement error in earnings. Moreover, the monthly frequency of the data allows us to take an event-study level approach and examine a tight window of outcomes around the month of a worker's contract expiration.

Our results indicate that the outcomes of incumbent workers in the UAE improve substantially following a contract expiration that occurs after the reform. Real earnings following a contract expiration increase by over 10 percent. Consistent with imperfect competition in the labor market, we observe that labor-supply to the firm, measured as the retention rate, increases for workers experiencing a contract expiration following the reform. In addition, the monthly rate of employer transitions at the end of a contract more than doubles. The monthly probability of leaving the UAE at the end of a contract also falls by about 4 percentage points. The results are similar with inclusion of a variety of controls and to restricting the data to various sub-samples. They also remain robust to implementing a bounding method to address concerns about selective exits out of the UAE. Finally, the results are robust to a falsification exercise where we shift the timing of individuals' contract expirations to rule out that unobserved trends in contract time explain the results.

The reform led firms to hire fewer new entrants to the UAE and to reduce the initial salaries of those workers. We exploit the panel nature of the data to show that there is some evidence that firms anticipate upcoming contract expirations and adjust their margin of hiring new entrants in the months prior to the actual realization of the contract expirations. Both the earnings and the quantities results are robust to the inclusion of controls to address time-varying changes though the quantities results are more sensitive to analyzing various sub-samples.

We use the estimates from regression results to recover the degree of market power that firms had over both incumbent migrants in the UAE and the pool of potential migrants. Our estimates suggest that firms have market power over both the pool of potential migrants and incumbent migrants already working at their firm in the UAE. Firms' monopsony power allows them to pay both new entrants and incumbents roughly half of their marginal product of their pre-reform marginal product. By increasing the labor-supply elasticity facing the firm, the reform increases the share of the marginal product paid to incumbent workers to 60%.

Finally, this research is also related to the literature on search models that emphasize job-to-job transitions as a key determinant of wages and employment in contemporary labor markets (Burdett and Mortensen 1998). For example, Hornstein, Krusell, and Violante (2011), Manning (2003), and Cahuc, Postel-Vinay and Robin (2006), while methodologically very diverse, all suggest that offers received while on the job are important for explaining wage variation.⁴ However, despite the strong predictions made by economic theory, credible well-identified estimates of the effects of facilitating labor mobility on individual labor market outcomes are lacking.

⁴See Rogerson and Shimer (2005) for a more complete review.

2 Institutional Background

The UAE, with an 89% migrant share of population, is an interesting context to study policy questions related to migration. Migration into the Gulf region in general increased substantially in the past decades. In the UAE specifically, the number of migrants jumped from 1.3 million in 1990 to 7.8 million in 2013 (UN 2013). Accompanying the surge in migrant flows to the area, there has been a great deal of international concern about the power that employers have over migrant workers. Human Rights Watch (2013) illustrates this concern in writing, based on anecdotal evidence, “Migrant workers in these countries typically have their passports confiscated and are forced to work under the highly exploitative *kafala* system of sponsorship-based employment, which prevents them from leaving employers. Employers are rarely, if ever, prosecuted for violations of labor law. As a result, migrant workers in the Gulf frequently experience hazardous working conditions, long hours, unpaid wages, and cramped and unsanitary housing.” However, there is little quantitative evidence on migrant labor market conditions in these countries, nor have there been any attempts to evaluate the impact of policy reforms that have been proposed and undertaken in Gulf countries in recent years.

Migrant workers make up 96% of the workforce in the United Arab Emirates (Forstenlechner and Rutledge 2011). Employers in the UAE recruit workers from around the world with the bulk of the workers from South Asia. Migrants are recruited through source country labor brokers, specialized UAE-based recruiting firms and by UAE firms directly. A signed contract and a passport (a non-trivial requirement in some source countries) are required to obtain a visa. Formally, employers and their contractors are forbidden from charging recruitment fees to workers, but it is unclear if this is enforced. Employers generally cover lodging, health insurance, and travel costs (conditional on contract fulfillment). Workers are entitled to 1 month of leave per year, and many wait several years to take 2 to 3 months contiguously. Workers are housed in large labor camps, which often span multiple employers. Employers pay fixed fees to the government for labor cards for each migrant worker under contract, which cover the cost of catching and deporting workers should they abscond from their job. Fees depend on the composition of the workforce of the firm, with skill-intensive and high local-emirati employment firms able to purchase cheaper labor cards. The government regulates contract lengths by the types of visas granted. Before 2011, standard contracts were three years long; since 2011, this was shortened to two years.

The contracts and visas are regulated under the *kafala* system, which is widely used in the Gulf countries (Longva 1999). Traditionally under this system, guarantors were used to enforce

contracts where the individual guarantor (*kafeel*) was liable for the credit, safety, and good conduct of the debtor (*kafila*). In modern Gulf countries, this has become an elaborate set of regulations on migrant labor, tethering workers to their employers via contracts and visas, and giving employers a substantial amount of power.⁵

Under the pre-2011 system, workers fired by their employers promptly lost their visa status and were required by law to leave the country soon after the employer terminated the contract.⁶ Workers had the right, however, to appeal the firm's decision to fire them to the government under certain circumstances, such as if wages were owed. If workers wished to end their contract early, they had to leave immediately and bear the travel costs, which would otherwise be borne by the company.

Most importantly, under the pre-2011 system, workers needed a No-Objection Certificate (NOC) from their existing employer in order to change employers either during an existing contract or after the contract expired. Anecdotal evidence suggests that some employers required workers to pay substantial fees in exchange for the NOC. Without an NOC at contract expiration, workers were subject to a visa ban and had to either return to their source country for at least 6 months before re-entering or renew with their current employer. This feature of the *kafala* system has fallen under widespread criticism. In Salem (2010a), a worker's statement illustrates some issues related to the NOC requirement: "At the beginning, when I gave my one-month notice to move to another job, my boss said OK, but at the end of the month he said no, he needs me, it is not his problem I didn't want to continue in that job."

Evidence that these restrictions are binding can also be seen from online forums where expatriate workers trade advice for dealing with visa issues in the UAE.⁷ Numerous posts are from workers asking for legal advice and complaining about the 6-month and other bans imposed if a worker leaves a contract without an NOC. For example, one user with the screen name "Exchange job" wrote in January 2011, "I am working in an exchange for three months. My salary is very low. Now I want to switch the job but my contract period is of three years. I also want to pay the ban charges if there is a ban. kindly guide me if it will be possible for me to change the job and as well as to pay the ban fee." Similarly, "Jahangir" wrote (typos in original) "Respected Sir, I ma very new in uae - dubai my comapny head office is in dubai and having one branch in ksa [Saudi Arabia] and i was appointed for ksa but company want to stay

⁵One ethnography suggests that "Migrants ..assume that any Emirati is capable of deporting any migrant should they choose." (Bristol-Rhys 2012, pg 68).

⁶While the numbers of migrants in the UAE without a valid visa is unknown, it is thought to be quite small (around 5% of the total population) as police will regularly stop workers and ask them for their papers.

⁷For example: <http://www.dubaiforums.com/dubai-visa/> or <http://www.desertspeak.com/>

in dubai on same salary and i already resign my past job, and write now my company makes my work permite but i don,t want to work with this on same salary in dubai so let me know what r the way to change the job in uae.”⁸ While it is difficult to validate the anecdotal evidence from the Internet, it does suggest that the legal contract restrictions are enforced and are seen as a constraint by workers.

These kinds of restrictions are not new. British Master and Servant law governed migrant indentured labor contracts throughout the Empire. The Gulf countries, then known as the Trucial states, were recipients of Indian migrant labor under this system beginning in the early 20th century. No-objection cards were issued by the British Political Agent to merchants in the Gulf as early as the 1930s (Seccombe and Lawless 1986). While the increase in migrant labor has been recent, the institutional foundation for the NOC system was laid well before formal codification in the 1970s.

2.1 Labor Mobility Reform

Discussions of reforming the NOC requirements in the UAE followed when neighboring Bahrain reformed a similar requirement in August 2009. The UAE government formally announced the reform in December 2010 and it took effect in January 2011 (Cabinet Resolution number 25 of 2010).⁹ The UAE Minister of Labour, Saqr Ghobash, stated that the change was intended to “improve the labour market and... protect the rights and benefits of the labourers as well as their employers” (Salem, 2010a).

The reform had a number of components. Most important for this paper is the reform that abolished the NOC requirement when a contract expired. Starting in January 2011, workers could directly switch employers without the NOC from their previous employers *after* their current contracts had expired. This change in mobility requirements only applied at the end of contracts; while in an existing contract, workers still needed an NOC to change employers without exiting the country for 6 months. Other components of the reform included some changes to visa fees for skilled workers, a shortening of the duration of standard contracts from 3 years to 2 years, as well as a lowering of the age of eligible workers from 65 to 60. The change in the duration of contracts only applied to new contracts beginning in or after January 2011 and did not shorten existing 3 year contracts.

Officials acknowledged the implications of the reform for labor market competition in the

⁸<http://www.desertspeak.com/viewtopic.php?t=1911> Accessed Feb. 4 2014.

⁹Our research has not found other major policy changes in the UAE in January 2011. Furthermore, the results presented for India and all other home countries in Table 6 indicate the results cannot be driven by a policy change in a single origin country.

UAE, with Minister Ghobash saying, “Giving the private sector more freedom of movement will have automatic impact on employers by the way of preserving their interests through creating many options for recruiting skillful workers as per the supply-demand equation... These measures [are] expected to play a major role in advancing efforts towards creating an efficient labour market and sharpening competitiveness and transformation towards a knowledge-driven economy” (WAM 2010). News reports also suggest that employers understood the incidence of the law, with complaints such as “We used to have control over them [migrant workers], and we knew it wasn’t easy from them to go, now we will lose this control.” (Salem 2010b).

3 A Framework for Labor Market Power

This section offers a framework for understanding the impact of increasing the labor market competition within the internal labor market in a context where firms have the option of recruiting and hiring from an external labor market. Given the large wage differences between the UAE and many other countries, it is not surprising that there is a large supply of foreign workers who are willing to migrate to and work in the UAE. One possible implication of the large supply of foreign workers with very low reservation wages is that firms do not need to respond to labor market regulations that govern within-country employer transitions; firms may simply replace workers with new entrants instead of responding to increases in within-country labor market competition. Our framework demonstrates this intuition may not hold.

While the labor market in the UAE is not classically monopsonistic given that several firms exist in the same geographic area and prior to the reform, and employer transitions occurred albeit rather rarely, we will use the term monopsony here to refer to labor market power, and not commit to any particular model of imperfect competition. We begin by defining a standard production function for a firm as $F(L_I, L_r)$ where incumbent workers already employed by the firm are denoted L_I and new recruits from source countries L_r .¹⁰ We further suppose that the number of incumbent workers is taken as given by the firm but the firm can choose how many of these workers to retain.¹¹ The retention, or staying, rate of incumbent workers is given by $s(w_c)$, so $L_I = s(w_c)L_c$. Similarly, we suppose that the firm faces a general upward-sloping

¹⁰Note that new recruits are drawn from the pool of potential recruits.

¹¹The intuition of this modeling choice is that the number of worker under contract in a period is not a current choice but determined by past hiring decisions.

wage function for new migrants $L_r(w_r)$.¹² Thus a firm's profits are given by:

$$\Pi(w_r, w_c | L_c) = \max_{w_c, w_r} F(s(w_c)L_c, L_r(w_r)) - w_c s(w_c)L_c - (w_r + v_r)L_r(w_r) \quad (1)$$

where $v_r > 0$ is the non-wage cost of recruiting and hiring a new entrant.

If we denote $\eta_j \equiv \frac{\epsilon_j(w_j)}{1 + \epsilon_j(w_j)}$, where ϵ_j is the labor-supply elasticity for labor type $j \in \{r, c\}$, we have the following first-order conditions:

$$F_{L_c}(s(w_c)L_c, L_r) = \frac{w_c}{\eta_c} \quad (2)$$

$$F_{L_r}(s(w_c)L_c, L_r) = \left(\frac{w_r}{\eta_r} + v_r \right). \quad (3)$$

As the market for labor of type j becomes perfectly competitive, then $\epsilon_j \rightarrow \infty$ and $\eta_j \rightarrow 1$. If labor markets are fully competitive and $\eta_j = 1$, marginal products are equal to wages (plus any recruitment costs). However, if the market is monopsonistic and $\eta_j < 1$, then marginal products are higher than wages, and employment is below its optimal level.

The reform corresponds with an increase in the elasticity of labor-supply facing the firm for L_c workers, i.e. an increase in η_c . In other words, the reform effectively increases the competition for workers already in the country.¹³ The distinctive monopsony prediction is that as market power falls, quantities *increase* even as wages also *increase*. This is because market power (together with an inability to wage-discriminate) gives firms an incentive to lower employment below the optimal level in order to reduce the wage paid.¹⁴

An increase in the number of incumbent workers retained will also change the marginal product of new recruits, as $\frac{dL_r}{d\eta_c} = \frac{dL_c}{d\eta_c} \frac{F_{L_r L_c}}{-F_{L_r L_r} + \frac{dw_r}{dL_r} \frac{1}{\eta_r}}$. The sign of this depends on whether new recruits and incumbent migrants are complements or substitutes. Given the homogeneous nature of the tasks in many of these jobs, it is plausible to assume they are substitutes. Thus an increase in η_c will result in a *decrease* in the number of new recruits hired. If employers also have market power over new recruits ($\eta_r < 1$), then this will imply that wages for new recruits

¹²Both $s(w_c)$ and $L_r(w_r)$ labor-supply functions should be understood as residual supply curves, and implicitly depend on the wages of other firms in the labor market.

¹³While we focus on the pure monopsony case for exposition, Weyl and Fabinger (2013) show that a wide variety of models of symmetric imperfect competition yield a generalized Lerner condition for oligopsonistic markets. In our context, this condition is: $\frac{F_{L_j} - w_j}{w_j} = \frac{\theta}{\epsilon_j^M}$, where ϵ_j^M is the market supply elasticity of type j labor and θ is a ‘‘conduct parameter’’ that summarizes the strategic interactions among firms. $\theta = 1$ yields classical monopsony, where the market labor-supply elasticity is also the firm labor-supply elasticity, while $\theta = 0$ corresponds to perfect competition.

¹⁴This prediction reflects Bresnahan's (1982) argument on identifying market power. Bresnahan argued that exogenous variables that changed the elasticity (i.e. the slopes) but did not affect the level of demand or supply should have no effect in competitive markets, but should alter prices and quantities in markets with oligopsonistic power.

will also *decrease*. This is in contrast to the predictions that would be obtained if the market were perfectly competitive, and the reform was thought of as an exogenous increase in the wage w_c (due to an increase in the ability of firms and workers to match); in this case, we would see employers decrease their use of retained workers and increase their demand for new recruits, increasing both quantities hired and wages paid.

To summarize, the model of labor market power predicts that the quantity and wages of incumbent workers will rise as a result of the reform. At the individual level, the quantity prediction implies that incumbent workers will be more likely to remain with their existing firms despite the increased ability to change firms. Thus, an additional prediction is that workers are more likely to remain in the UAE. Unlike for incumbent workers, the model predicts that the quantity and wages of new entrants to the UAE will both fall. Intuitively, the differences in the outcomes for incumbent workers and new entrants reflects the fact that labor market competition has been reduced for incumbent workers only but these two types of workers are substitutes.

4 Data

4.1 Salary Disbursal Data

The data on wage disbursements of migrant workers are from a company in the UAE called UAE Exchange. The company provides payroll disbursement services to other firms in addition to offering other financial transactions such as remittances. This firm pays wages to approximately 10-15% of the total migrant workforce in the country. Many firms, including quite large and small ones, use a payroll processing firm in order to adhere to the wage protection system, which was implemented by the government in 2009 and 2010 (with larger firms required to implement the system earlier) to protect workers by creating electronic records of wage payments.

We have monthly payroll disbursements for the period from January 2009 to October 2012. Recall that the reform went into effect in January 2011, so the data span both sides of the reform. The entire sample of earnings disbursements includes 427,265 unique individuals working in 20,366 firms. In the UAE, salaries are paid out on a monthly basis.¹⁵ There are on average 17.6 monthly salary observations per worker. The key advantage of the data is that they are high-frequency administrative records of actual earnings transferred to workers, and should not suffer from issues of recall error that are common in survey-based questions on earnings.

¹⁵In the less than 5% of observations for which multiple payments are made to an individual in a single month, we aggregate those into the total earned in that month.

The observed earnings may differ from total compensation for several reasons. First, many migrant workers are compensated with several in-kind benefits, including housing and food. Second, workers may supplement their earnings in their primary jobs with informal work. This is unlikely to be as common in the UAE as in other contexts because it is illegal for migrant workers to receive compensation for work outside of the one employer associated with their visas.

Because the data are from administrative payroll processing records, other information available for each worker is limited. The data include firm identifiers and some demographic characteristics of workers, including their country of origin, age and gender. It is important to note that the data set does not include any information on actual hours worked in each month.

4.2 Ministry of Labor Administrative Contracts Data

In addition to the salary disbursement data, we also received data on migrant workers' labor contracts from the UAE Ministry of Labor (MOL). Two key variables in this data set are the start and end dates of the labor contract signed between a migrant worker and a firm. This allows us to identify the exact month that a worker's labor contract will expire. Furthermore, the MOL data allows us to link individuals in the UAE Exchange payroll data as they move across firms. Not all firms in the UAE use UAE Exchange for payroll processing so we do not observe all firm-to-firm transitions of workers in the UAE Exchange data alone.

Thus, a key benefit of the MOL data is that it allows us to identify whether a worker that disappears from the UAE Exchange dataset switches to another firm that does not use UAE Exchange for payroll processing or leaves the MOL data entirely.¹⁶ We characterize those migrants who leave the MOL data as having exited the UAE, and this is true in the vast majority of cases. However, a fraction of migrant workers who leave the MOL data remain in the UAE. This reflects the fact that the MOL data only includes migrant labor contracts that fall under the jurisdiction of the Ministry of Labor. Domestic workers fall under the jurisdiction of the Ministry of the Interior, as do any workers that work in freezone areas of the UAE.¹⁷ A comparison of the MOL data to UN population figures for migrant workers in the UAE in 2012 suggests that the MOL data covers approximately 80% of all migrant workers in the country.

In addition to the start and end dates of contracts, the MOL data also includes other details of each labor contract, including contracted hours, earnings, and total compensation.¹⁸ It would

¹⁶However, a limitation of the data is that we cannot distinguish voluntary worker separations (quits) from involuntary separations.

¹⁷Freezones are industrial parks throughout the UAE that provide special incentives for foreign investments, such as tax breaks and less restrictions on foreign ownership. The bulk of the freezones are in the vicinity of the cities of Dubai and Sharjah.

¹⁸Total compensation includes the value specified in the contract for housing, food and transportation.

be inaccurate to assume that contracted earnings are equivalent to actual earnings; a comparison of the MOL data and the payroll data suggests that the contracted earnings are a lower-bound on workers' earnings. Most workers earn more than what is stated in their contract and the amount fluctuates considerably from month to month. The data set also contains all of the demographic characteristics included in the UAE Exchange data as well as some additional ones such as religion and educational attainment.

Both the MOL contracts and the UAE Exchange payroll data sets include a unique government-issued identifier that is called the labor card ID number. Thus, the matching between the two data sets is straightforward and outlined in more detail in Appendix A.1. We are able to match 81% of the observations in the payroll data with their corresponding contracts in the MOL data, and Appendix Figure A.1 shows that the earnings densities between the matched and unmatched payroll observations are virtually identical. Appendix Figure A.2 shows the densities in the contract salary for individuals who match with the payroll data and the rest of the MOL sample that is not in the payroll data. The distribution is extremely similar for the lower end of the distribution and the comparison suggests that the payroll data is more oriented towards the median and lower end of the salary distribution of migrants and under-samples migrant workers at the top end of the salary distribution.

5 Descriptive Statistics

5.1 Administrative Contracts Data

We begin by showing summary statistics from the MOL contracts data, which provide the universe of labor contracts under the jurisdiction of the MOL. Figure 1 shows the real change in the compensation stipulated in the new contract compared to the previous contract by the expiration date of the previous contract. Compensation includes both earnings and the value of employer-provided housing and transportation. This includes both employer transitions and individuals who re-sign contracts with their previous employers. The vertical line indicates December 2010, the date that the reform was announced, which is also the month immediately prior to the implementation of the reform. We see a substantial increase the growth rate of compensation following the reform.¹⁹

In Figure 2, we show the total number of workers who re-sign contracts with their previous employer by the expiration date of the contract. We see an increase in the number of workers

¹⁹Note that the negative gains in compensation that are observed prior to the reform are driven by the adjustment for inflation. The nominal changes in compensation over the full period shown are positive.

that are retained by their existing employers after the reform.²⁰ This is consistent with workers being more likely to remain within the UAE after the reform.²¹ Figure 3 shows the total number of employer transitions that occur at the end of a contract by the expiration date of the contract. This figure provides immediate evidence that employer transitions increased following the reform.

All three of the figures provide suggestive evidence that the reform had an immediate effect on earnings, retention, and transitions for individuals whose contracts were expiring around the time of the reform. Furthermore, the magnitude of the effects are quite large.

5.2 Salary Disbursal and Administrative Contracts Merged Data

Table 1 presents the summary statistics for the sample used in our estimation. The first three columns show the mean, standard deviation and number of observations for the person-month for the months between January 2009 to December 2010. The last three columns display the same statistics for the post-reform period of January 2011 to October 2012.

The first four rows present our main outcomes of interest. Log monthly earnings is the logarithm of the real monthly earnings disbursal that the worker received.²² The average log earnings is a little over 7; this corresponds to about 1100 dirham or USD\$300. This is the actual earnings disbursal reported by the payroll-processing firm and does not include the value of in-kind benefits. A simple pre-post comparison shows a small increase in average real earnings following the reform.

Stay with firm is a time-varying variable that is 100 if the individual stays with the same firm as in the previous period, and zero otherwise. In other words, the variable equals zero if the individual either changes firms or exits the UAE. The vast majority of individuals stay with the same firm month-to-month. About 95% of individuals stay with the same firm in the months observed prior to the reform, and this increases slightly to 96% after the reform.

Individuals who do not stay with their existing firm either exit the UAE or switch employers. Exit UAE is an indicator variable that equals 100 if the individual leaves the sample for at least 6 months, and zero otherwise. There is some noise in this measure as individuals may move

²⁰Figures 2 and 3 include only employer stays and transitions that occur within three months of the contract expiration to account for the possibility that workers return to their home countries for a 1 or 2 month visit before beginning their new positions. The slight leads and lags in the response is a result of this, and disappears when we use only immediate transitions (available on request).

²¹The MOL data do not indicate when migrants leave the UAE so we do not present a corresponding figure with country exits.

²²We convert nominal earnings to real earnings using the monthly consumer price index published by the UAE National Bureau of Statistics. Earnings are in 2007 dirham.

within the UAE but out of the jurisdiction of the MOL to a freezone area and would be counted as exiting the UAE. The rate of exiting prior to the reform was 4.8% per month; after the reform, this falls to 3.3% per month.

Employer change equals 100 if the individual changed firms within the past 3 months, and zero otherwise.²³ Prior to the reform, the rates of employer change are quite low at 0.2% per month (or 2.4% per year), which translates to only 2 workers per thousand who changed employers each month. This low rate should not be that surprising in the pre-reform period given that workers could not freely change employers either during or at the end of a contract. The average rate of employer change more than triples after the reform.

Stayer is a time-invariant indicator that is defined as an individual who does not change employers at all during the sample period. The vast majority of workers do not change employers at all during the sample period. The majority of the workers in our sample work in construction.²⁴ The average age of workers is mid-thirties and the vast majority of them are male. Educated is an indicator variable that equals one if the person has higher than intermediate education. The pool of educated workers increases substantially after the reform. About 40% of the workers are Muslim. Over 60% of the migrant workers in our sample work in the neighboring cities of Dubai and Sharjah. Indians represent the largest nationality among migrants in the UAE and are about half of our sample.

The summary statistics demonstrate some sizable changes in the composition of worker characteristics over time. This may be driven by changes in the selection of individuals into or out of the country over time. We address the concern that the results may be driven by changes in the characteristics of workers in two ways. First, we allow for time-varying effects of observable worker characteristics. However, there may also be changes in characteristics that we cannot observe. In section 6.5, we also implement a bounding exercise that tests whether the results are robust to maximizing the impact of selection on the estimates.

We do not directly observe hours worked per month in either of the data sets. However, we do observe actual earnings disbursements each month and the earnings and hours stipulated in the contract. We construct two measures of hours worked each month based on the assumption that variation in earnings month-to-month is a reflection of variation in hours.²⁵ The upper

²³See the Data Appendix for more information on the construction of this and other variables.

²⁴Each firm is coded by at least two research assistants. The coding is based on the name of the firm if it contains explicit industry information or by researching the firm. If the two research assistants coded the firm differently, then another coding was done by a third research assistant. We thank Marton Pono, Mengxing Lin, Zhiwen Xie and Cheng Xu for their assistance in industry coding.

²⁵That hours was the primary source of earnings variation was confirmed by conversations with UAE Exchange officials.

bound of hours worked per month is constructed based on the assumption that overtime pay equals 1.25 times the standard hourly wage and the lower bound calculation of hours worked assumes that overtime is paid at a rate of 1.5 times the standard hourly wage.²⁶ The average number of hours worked per month falls from around 260 in the pre-reform period to 240 in the post-reform period.

6 Estimation Strategy and Results

6.1 Overview of Strategy

The estimation strategy for the analysis of the effects of the reform on incumbent workers is analogous to a differences-in-differences framework. We examine worker outcomes before and after the implementation of the reform in January 2011. The other comparison that we exploit is looking at outcomes before and after the worker’s contract expires.

Given that we have less than three full years of data on salary disbursement and that the standard length for contracts beginning prior to 2011 was 3 years, we have essentially no individuals who have contracts expiring both before and after the implementation of the reform. Thus, we might think of individuals whose contracts expire after the reform as our treatment group and individuals whose contracts expire before the reform as our comparison group.

One concern is that the types of individuals entering the UAE changes over time, and the pool of individuals with contracts expiring prior to the reform is different from the pool of individuals with contracts expiring after the reform. However, it is important to note that any changes in the selection of individuals cannot be driven by an endogenous response to the reform itself. This is because individuals whose contracts expired in 2010 versus in 2011 have contracts that began in 2007 and 2008, respectively, and this precedes serious discussion of such reforms in the UAE.²⁷ Our specification also includes individual fixed effects which should remove any time-invariant differences across the groups.

Our analysis focuses on 7 periods per individual. We look at the three months leading up to an individual’s contract expiration, the period of the contract expiration, and the three months following the initial contract expiration. Whether the month of contract expiration itself can be considered post-contract expiration varies by individual because a person’s contract may expire at the beginning or end of a month and he may or may not have the opportunity to transition

²⁶UAE law stipulates rates of overtime between 1.25 to 1.5 depending on the time of the day and day of the week when the extra hours takes place.

²⁷Using the MOL data, Appendix Figure A.3 shows that there is no break in either average contract compensation or the number of new contracts three years prior to the announcement of the reform.

within the expiration month itself. There are a few reasons that we adopt a strategy of using 3 leads and lags around the time of the contract expiration. First, it allows us to examine whether there are pre-expiration trends that suggest that the date of contract expiration is not a clean event. Second, the three lags following the contract expiration can be important as many individuals return to their home countries for vacations of 1 to 2 months following a contract expiration.²⁸ Thus, any post-contract expiration effects may not show up in just one month.

6.2 Empirical Specifications

Corresponding to the strategy described above, we begin our analysis with the following specification:

$$y_{it} = \sum_{k=-3}^3 \gamma_k^{Post2011} D_{it+k} + \sum_{k=-3}^3 \gamma_k^{Pre2011} D_{it+k} + \delta_i + \delta_t + \epsilon_{it} \quad (4)$$

where y_{it} denotes the outcomes of interest for incumbent workers: log earnings, staying with the firm, exiting the UAE and employer transitions. The variable D is a dummy variable that indicates the period relative to the contract expiration date. The sample is restricted to the 7 contiguous months centered around a contract expiration, so $k = -3$ corresponds to 3 periods before the contract expires and $k = 3$ corresponds to 3 periods after the previous contract expired. Thus, the coefficient $\gamma_k^{Pre2011}$ provides the effect of the contract expiration prior to the 2011 reform, and the coefficient $\gamma_k^{Post2011}$ provides the effect of the contract expiration after the 2011 reform. We are most interested in whether the effects of contract expirations after the reform are different from the effects prior to the reform, and that is given by the estimates of $\gamma^{Post2011} - \gamma^{Pre2011}$. We also include year-month fixed effects and individual fixed effects. The standard errors are robust and clustered by individual.

6.3 Graphical Representation of Estimates

Given the large number of coefficients, we show graphical plots of $\gamma_k^{Post2011}$ and $\gamma_k^{Pre2011}$ from estimates of equation 4. Figure 4 displays the coefficients together with 95% confidence intervals when the dependent variable is log earnings. The omitted category is the month immediately prior to the contract expiration ($k = -1$). The bold line refers to the post reform coefficient ($\gamma^{Post2011}$) while the other line presents the pre-reform coefficient ($\gamma^{Pre2011}$). The figure shows that prior to the reform, log earnings did not increase following a contract expiration. This may not be surprising given that in this period, employers had the right to retain workers by not providing a NOC. In contrast, after the reform, we see a significant increase in log earnings that

²⁸This is true regardless of whether they change employers or not.

begins immediately after the contract expires. In addition, there are no significant post-reform effects in the periods prior to the expiration.

Figure 5 presents the estimates where the dependent variable is whether the individual stays with the same firm. Prior to the reform, individuals are less likely to remain at a firm after a contract expiration relative to before the expiration. After the reform, individuals are significantly more likely than before the reform to be retained by their firm following a contract expiration. These individual-level results on the probability of incumbent workers staying at their existing firms correspond with the measure of worker quantities in the model.

Figure 6 shows the same estimates where the dependent variable is whether the individual exits the UAE. Consistent with the limited options available to individuals prior to the reform, we see an increase in exits following a contract expiration on average, but this effect is significantly attenuated following the reform. This suggests that workers were less likely to return to their home countries following the expiration of their contracts after the reform than before. These results suggest that workers are better off in the UAE with the presence of additional work opportunities and/or higher wages.

Finally, Figure 7 shows the coefficients from equation 4 where the dependent variable is employer transitions. In both the pre-reform and post-reform period, the pre-contract expiration trends show no employer transitions in the three months prior to the contract expiration. There is a significant increase in the probability of employer transitions in the pre-reform period. In the post-reform period, there is a significantly larger probability of employer transitions relative to the pre-reform period that occurs immediately in the month of expiration but then declines 3 months after the expiration.

Overall, these results are consistent with the predictions of the impact of reducing monopsony power of firms and moving towards a more competitive labor market. The earnings and quantities of incumbent workers both rise. In regressions estimated at the individual level, the increase in quantities is observed through the increased probability of staying at the firm. Note that the model presented does not formally have any prediction on employer transitions. In theory, if firms respond to the increased competition for workers by appropriately adjusting earnings, there may be no employer transitions in equilibrium. In reality, we would expect that a reform that allows workers to the right to change employers to lead to an increase in job transitions. However, the magnitude of the estimated effects on job transitions is much smaller than the estimated effects on earnings and staying with firm in the UAE. This underscores the idea that the main effect of the reform was through firms responding to increased labor market competition rather than being driven by transitions increasing the match quality between

workers and firms.

6.4 Estimates of Reform Effects

While the figures provide compelling evidence, we formally present the regressions results of the following specification:

$$y_{it} = \sum_{k=0}^3 \gamma_k^{Post2011} D_{it+k} + \sum_{k=0}^3 \gamma_k^{Pre2011} D_{it+k} + \delta_i + \delta_t + \epsilon_{it} \quad (5)$$

The key difference from equation 4 is that we omit the leads to contract expiration ($-3 \leq k < 0$), so the coefficient estimates are relative to all three months prior to expiration. Given that the estimates of other leads were generally not significantly different from the period prior to expiration, these estimates are quantitatively very similar but parsimonious enough to display in tables. The main hypothesis to be tested is whether $\sum_{k=1}^3 \gamma_k^{Post2011} - \gamma_k^{Pre2011} = 0$ for earnings, worker retention, exits from the UAE and employer transitions. This is the test of whether outcomes are different following a contract expiration after the reform as compared with before the reform. Given the variation in exactly when in the month contracts expire, we are also interested in testing $\gamma_0^{Post2011} - \gamma_0^{Pre2011} = 0$, which we show in the third row of each panel, although this is not our main parameter of interest.

In addition to the basic specification given in equation 5, we also include a number of controls in order to eliminate possible confounds in our identification strategy. We include quartic polynomials of the time between the date that the current contract expires and the reform, separately for before and after the reform. This is in order to control for possible changes in the wage-contract tenure profile owing to the reform, or, albeit less likely, endogenous transitions and exits as a function of contract tenure.

We next include a vector of worker characteristics (age, Indian citizenship, education) interacted with year-quarter fixed effects. This allows for time-varying effects of observable differences in the characteristics of individuals whose contracts are expiring at different times. We also include an indicator for whether the initial job was in construction interacted with year-quarter fixed effects, as Table 1 suggested substantial changes over time in the share of workers in construction.

Finally, we include three months of lagged earnings, as well as their interactions with *Post2011*, in order to control for autocorrelation in earnings, as well as endogenous transitions owing to past earnings.²⁹ For example, employers may try to terminate worker contracts

²⁹The last columns of Appendix Table A.1 show that the earnings results are also robust to the inclusion of firm fixed effects. As there are relatively few firm-to-firm transitions, we show these only for completeness.

prior to the expiration date following an unusually low period of labor demand, which would show up as low earnings immediately prior to a separation.

In addition to the various control variables, we also examine the results when we restrict the sample to workers with earnings observations both before and after the reform. This ensures that the estimates are not driven by changes in the composition of new entrants over time. Finally, we examine a sub-sample where we discard the data in the first and last calendar quarter of the sample, together with the quarter immediately preceding and immediately following the reform (quarter 4 of 2010 and quarter 1 of 2011). Dropping the first and last quarter addresses the concern that there is a selection problem for these periods; for example, not all firms may have paid out their wages for October 2012 when the data were obtained for us. Dropping the quarters immediately around the reform addresses potential issues that the timing of announcement and implementation were in response to labor market conditions in those particular months.

Panel A of Table 2 shows the key estimates of interest, $\sum_{k=1}^3 \gamma_k^{Post2011} - \gamma_k^{Pre2011} = 0$ and $\gamma_0^{Post2011} - \gamma_0^{Pre2011} = 0$ from equation 5 for log earnings.³⁰ Column 1 shows results from the specification with no controls. The top row of the table shows the pooled effect of all three months after the contract expiration interacted with the post-reform dummy, minus the pooled effect of all three months after the contract expiration interacted with the pre-reform indicator.³¹ The standard error is reported in the row below. The third row shows the differential effect on the month of expiration, and this is always positive and significant, as well as always smaller than the average effect in the subsequent 3 months, consistent with substantial heterogeneity in exactly when in the month contracts expire.

The magnitudes of the effects are substantial. We find an 11% effect on real monthly earnings, that is, monthly earnings grow by about 11% in the 3 months following a contract expiration after the reform, with an increase of 1.4% in the month of contract expiration. Column 2 includes polynomials in time to reform, and column 3 includes both the time to reform polynomials as well as the time-varying effects of individual characteristics. The coefficients are very similar across specifications, with a 1.3 to 1.5% additional increase in earnings in the month of a contract expiration following the reform, and a mean increase of 11% in the 3 months following a contract expiration post 2011. The coefficient magnitudes change somewhat when autocorrelation in earnings is accounted for, but there is still a 3% increase in the month of a contract expiration

³⁰ Appendix Tables A.2 - A.5 display all of the corresponding estimates of $\gamma_k^{Post2011}$ and $\gamma_k^{Pre2011}$ for the four main outcomes of interest. The first 4 coefficients are $\gamma_3^{Post2011} \dots \gamma_0^{Post2011}$, while the next four are $\gamma_3^{Pre2011} \dots \gamma_0^{Pre2011}$. Clearly, in Appendix Table A.2, there is a differential increase in earnings following a contract expiration after the labor reform. This occurs both in the month of expiration as well as in the 3 months afterwards.

³¹ Note the pooled average effect is given by $\sum_{k=1}^3 \frac{\gamma_k^{Post2011} - \gamma_k^{Pre2011}}{3}$.

post-reform, and an additional 5.3% increase in earnings over the next 3 months.³² These estimates are all significant at the 1% level.

Columns 5 and 6 correspond with columns 1 and 3, respectively, except the sample is restricted to workers with at least one earnings observation before and after the reform. The coefficients on the month of contract expiration post-reform increase to around 5%, while the mean earnings increase over the subsequent 3 months is roughly 9%. Columns 7 and 8 estimate the same specifications, but now the sample omits the first and last quarter of the sample and the two quarters surrounding the reform. The coefficients are very similar to columns 1 and 3, but slightly larger in magnitude. The estimates with the sub-samples remain significant at the 1% level.

One possible concern is that we are only observing earnings, and not wages. Thus it could be that the estimated earnings increase is coming from an increase in hours worked, rather than an increase in wages. While we do not observe actual hours worked each month, we construct upper and lower bounds on hours worked using data from the MOL on the contracted hours and contracted wages. Table 3 presents these results and confirms that hours did not increase following the reform. If anything, there is some evidence of a decline in hours, but this is imprecise and sensitive to specification.

We present the estimates where the dependent variable is the probability of staying with the same firm (times 100) in panel B of Table 2. After the reform, workers are more likely to stay with their existing firm. Across the various specifications and sub-samples, the effect size range implies that workers are 3 to 6.4 percentage points (22 to 44%) more likely to continue working for the same employer following a contract expiration. This is significant at the 1% level in all specifications. The strong positive effect on firm retention is expected because the magnitude of the decline in the probability of exiting the UAE is much larger than the magnitude of the increase in employer transitions.

Panel A of Table 4 shows estimates for exits from the UAE and has the same structure as Tables 2. The results show consistent positive effects of contract expirations on the probability of exit on average, but significant reductions in this probability following the reform. These estimates are all significant at the 1% level. The effects are apparent in the month of the contract expiration, but become larger in the subsequent months. The one set of results with substantially different coefficients is in the trimmed sample with controls, where the negative

³²There is low autocorrelation in earnings conditional on worker and month fixed effects (with the first and largest lag having a coefficient of 0.027 pre-reform, and 0.03 following the reform). The low autocorrelation and relatively long panel suggests that Nickell bias is not likely to be a problem in this specification, despite lagged dependent variables.

effect of the reform on the probability of leaving the UAE following a contract expiration falls by about 50%, but is still significant. Given that the rate of employer transitions is generally low, the results for country exits generally mirror the results where the dependent variable is whether the individual stays with the firm. For parsimony, we omit the results for staying with the firm in the rest of the paper and continue to present the results for country exits.

Panel B of Table 4 shows the same specifications and samples where the dependent variable is employer transitions. Recall that from Table 1 the overall rate of employer transitions is quite low, and so the magnitude of the coefficients is substantial relative to the base rate of transitions. Without controls, we see a 0.48 percentage point increase in the probability of an employer transition during the month of a contract expiration, with an extra 0.67 percentage point increase per month on average over the next 3 months.³³ These estimates are significant at the 1% level.

Another way to consider the magnitude of the effects on employer transitions is on a per-contract basis rather than on a per month basis. Individuals can only exit a contract (for another firm or to leave the contract) once per contract. The per-contract impact is given by $\sum_{k=1}^3 \gamma_k^{Post2011} - \gamma_k^{Pre2011}$. The reform increases the per-contract probability of changing employers by 2 percentage points. While the magnitude of the impact on mobility may seem small, this represents a doubling of the base rate of transitions prior to the reform.

When the tenure polynomials are included, the coefficients are virtually identical. When both time to reform polynomials and the time-varying effects of worker characteristics are included, the effect during the month of expiration increases and the probability of a transition over the next 3 months are both lower by roughly 50%, but still significant at the 1% level. The lower magnitude of the effect, where the additional post-reform monthly probability of a transition is 0.26 percentage points, persists with the inclusion of lagged earnings.

In columns 5 and 6, where the sample is restricted to observations with earnings observed both before and after the reform, the immediate effect of a contract expiration after the reform is a small fall in the probability of a transition, but this is offset by a increased probability of a transition 1 and 2 months after the contract expired. In columns 7 and 8, where the sample excludes months at the beginning and end of the sample as well as near the reform period, we obtain coefficients quite similar to columns 1 and 3. To put these coefficients into perspective, even the smaller coefficients represent a doubling of the base rate of employer transitions.

Despite the large change in transitions induced by the reform, the post-reform level of tran-

³³Looking at the individual lags in Appendix Table A.4, we can see that the post-reform coefficient is smallest in the last lag, consistent with the relatively short window workers have to find a new employer.

sitions remains relatively low. In equilibrium, as firms adjust their payments to workers, the threat of changing employers can have effects on earnings without actual transitions.³⁴ If there is little match-specific productivity in this context, then transitions will yield little surplus. Another plausible explanation is that the infrastructure for searching for positions in the UAE was not well developed prior to the reform and requires more time than the sample frame of our data to fully develop. Finally, employers could have private information about worker quality, so that transitions do not occur because few employers are willing to hire incumbent migrants that the initial firms are unwilling to pay to retain.

Overall, the results suggest that the labor reform led to an improvement in the outcomes of migrant workers already in the UAE. The basic results are consistent with a reduction of monopsony power. Granting them the ability to switch jobs at the end of a multi-year contract without needing approval from their previous employers increased employer transitions, worker retention and earnings and decreased the likelihood of leaving the UAE for at least 6 months.

6.5 Accounting for Selection: Imputed Outcomes

One important concern is that the selection induced by exits from the UAE labor market could be significantly biasing the results on earnings and transitions.³⁵ We implement two bounds, one wider and thus more conservative than the other more “naive” bounds. Extending Manski (1990) to our differences-in-differences setting, we deal with this by imputing earnings and employer transitions for observations that exit the UAE. The aim is to produce upper and lower bounds on our main results. For both log earnings and employer transitions, we first recover residuals of each outcome \tilde{y} in the 7-period window around a contract expiration, conditional on worker and year-month fixed effects. We calculate the 90th and 10th percentile values of the distribution of residuals separately for before and after a contract expiration, and separately pre and post the reform. In other words, we calculate \tilde{y}_v^{tw} where t denotes pre or post reform, v denotes the 90th or 10th percentile and w equals 1 for after contract expiration and 0 for before contract expiration.

To impute an upper bound on our coefficients, we assume that all exits following a contract expiration after the reform have the 90th percentile value, \tilde{y}_{90}^{Post1} , and all exits prior to a contract expiration, but after the reform, have the 10th percentile value, \tilde{y}_{10}^{Post0} . Similarly, we impute \tilde{y}_{90}^{Pre0} for all pre contract expiration exits prior to the reform, and \tilde{y}_{10}^{Pre1} for all the post contract

³⁴An analogy be found in no-fault divorce laws, which increased settlement payments but did not appreciably increase the divorce rate (Peters 1986).

³⁵This selection can include exiting the UAE entirely, entering the informal market or working in freezones outside of the jurisdiction of the MOL.

expiration exits prior to the reform. For the lower bound, we impute \tilde{y}_{90}^{Post0} to all exits prior to a contract expiration but following the reform, and \tilde{y}_{10}^{Post1} to all exits following a contract expiration after the reform. The parallel assignment is done with \tilde{y}_{90}^{Pre1} and \tilde{y}_{10}^{Pre0} to exits prior to the reform. This strategy maximizes the impact of selection on the coefficients estimated by our differences-in-differences framework. The intuition of this approach is that the reform alters the types of individuals who choose to leave the country in the way that will shift our estimates the most.

The naive bound just assigns \tilde{y}_{90} to all exits for the lower bound and \tilde{y}_{10} to all exits for the upper bound regardless of whether the exit occurs before or after the reform or prior to or following a contract expiration. It can be shown that these naive bounds are tighter bounds than the ones above. Table 5 shows the coefficients of equation 5 using the imputed values of earnings and transitions. Column 1 shows the coarse bounds and column 2 shows the naive bound estimate of the baseline specification without controls for the imputation of log earnings that recovers an upper bound on the coefficients of interest, while columns 3 and 4 show the coarse and tight lower bound on the same coefficients. The upper bound of the impact of the reform on earnings over the 3 months following a contract expiration is 17–18%, while the lower bound is 3-5%. Thus, both remain positive and statistically significant at the 1% level. While the coarse bounds are wide, it is reassuring that the estimated earnings effects remain positive even when the pattern of selection on imputed wages is chosen to minimize the estimated effect. In contrast, the tighter naive bounds are similar to the main results, with the upper bound at 12% and the lower bound at 9%, both very close to the 11% estimate from the main sample.

Columns 5-8 report the same results with employer changes as the dependent variable. The overall impact is the same across the various bounds. Contract expirations result in increased likelihood of employer transitions following the reform, and the estimates are significant at the standard levels.

6.6 Time-shifted placebos

In order to rule out further sources of unobserved trends driving the results, we conduct a falsification exercise where we assume that contract expiration dates are uniformly shifted backwards by multiples of 3 months from 0 to 18. Formally, we re-estimate equation 5, replacing D_{it} with $\tilde{D}_{it}^j \equiv D_{i,t-3j}$, where j runs from 0 to 6.

Figure 8 shows the resulting plots of $\sum_{k=1}^3 \frac{\tilde{\gamma}_{i,t+k}^{Post2011,j} - \tilde{\gamma}_{i,t+k}^{Pre2011,j}}{3}$ for log earnings. Consistent with our previous results, the only positive and significant coefficient is where $j = 0$, which corresponds to our main specification. Figure 10 shows the same plot for employment changes,

while Figure 9 shows the same plot for UAE exits. In all cases, we obtain the result from our main specification at $j = 0$, a much smaller result at $j = 3$, and then no or little effect from $j = 6$ to $j = 18$. Overall, this suggests that our main effects are not driven by other changes in the contract tenure profile that are not due to contract expirations.

7 Heterogeneous Effects of the Reform

7.1 Heterogeneity By Worker Characteristics

We next examine heterogeneity by the worker characteristics in our data. Table 6 shows the estimates by various sub-populations. We begin by examining workers from India, who are by far the largest source country in our sample. Column 1 of panels A and B in Table 6 show that Indians both experience a larger wage effect, as well as a larger mobility effect. The results are somewhat smaller for the pool of workers from countries other than India, but they remain significant and the signs remain the same. Migrants who come from countries other than India are on average from countries poorer than India. Of the migrants that are not from India, about 45% are from Bangladesh and 25% from Pakistan and the GDP per capita of both countries is lower than in India. The outside opportunities of migrants from poorer countries are worse in their home countries and this affects their bargaining power with firms. However, it could also be that Indians are able to form very large networks due to their numbers and thus are able to take better advantage of increased mobility in the UAE. Furthermore, this suggests that the results cannot be driven by a policy change that occurs in a single host country in the same month as the reform in the UAE.

Column 3 of panel A in Table 6 shows that educated workers experienced a smaller wage gain from the reform, but experienced a larger than average probability of an employer transition, as shown in column 3 of panel B in Table 6. These results provide some evidence against the idea that the results are driven primarily by increasing match quality between firms and workers because we would expect match quality to matter more for highly educated workers.

When we look at workers who began in the construction sector, we see large effects of the reform on wages, at roughly 15%, and small increases in mobility, around 0.2 percentage points. Construction is the largest sector and the estimates confirm that the effects of the reform as present in but not limited to the construction sector. We also look separately at workers in the Dubai and Sharjah cities, as these workers potentially have more access to the free enterprise zones near these cities, which are governed by a different labor regulation system. We see a somewhat smaller (7%) wage effect, and a much larger mobility effect of 1.2 percentage points.

Interestingly, as shown in panel B, there does not seem to be significant heterogeneity in the probability of exiting from the UAE across the same sub-samples. The fact that this is true even in the Dubai-Sharjah sub-sample suggests that differential exits to the freezones is unlikely to be a significant explanation of our results. This also suggests that the heterogeneity in the change in firm-level elasticity of labor supply is driven more by heterogeneity in transitions than heterogeneity in exit opportunities.

Finally we look at young workers defined as those under the age of 35 at the start of the sample. While these workers do not experience wage effects that are very different from the overall sample (panel A, column 6), they do seem to experience a somewhat larger than average increase in mobility following the reform (panel C, column 5). While the mandated age of retirement for migrant workers was reduced from 65 to 60 at the same time as the NOC reform, this cannot explain the results as the estimated effects are not only driven by old workers. Perhaps consistent with the value of future work opportunities, we see a slightly larger fall in exits from the UAE for younger workers.

7.2 Unconditional Quantile Regressions

To understand better which incumbent workers are benefiting from the reform, we examine the effect of the reform by the unconditional quantiles of the earnings distribution. We do this using the method of Firpo, Fortin, and Lemieux (2009), which recovers the unconditional quantile partial effect. We are interested in the effects of the reform on the unconditional distribution of earnings, rather than the distribution of earnings conditional on worker and year-month fixed effects or on covariates, as the increased mobility may have altered the returns to various worker characteristics. For each decile τ , we define q_τ to be the value of decile τ of the distribution $F(y)$, $\widehat{f}(y)$ to be the estimated density of y , and $RIF_\tau(y)$ as the recentered influence function, given by

$$RIF_\tau(y) = q_\tau + \frac{\tau - \mathbf{1}(y \leq q_\tau)}{\widehat{f}(y)} \quad (6)$$

In other words, RIF is a measure of how much a given y “influences” any given function of the distribution $F(y)$. In this case, the function is the decile q_τ . The linear regression then recovers the effect of the independent variables on the (deciles of) the whole distribution $F(y)$.

With log earnings as y , we estimate the following regression for $\tau = 10, 20, \dots, 90$:

$$RIF_\tau(y_{it}) = \sum_{k=-3}^3 \gamma_{k\tau}^{Post2011} D_{it+k} + \sum_{k=-3}^3 \gamma_{k\tau}^{Pre2011} D_{it+k} + \beta_t X_{it} + \delta_i + \delta_t + \epsilon_{it} \quad (7)$$

Figure 11 plots the effects $\sum_{k=1}^3 \frac{\gamma_{k\tau}^{Post2011} - \gamma_{k\tau}^{Pre2011}}{3}$ for each τ . Unsurprisingly, the effect of the reform is uniformly positive at all deciles. More interestingly, the effects of the reform seem to be the largest at the bottom the wage distribution and this difference is statistically significant. This is consistent with the gap between marginal product and wage being largest at bottom of the wage distribution, so that the returns from increasing competition are the highest. Thus, the reform reduces wage dispersion due to imperfect competition, consistent with models of search frictions such as Burdett-Mortensen (1998).

8 Firm Hiring Decisions and New Entrants' Outcomes

We examine the impact of the reform on the firms' hiring decisions over new entrants. The empirical strategy here uses variation in the number of contracts that are expiring for a firm before and after the reform. As with the identification strategy for the individual level outcomes, this approach takes advantage of the fact that standard labor contracts are three years long. Variation in the contract expirations before and after the implementation of the reform are driven by hiring decisions that occur well before firms were aware of the possibility of such a reform. Furthermore, the number of contracts expiring each period are unlikely to be driven by the economic circumstances in that period. The main idea of the strategy is that firms that have more contracts expiring in the period after the reform relative to before it will be more exposed to the effects of the reform.

It is important to note one key difference in the empirical strategy for individuals as compared to the one presented here for firms. Even as individuals approach the expiration dates of their contracts, the costs of job mobility prior to the contract expiration remains equally high after the reform as compared to before the reform. In contrast, firm may anticipate periods where they have higher levels of contract expirations and can change their behavior before the actual period in which the contracts expire. Given the panel nature of the data, we can examine whether these anticipation effects may be happening.

We begin by estimating the following equation:

$$y_{jt} = \beta^{Post2011} \log \text{Expire}_{jt} + \beta^{Pre2011} \log \text{Expire}_{jt} + \delta_j + \delta_t + \epsilon_{jt} \quad (8)$$

where $\log \text{Expire}_{jt}$ is the logarithm of the number of labor contracts expiring in period t at firm j . The regression also includes firm fixed effects and year-month fixed effects. Errors are clustered at the firm level. We are interested in whether the impact of worker contract expirations on firms' hiring outcomes changes after the reform as compared with prior to the reform. This is

given by the estimate of $\beta^{Post2011} - \beta^{Pre2011}$.

In order to examine whether there are anticipation effects or adjustments that occur after contract expirations, we include three leads and lags of contract expirations. In other words, we estimate:

$$y_{jt} = \sum_{k=-3}^3 \beta_k^{Post2011} \log \text{Expire}_{jt} + \sum_{k=-3}^3 \beta_k^{Pre2011} \log \text{Expire}_{jt} + \delta_j + \delta_t + \epsilon_{jt} \quad (9)$$

and k refers to leads of log expirations when $k < 0$ and to the lags when $k > 0$.

The data used in this analysis aggregates the worker-level data to the firm level, and includes the data from the payroll processing firm combined with information on contracts from the MOL to construct a monthly panel of firms. The number of contracts expiring each period in a firm is taken by aggregating all of the MOL contract level information. Our firm analysis relies on the payroll data and the MOL contracts data, so our information on firms is currently limited to worker outcomes.

We examine how firm hiring responds to the number of contract expirations that the firm faces before and after the reform. The first row of Table 7 shows the estimate of $\beta^{Post2011} - \beta^{Pre2011}$. Column 1 presents the parsimonious specification. Column 2 includes fixed effects for each city by year-month to control for time-varying city-level changes. Column 3 includes the three leads and lags of the logarithm of contract expirations corresponding to equation 9. Column 4 limits the sample to firms with observations both before and after the reform. The sample specification removes firms that do not exist prior to the reform and firms that die after the reform. Finally, column 5 is the trimmed sample that omits the first and last calendar quarter of the data as well as the quarters immediately surrounding the announcement and implementation of the reform.

Panel A of Table 7 presents the impact of the reform on hiring new entrants to the UAE.³⁶ The outcome is the logarithm of the number of workers hired that month who are entering the UAE for the first time.³⁷ The results generally indicate that firms with a greater number of contracts expiring after the reform relative to before the reform hire fewer new entrants to the UAE. As the model suggests, the reduction in the hiring of new entrants by firms is the result of the costs associated with the lower probability of retaining workers after the initial contract expires. The parsimonious estimates and the estimates that control for city by year-month fixed

³⁶We also examined the impact of the reform on the characteristics of new entrants hired. We find no effects on the education or age of new entrants. These results are available on request.

³⁷This variable is constructed using the full history of contracts in the MOL data and whether the payroll data corresponds to the first month of the first contract of the individual.

effects indicate that a standard deviation increase in the percent of contracts expiring corresponds with about a 2 percent decline in the number of new entrants hired. This is significant at the 10 percent level. The magnitude of the impact doubles when the sample is restricted to only include firms that exist on both sides of the reform and in the trimmed sample, and the results are significant at the 1% level. With the inclusion of the leads and lags contract expirations (column 3), the difference in the impact of contract expirations after the reform relative to before the reform no longer has a significant, negative effect on the new entrants hired in the month in which the expirations occur. In this specification, the impact of the leads are jointly negative and significant at the 5% level. This suggests that firms may anticipate the effects of their workers' contract expirations in the months leading up to them, and adjust their decisions on hiring new entrants before the actual contract expirations are realized.

We examine the impact of the reform on the earnings paid to new entrants in Panel B of Table 7.³⁸ The estimates in column 1 and 2, corresponding to the parsimonious specification and the inclusion of city by year-month fixed effects, respectively, indicate a 3 percent decline in the earnings of new entrants. These estimates are significant at the 10% level. However, the estimates are no longer significant at the standard levels with the two restricted samples. In the specification with the leads and lags of expirations in column 3, the results show that firms adjust the earnings of new entrants in the months prior to the expirations. Firms' anticipation of the implications of the contract expirations and the decrease in the earnings of new entrants in the months prior is consistent with the results in Panel A where hiring of new entrants also adjusts in the three months prior to the expirations.

These results provide a fuller understanding of the effects of the reform. While existing workers in the UAE are shown to be better off with higher job mobility, higher earnings and declines in their rates of leaving the UAE, the firm-level results demonstrate that firms respond to the firm in ways that may not be entirely positive for all workers in the short run. In particular, firms on average hired fewer new entrants and the earnings of new entrants in their firm contracts in the UAE fell following the reform. These results are consistent with high substitutability in the model of monopsony presented and highlight a potential trade-off in reforms that relax labor market restrictions on migrant workers; labor market restrictions on migrants in the host country encourage firms to bring in more migrant workers than they would otherwise.

³⁸Table A.6 presents the firm-level estimates that correspond to the outcomes examined in the worker-level regressions. The direction and significance of the estimates are generally consistent with the individual-level results.

9 Recovering Market Power Parameters

We can use our estimates of worker and firm responses to the reform to recover the main parameters of our model. Of particular interest are ϵ_c and ϵ_r which capture the degree of monopsony power firms have vis-a-vis incumbent and potential migrant workers, and allow us to recover the incidence of the reform on both potential migrant workers as well as existing migrant workers. Throughout this section we use our preferred estimates. From our firm-level estimates, we have $\frac{\Delta \log w_r}{\Delta \log L_I} = -0.034$ and $\frac{\Delta \log L_r}{\Delta \log L_I} = -0.038$.³⁹

With the assumption that η_r is constant before and after the reform, we can easily obtain an estimate of ϵ_r by using the change in contract expirations interacted with the reform as an exogenous shock to the marginal product of potential migrants. Using contract expirations are exogenous variation in the the quantity of new entrants, we can estimate:

$$\Delta \log(w_r) = \frac{1}{\epsilon_r} \Delta \log L_r \quad (10)$$

as the ratio $\epsilon_r = \frac{\Delta \log L_r}{\Delta \log w_r} = \frac{0.038}{0.034} = 1.1$ and $\eta_r = 0.53$. This implies that wages for new recruits are roughly one-half of marginal product.

It is somewhat more involved to recover ϵ_c . This is because the reform itself increased ϵ_c , and the response of firms will depend on the elasticity of substitution σ , so that:

$$\log \frac{w_c}{\eta_c} - \log \left(\frac{w_r}{\eta_r} + v_r \right) = -\frac{1}{\sigma} \frac{\log L_r}{\log s(w_c)L_I} \quad (11)$$

which yields the following expression for the change in η_c (assuming that σ and v_r are constant before and after the reform):

$$\Delta \log \eta_c = \Delta \log w_c - \Delta \log w_r - \frac{1}{\sigma} \Delta \log s(w_c)L_I + \frac{1}{\sigma} \Delta \log L_r \quad (12)$$

From the worker-level estimates, we have that the increase in the wage for a worker experiencing a contract expiration is $\Delta \log w_c = 0.11$. The total percentage increase in the probability of staying with the firm, $s(w_c)$, is 0.12.⁴⁰ Thus, we have $\Delta \log w_c = 0.11$, $\Delta \log w_r = -0.034$, $\Delta \log s(w_c)L_I = 0.12$ and $\Delta \log L_r = -0.038$, which implies a $\Delta \log \eta_c = 0.1474 - \frac{1}{\sigma}0.082$. Thus, the impact of the reform on the elasticity of labor supply of incumbent migrants depends on the value of σ . We cannot estimate σ from our data, as relative prices and relative quantities are not

³⁹Note that we need to convert the firm-level estimates to be comparable in magnitude with the worker-level estimates. Since the average number of expirations per month per firm is roughly 1, a 100% increase in expirations corresponds to 1 extra expiration on average.

⁴⁰To get this, we multiple the coefficient estimate, 3.8%, by 3 (the number of months post-expiration) and divided by 95, which is the average rate of staying.

sufficient to identify the elasticity of substitution in a non-competitive model even if exogenous variation in quantities is available.

To side-step this issue, we calculate ϵ_c^{post} for several values of σ . We can use the definition of η_c to solve $\Delta \log \eta_c = \log \frac{\epsilon_c^{post}}{1+\epsilon_c^{pre}} - \log \frac{\epsilon_c^{pre}}{1+\epsilon_c^{pre}}$ for ϵ_c^{post} to get:

$$\epsilon_c^{post} \approx \frac{(1 + \Delta \log \eta_c) \epsilon_c^{pre}}{1 - (\Delta \log \eta_c) \epsilon_c^{pre}} \quad (13)$$

This implies a percent increase in ϵ_c is approximated by $\frac{\Delta \log \eta_c (1 + \epsilon_c^{pre})}{1 - (\Delta \log \eta_c) \epsilon_c^{pre}}$. However, obtaining ϵ_c^{pre} requires further assumptions.⁴¹ We consider two scenarios, and fortunately, both yield similar results. First, and most simply, is to note that prior to the reform the distribution of outside options available to both potential migrants and incumbent migrants were the same, and assume $\epsilon_c^{pre} = \epsilon_r$. Independent of migration costs, both sets of workers can either take the job with the firm, or some other option in the source country. If we suppose that $\sigma \geq 1$, which is plausible given that potential migrants and incumbent migrants are likely to be relatively substitutable, then the lowest value of ϵ_c^{post} is 1.26, increasing up to 1.51 when $\sigma = 20$. Thus, for a broad range of σ , we obtain post-reform firm-level labor supply elasticities that are quite low, but are 15% to 40% greater than the pre-reform elasticity.

While plausible, there are still reasons to doubt the assumption that $\epsilon_r = \epsilon_c^{pre}$, as incumbent and potential migrants may have very different information about labor market opportunities while in the home country. Alternatively, assuming that L_r and L_c are perfect substitutes with no productivity differences, so that $\sigma = \infty$, we can recover η_c^{post} and η_c^{pre} from the first-order condition:

$$\frac{w_c^{pre}}{\eta_c^{pre}} = \frac{w_r}{\eta_r} + v_r = w_r \left(\frac{1}{0.53} + \frac{0.2}{3} \right) \quad (14)$$

where we make use of an estimate of that recruitment costs are approximately 20% of a worker's annual earnings (Gutcher 2013).⁴² From this, we can recover $\eta_c^{pre} = \frac{w_c^{pre}}{w_r^{pre} (.067 + \frac{1}{.53})} = 0.52$ since firm-level summary statistics imply $\log \frac{w_c}{w_r} = 0.01$. This suggests that $\epsilon_c^{pre} = 1.04$, and $\Delta \log \eta_c = 0.1474$, which implies a roughly 35% increase in the elasticity of incumbent migrant labor supply facing the firm, up to 1.41. While our context is quite different from other labor markets, this estimate is well within the range of elasticities of 1 to 1.9 reported by Falch (2010) for Norwegian teachers, although much larger than the 0.1 elasticity reported for VA

⁴¹If we assumed that the reform led to perfect competition, then $\epsilon_c^{post} = \infty$, and we could recover $\epsilon_c^{pre} = 15.29$ ($\sigma = 1$) to $\epsilon_c^{pre} = 6.8$ ($\sigma = \infty$). However, our results on transitions and our discussion of the low transition rates above suggest that assuming perfect competition after the reform is implausible.

⁴²Note we adjust the recruitment cost by the fact that contracts last for three years but the estimated recruitment cost is relative to one year of work.

hospital nurses by Staiger, Spetz, and Phibbs (2010). These results are also different from the effectively infinite elasticities for low-wage nurse aides reported by Matsudaira (2014). Our estimates suggest workers were getting roughly 50% of their marginal product before the reform, increasing to up to 60% following the reform.⁴³

Taken together, these estimates imply that the pre-reform labor supply elasticity facing the firm is close to 1, increasing to between 1.2 and 1.5 following the reform. While a far cry from perfect competition, it does suggest that the reform that allowed workers an opportunity to switch employers decreased the degree of monopsony power and closed the gap between wages and marginal product.

10 Alternative Explanations

10.1 Match Quality

Reducing the cost of job-to-job transitions may increase productivity (and hence workers' earnings) by creating better matches between workers and firms. This is unlikely to be the main explanation for the results for several reasons. First, the results on new entrants are consistent with the model of market power and not consistent with the predictions of a model of match quality and pure competition. Furthermore, our results indicate that highly educated workers experience a smaller wage increase following the reform than less educated workers (Panel A of Table 6). The quantile estimates shown in Section 7.2 demonstrate that the earnings effects are largest at the lower end of the earnings distribution. If we expect match quality to matter most for highly skilled workers, these provide suggestive evidence against the idea that match quality alone explains the results.

More importantly, the aggregate rates of employer transitions are low even after the reform. We examine the impact of the reform on the earnings for the sample of individuals who remain with the same firm after their contract expires. The results are presented in columns 1 and 2 of Appendix Table A.1. Given that the stayers represent 97% of the sample, it is not surprising that the earnings results are very close to the main estimates. This indicates that employers responded to the increased labor market competition by altering wages paid to workers without equilibrium transitions as suggested in Cahuc, Postel-Vinay and Robin (2006). This also highlights the fact that the earnings results are not only driven by job changers, suggesting that match quality alone cannot explain the results. While match quality may play a role, the evidence suggests that increase in labor market competition is the primary explanation for the effects.

⁴³This is less than the 70-85% of marginal product found by Isen (2013) in the United States.

10.2 Contract Duration

In this section, we focus on the possibility that the change in contract duration (from 3 to 2 years) that was announced and implemented at the same time as the NOC reform could be driving the estimated results. While there were other changes to the minimum and maximum ages, as well as labor card costs for highly educated in the reform, they do not affect the vast majority of workers in our sample.⁴⁴

Using the data on all contracts in the MOL database, Figure 12 shows the average length of worker contracts by the start date of the contract. Contract lengths fell substantially at the time the reforms on contract length and on the NOC requirements were implemented.⁴⁵ How the change in contract duration might affect the main estimates depends on whether employers and workers prefer longer or shorter contracts. Shorter contracts correspond to shorter durations of tied labor for employers and may lead employers to offer lower wages; this would suggest the bias from the change in contract length would bias our results downwards.

In contrast, if workers prefer long contracts, as they provide more employment stability, the change in contract length may lead to workers needing higher wages to compensate for the lower job stability. However, if workers are dissatisfied with contracts of a shorter duration, we would expect to see a higher rate of exiting the UAE, the opposite of what we see in the data.

Another possibility is that firms prefer longer contracts for new entrants because firms can pay new entrants less than incumbent migrants. A decline in the duration of contracts can shift firms' preference for workers away from new entrants towards incumbent workers and increase competition for workers in the UAE. This explanation is consistent with the results for the hiring and earnings of new entrants and for the results on the earnings and country exits of incumbent migrants in the UAE.⁴⁶ However, the magnitudes of the estimates indicate this is unlikely to be the primary driver for the effects we find. The recruitment costs for new entrants that would be necessary to justify employers being willing to pay all incumbent migrants over 10% more every year far exceeds the reported estimate of recruitment costs, particularly when the high rate of renewal and low age of workers is considered.

⁴⁴The last column of each panel of Table 6 demonstrates that the results remain similar for workers younger than 35 and are not driven exclusively by old workers.

⁴⁵Note that the full MOL contracts data includes special short term contracts in addition to the standard multiple year contracts. The ratio of standard contracts to special short term ones did not change over the sample period.

⁴⁶This story in itself would not lead to an increase in employer transitions among incumbent workers.

10.3 Other Issues

Another potential interpretation is that workers are not exiting to their source country but in fact are remaining in the UAE with a valid visa in the illegal labor market. While this is likely to happen in some cases, it does not affect the interpretation of our results for earnings or job mobility. As shown in Section 6.5, the results are robust to addressing selective exits from the data. Furthermore, it is unlikely to represent a major component of the exit response as the informal labor market is relatively small in the UAE. The government devotes substantial resources to locating and deporting most illegal workers, and sentences for illegal activity are quite harsh (50,000 AED for employers hiring workers without a valid visa).⁴⁷

One possible concern is that the results on earnings are not reflective of the total compensation of workers or job quality.⁴⁸ This is unlikely to be an important concern as the observed reduction in exits suggests that the observed increases in earnings are not completely offset by reductions in other sources of compensation or declines in job quality. However, the data also allow us to consider this more carefully. We examine whether in-kind transfers, which usually include employer-provided housing and food, may explain the results. We observe the value of in-kind benefits associated with contracts in the MOL data. We find that the ratio of in-kind benefits to contract earnings does not change after the reform. This suggests that the increase in earnings associated with the reform is not completely offset by a decrease in the value of in-kind transfers.

Another concern is that changes in recruitment costs occurring at the same time as the change in mobility requirements increased demand for incumbent migrants already in the UAE. While there was a reduction in visa fees (down to 300 AED), it was only for workers with university degrees (who are very few in our sample) and for firms with greater than 15% Emirati workers and greater than 20% skilled workers. This is certainly not enough to account for a 10% increase in earnings across our sample of largely low wage workers. We consider whether a large shift in non-visa recruitment costs may explain the results. While we do not have data on hiring costs, we identify 652 recruitment firms in the MOL data by whether the words recruitment, human

⁴⁷An amnesty in the UAE in 2007 had up to 342,000 workers take advantage of it. Baker (2013) writes that almost 100% of illegal immigrants applied for a similar amnesty in the U.S., a plausible estimate is that this is most of the informal workforce. This estimate implies that roughly 5% of the workforce is illegal. Given we have effects on exits of up to 6 percentage points, it is unlikely that the bulk of our effect is coming through exits to the informal sector. In addition, news reports from 2011 suggest that absconding workers (which could be exits to the informal sector) fell from 27,231 in 2010 to 15,000 in 2011, and attributed this to “the ministry allowed workers in the private sector to move from one job to another without a six-month ban”. <http://gulfnews.com/news/gulf/uae/general/huge-decrease-in-number-of-absconders-1.888883> Accessed March 17, 2014.

⁴⁸This alternative interpretation could only potentially affect the results for earnings and not the findings on transitions and exits.

resources or manpower are in the name of the firm. We see no break in the trends in firm size or contract earnings for recruitment firms around December 2010 or January 2011 for recruitment firms in our data. Assuming that the size and earnings of recruitment firms would reflect any dramatic changes in the prices in the recruitment market, the data suggest that a change in recruitment costs that occurred at the same time as the mobility reform cannot explain the results.

We also consider the possibility that there is an event or policy change in one source country that occurs in January 2011 can explain all of the results for both incumbent and new migrants. Given that the most common nationality is Indian, we look at whether the estimated effects exist both for Indians and for non-Indian migrants. Columns 1 and 2 of Table 6 presents the results for the three main outcomes of interest. While there are some differences in the magnitudes of the effects for Indians and for migrants from other countries, the effects are the same sign and significance for the two groups. This confirms that a policy change in one of the source countries cannot explain the results.

A related concern is that a labor demand shock contemporaneous with the reform is driving the results. However, Figure 1 shows that the increase in contracted compensation occurs immediately after the reform and remains at a higher level, suggesting that the results are not driven by a short-term labor demand shock. We also tested for heterogeneity by labor demand, by using the number of new migrants in a quarter as a proxy for overall labor demand. However, these interactions yield no significant effects on earnings or transitions.

11 Conclusion

Transnational labor markets, including markets migrants from poor countries are employed for fixed-length contracts in richer countries, are growing in importance and are a potentially powerful way to raise world welfare and reduce across-country income inequality (Clemens 2011, Kennan 2013). While the international mobility allows workers in poor countries an opportunity to earn much higher income, the institutions surrounding these labor markets restrict the ability of workers to take advantage of labor market opportunities in the destination country and create potentially distortionary labor market power for employers. These institutions are present, in some form, in virtually every country with an extensive migrant worker population.

The reform of the NOC requirement of the *kafala* system in the UAE provides an opportunity to study the impact of increased labor market competition in the host country on migrants' labor market outcomes. Our paper considers the interplay between local and global labor markets

by offering a framework for understanding how a reform targeted at within-country changes affects both workers in the country as well as migrants who wish to migrate. Previously, visa requirements tied migrant workers to employers for the duration of their contract and restricted the extent of mobility in the labor market. The 2011 labor reform, which allowed workers to change employers after the expiration of a contract without explicit approval from their existing employers was quite effective: for incumbent workers in the UAE, wages increased by 10%, labor mobility doubled, and exits from the UAE fell, with a reduction in firm separations of up to 6 percentage points. At the same time, the reform led firms to hire fewer new entrants and they did not experience an increase in earnings in their initial contracts following the reform.

Our results suggest that international mobility is not enough to allow workers to capture their full marginal productivity. Restrictions on mobility within the destination country play an important role in depressing wages, and suggest that the surplus from global migration may be reduced and asymmetrically distributed due to lowered competition. This has important implications for policies governing migrant workers. These results also offer insight into many other types of labor markets where workers sign contracts that tie themselves to employers, be they bonded labor in agricultural settings or non-compete clauses in the U.S.

Additional research is needed to fully understand the implications of reforming the *kafala* system on international migration. More GCC countries, such as Saudi Arabia and Qatar, are contemplating similar reforms in the face of widespread international pressure. While we demonstrate that migrant workers already in the UAE benefited from the reform, albeit at the expense of new entrants, the reform can potentially affect firm choices and outcomes that we cannot observe in our data sets. While the model suggests some firm outcomes, such as profits or technology, that could be altered by the reform, we leave models and tests of the potentially rich general equilibrium effects of the labor reform to future work.

Finally, there are potentially complex long-run effects not considered in our paper. For example, our results suggest that increased labor market competition for incumbent migrants encourages migrants to stay at the destination longer, increasing the duration of migration spells and increasing the fraction of long-term migrants. While few of the migrants in the UAE can ever achieve citizenship, many countries like the U.S. that do offer migrants a path towards citizenship may face a more complex set of political considerations.

A Data Appendix

A.1 Matching Ministry of Labor Data with Payroll Processing Data

Both the Ministry of Labor (MOL) data on labor contracts of migrant workers and the payroll processing records with earnings disbursements contain a government issued identifier called the labor card id number. This numeric identifier is associated with each individual-firm match. When workers change employer, they receive a new labor card with a new labor card id number.

We use this identifier to match the two data sets. We lose 107,698 individuals in the payroll processing data set who have missing, non-numeric or incomplete identifiers. This is partially driven by the fact that some individuals in the payroll processing data set do not provide their labor card id. Some individuals provide the company with their passport or a driver's license, but the labor card id is used in the vast majority of cases. We are able to match 553,375 individuals in the payroll processing data with their contract information in the MOL data set. There are 25,883 individuals present in the payroll processing data that are not matched into the MOL data set. This reflects the fact that some migrant workers, including domestic workers and those working in the freezone areas of the UAE, fall under the jurisdiction of the Ministry of the Interior rather than the MOL. In A.1 we show the earnings density of the matched and unmatched workers in the UAE Exchange data. The Figure clearly shows that there is little difference in the densities, suggesting that the matching was not biased towards higher wages.

A.2 Variable Construction

Combining the MOL data with the payroll disbursement data allows us to construct several key variables in our analysis. Not all firms use the payroll services of UAE Exchange for earnings disbursement at all or for the entire period for which we have earnings data. The payroll data alone cannot allow us to distinguish whether a person that leaves the payroll data has changed employer or left the UAE. Fortunately, we are able to use the MOL data to construct these variables.

A.2.1 Exiting the UAE

We create a variable for exiting the UAE that takes on a value of one in the month in which an individual leaves the UAE, and zero otherwise. Because we have all of the labor contracts in the MOL, we identify country exits if one of two conditions holds. The first condition identifies whether the person leaves the country permanently. This is defined by whether the person's labor card is no longer active, and there are no subsequent contracts associated with that individual.

This includes both individuals that depart before their contract expires and those that leave when it expires. The second condition identifies whether the individual leaves the country for at least six months before returning to the UAE. If workers break their labor contracts with consent from their employers or if employers terminate the labor contracts without providing a NOC, then workers can only start jobs with new employers after leaving the UAE for six months. This is defined using whether the gap between the start of the next contract and the end of the previous contract is greater than six months.

A.2.2 Employer Change

This variable equals one if the person switches firm and zero otherwise. The MOL data contains both labor card id numbers, which change for each contract that an individual has with a firm, and an individual identifier that links a person across all of his contracts both within an employer and across employers. If an individual changes firms but there is at least 6 months between the end of one contract and the start of the other, this is classified as exiting the UAE and is missing for this variable. Thus, this variable captures a direct firm-to-firm switch rather than an individual who must leave the country for six months before he is allowed to take a new job.

A.2.3 Staying with the Firm/Firm Retention

Finally, we create an indicator variable for staying with the firm. This equals one if the individual works at the same firm as they did in the previous period. It equals zero if either employer change or exiting the UAE (as defined above) are equal to 1.

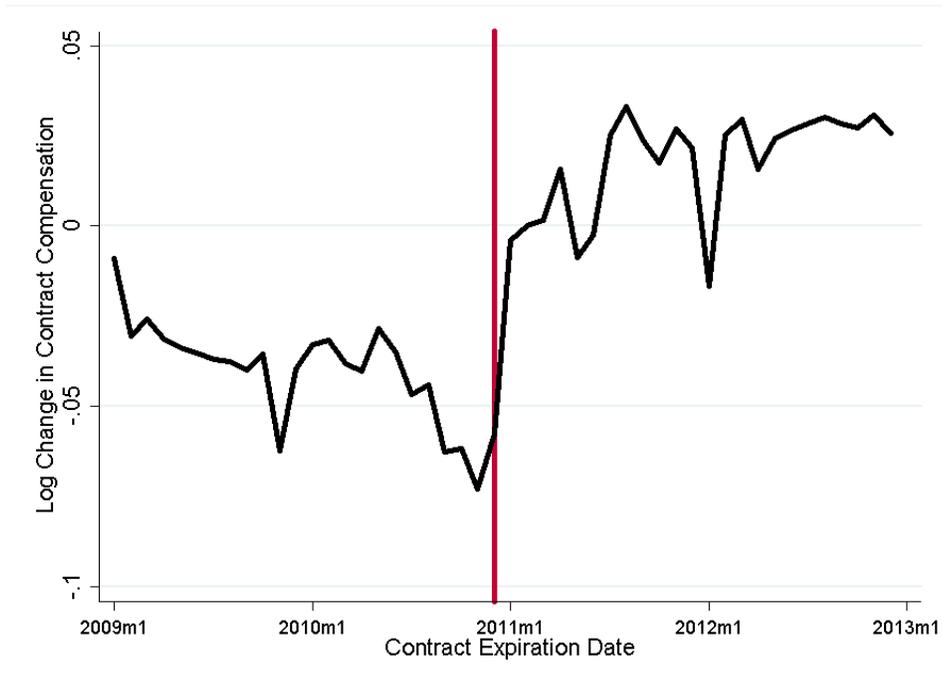
References

- ABRAMITZKY, R. AND F. BRAGGION (2006): “Migration and Human capital: Self-Selection of Indentured Servants to the Americas,” *The Journal of Economic History*, 66, 882–905.
- BAKER, S. (2013): “Effect of Immigrant Legalization on Crime: the 1986 Immigration Reform and Control Act,” *Working paper*.
- BARDHAN, P. K. (1983): “Labor-Tying in a Poor Agrarian Economy: A Theoretical and Empirical Analysis,” *The Quarterly Journal of Economics*, 98, 501–514.
- BRESNAHAN, T. F. (1982): “The Oligopoly Solution Concept is Identified,” *Economics Letters*, 10, 87–92.
- BRISTOL-RHYS, J., Z. BABAR, AND M. KAMRAVA (2012): “Socio-Spatial Boundaries in Abu Dhabi,” in *Migrant Labour in the Persian Gulf*, ed. by M. Kamrava and Z. Babar, Hurst Press, 59–84.
- BURDETT, K. AND D. T. MORTENSEN (1998): “Wage Differentials, Employer Size, and Unemployment,” *International Economic Review*, 257–273.
- CAHUC, P., F. POSTEL-VINAY, AND J.-M. ROBIN (2006): “Wage Bargaining with On-the-Job Search: Theory and Evidence,” *Econometrica*, 74, 323–364.
- CLEMENS, M. A. (2011): “Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?” *Journal of Economic Perspectives*, 25, 83–106.
- (2012): “The Effects of UAE Construction Jobs on Indians and Their Families: Initial Findings,” *Working paper*.
- (2013): “Why Do Programmers Earn More in Houston Than Hyderabad? Evidence from Randomized Processing of US Visas,” *American Economic Review*, 103, 198–202.
- FALCH, T. (2010): “The Elasticity of Labor Supply at the Establishment Level,” *Journal of Labor Economics*, 28, 237–266.
- FIRPO, S., N. M. FORTIN, AND T. LEMIEUX (2009): “Unconditional Quantile Regressions,” *Econometrica*, 77, 953–973.
- FORSTENLECHNER, I. AND E. J. RUTLEDGE (2011): “The GCC’s “Demographic Imbalance”: Perceptions, Realities and Policy Options,” *Middle East Policy*, 18, 25–43.
- GALENSON, D. W. (1984): “The Rise and Fall of Indentured Servitude in the Americas: an Economic Analysis,” *The Journal of Economic History*, 44, 1–26.
- GIBBONS, R. (1998): “Incentives and Careers in Organizations,” *Journal of Economic Perspectives*, 12, 115–32.
- GIBSON, J., D. MCKENZIE, AND S. STILLMAN (2011): “The Impacts of International Migration on Remaining Household Members: Omnibus Results from a Migration Lottery Program,” *Review of Economics and Statistics*, 93, 1297–1318.

- GUTCHER, LIANNE (2013): “I-expatriate Gets the Recruitment Job Done for UAE Companies,” <http://www.thenational.ae/business/industry-insights/the-life/i-expatriate-gets-the-recruitment-job-done-for-uae-companies>, accessed June 2014.
- HORNSTEIN, A., P. KRUSELL, AND G. L. VIOLANTE (2011): “Frictional Wage Dispersion in Search Models: A Quantitative Assessment,” *American Economic Review*, 101, 2873–98.
- ISEN, A. (2013): “Dying to Know: Are Workers Paid Their Marginal Product?” Tech. rep., unpublished Manuscript.
- KENNAN, J. (2013): “Open Borders,” *Review of Economic Dynamics*, 16, L1–L13.
- LONGVA, A. N. (1999): “Keeping Migrant Workers in Check: The Kafala System in the Gulf,” *Middle East Report*, 29, 20–22.
- MANNING, A. (2003): *Monopsony in Motion: Imperfect Competition in Labor Markets*, Princeton University Press.
- (2011): “Imperfect Competition in Labor Markets,” *Handbook of Labor Economics*, 4, 973–1041.
- MANSKI, C. F. (1990): “Nonparametric Bounds on Treatment Effects,” *The American Economic Review*, 319–323.
- MARX, M. (2011): “The Firm Strikes Back: Non-Compete Agreements and the Mobility of Technical Professionals,” *American Sociological Review*, 76, 695–712.
- MATSUDAIRA, J. D. (2014): “Monopsony in the Low-Wage Labor Market? Evidence from Minimum Nurse Staffing Regulations,” *Review of Economics and Statistics*, 96, 92–102.
- MCKENZIE, D., S. STILLMAN, AND J. GIBSON (2010): “How Important is Selection? Experimental vs. Non-Experimental Measures of the Income Gains from Migration,” *Journal of the European Economic Association*, 8, 913–945.
- MCKENZIE, D., C. THEOHARIDES, AND D. YANG (2012): “Distortions in the International Migrant Labor Market: Evidence from Filipino Migration and Wage Responses to Destination Country Economic Shocks,” *World Bank Policy Research Working Paper*.
- NAIDU, S. (2010): “Recruitment Restrictions and Labor Markets: Evidence from the Postbellum US South,” *Journal of Labor Economics*, 28, 413–445.
- NAIDU, S. AND N. YUCHTMAN (2013): “Coercive Contract Enforcement: Law and the Labor Market in Nineteenth Century Industrial Britain,” *American Economic Review*, 103, 107–44.
- NATIONS, U. (2012): “World Population Prospects: The 2012 Revision,” http://esa.un.org/unpd/wpp/unpp/panel_indicators.htm, accessed February 2014.
- NYARKO, Y., S.-Y. WANG, AND T. JOSEPH (2013): “Wage Flows and Remittances,” *Working Paper*.

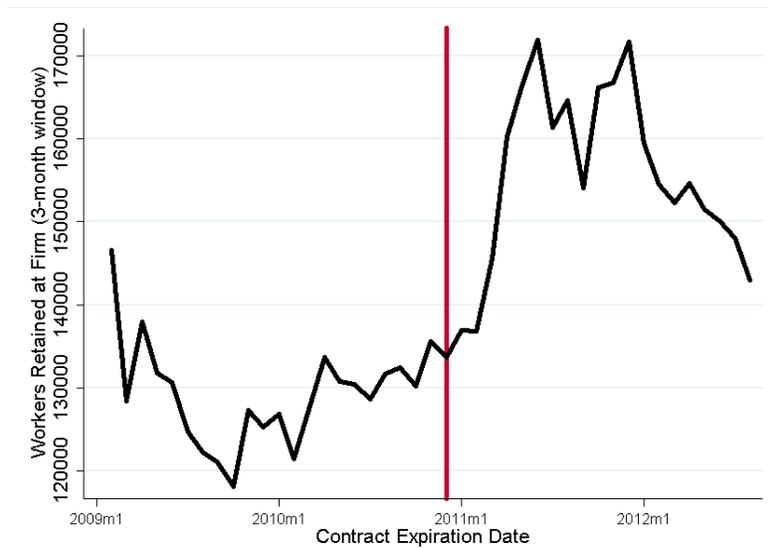
- PETERS, H. E. (1986): “Marriage and Divorce: Informational Constraints and Private Contracting,” *The American Economic Review*, 437–454.
- ROBINSON, J. (1933): *The Economics of Imperfect Competition*, London:Macmillan.
- ROGERSON, R., R. SHIMER, AND R. WRIGHT (2005): “Search-Theoretic Models of the Labor Market: A Survey,” *Journal of Economic Literature*, 43, 959–988.
- RUHS, M. (2013): *The Price of Rights: Regulating International Labor Migration*, Princeton University Press.
- SALEM, OLA (2010a): “Employers Criticize New Labour Regulations,” <http://www.thenational.ae/news/uae-news/politics/employers-criticise-new-labour-regulations>, accessed February 2014.
- (2010b): “New Law Will Make it Easier for Expatriates to Change Jobs,” <http://www.thenational.ae/news/uae-news/new-law-will-make-it-easier-for-expatriates-to-change-jobs>, accessed February 2014.
- SCULLY, G. W. (1974): “Pay and Performance in Major League Baseball,” *The American Economic Review*, 915–930.
- SECCOMBE, I. J. AND R. LAWLESS (1986): “Foreign Worker Dependence in the Gulf, and the International Oil Companies: 1910-50.” *The International migration review*, 20, 548–574.
- STAIGER, D. O., J. SPETZ, AND C. S. PHIBBS (2010): “Is There Monopsony in the Labor Market? Evidence from a Natural Experiment,” *Journal of Labor Economics*, 28, 211–236.
- VON LILIENFELD-TOAL, U. AND D. MOOKHERJEE (2010): “The Political Economy of Debt Bondage,” *American Economic Journal: Microeconomics*, 44–84.
- WAM (2010): “No Six-Month Work Ban From January 1, 2011,” <http://www.emirates247.com/news/emirates/no-six-month-work-ban-from-january-1-2011-2010-12-18-1.330941>, accessed February 2014.
- WATCH, H. R. (2013): “South Asia: Protect Migrant Workers to Gulf Countries,” <http://www.hrw.org/news/2013/12/18/south-asia-protect-migrant-workers-gulf-countries>, accessed February 2014.
- WEYL, E. G. AND M. FABINGER (2013): “Pass-Through as an Economic Tool: Principles of Incidence Under Imperfect Competition,” *Journal of Political Economy*, 121, 528–583.

Figure 1: Average Change in Log Real Compensation by Contract Expiration Date



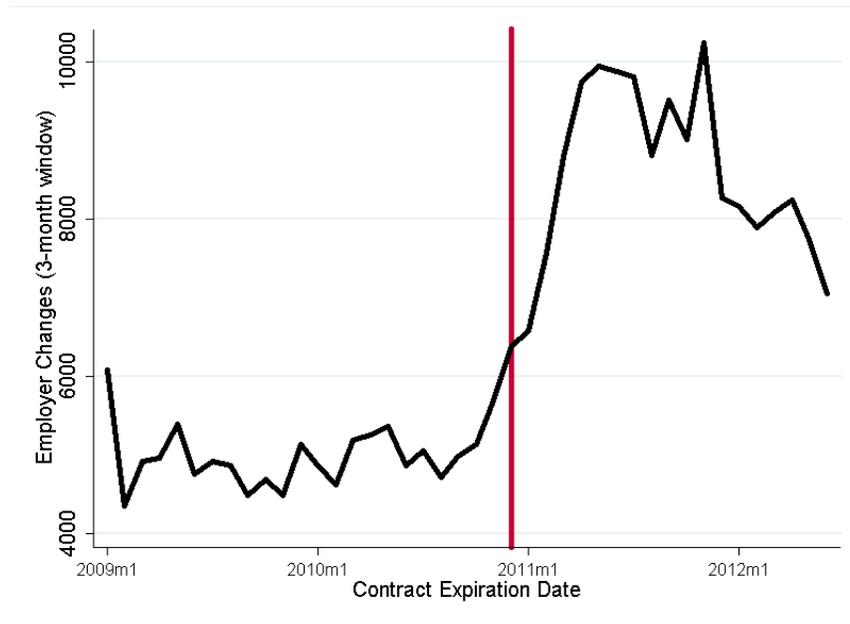
This shows the average log change in real compensation from one contract in time t to the next contract in $t + 1$ where t is the month of expiration of the preceding contract. Compensation includes the value of earnings and benefits defined in the contract. The sample is the MOL data.

Figure 2: Total Workers Retained by Firm by Contract Expiration Date



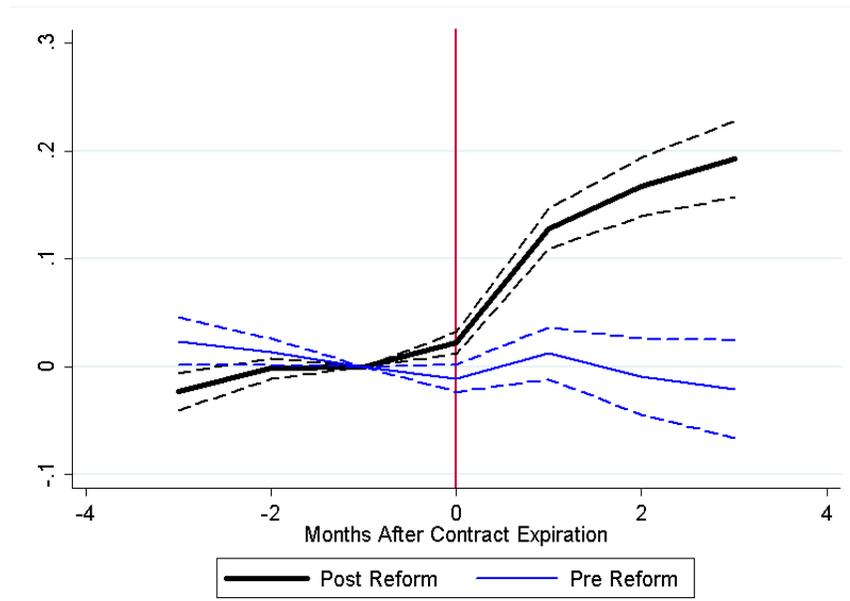
This shows the total number of workers that sign a new contract with their existing firm by contract expiration date. This is smoothed over the two adjacent months. The sample is the MOL data.

Figure 3: Total Changes in Employer by Contract Expiration Date



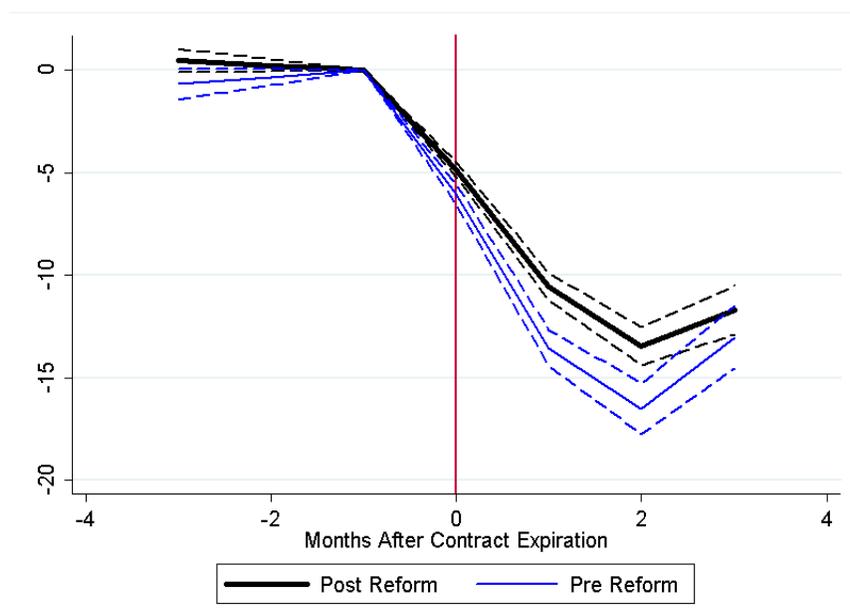
This shows the total number of employer changes that occur by contract expiration date. The total includes employer transitions that occur within three months of the contract expiration. The sample is the MOL data.

Figure 4: Impact of a Contract Expiration on Log Earnings: Pre- and Post-reform



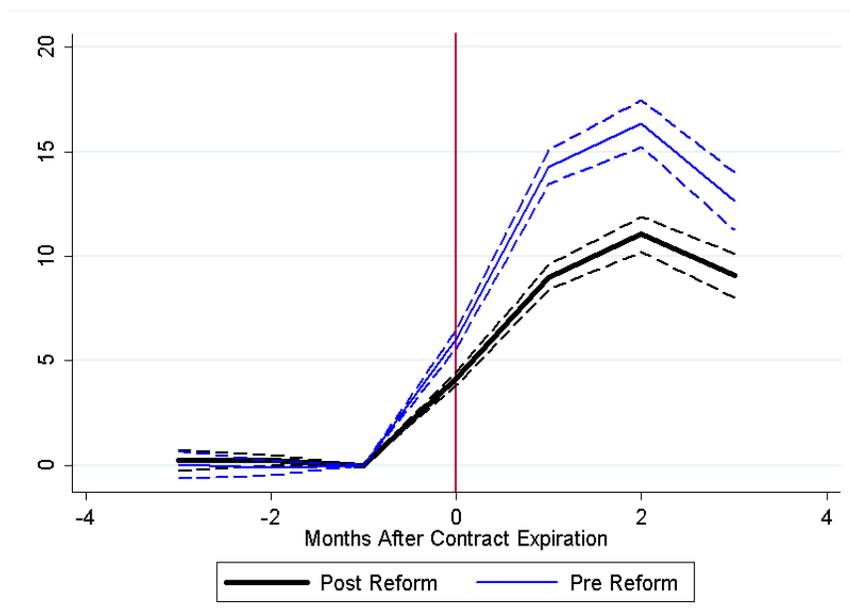
This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is log earnings. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval.

Figure 5: Impact of a Contract Expiration on Firm Retention: Pre- and Post-reform



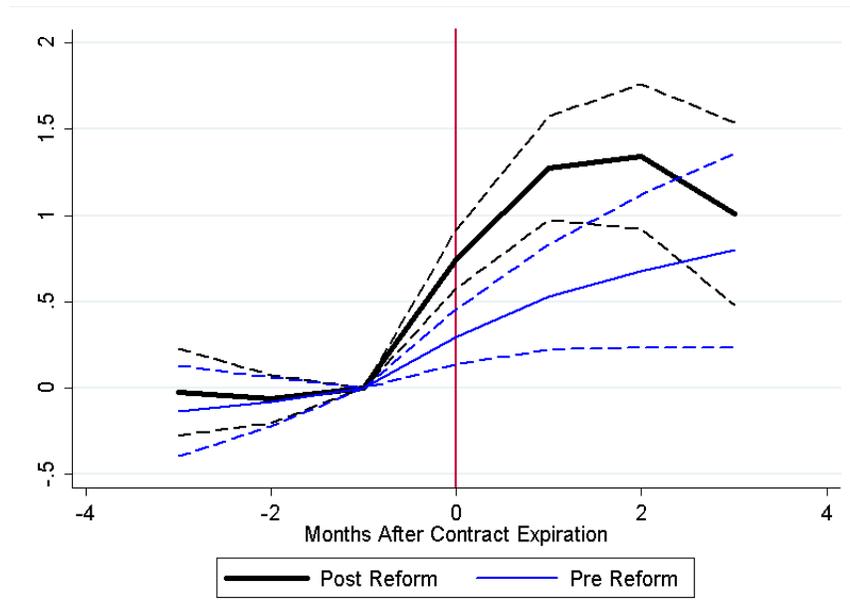
This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is country exits. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval.

Figure 6: Impact of a Contract Expiration on Country Exits: Pre- and Post-reform



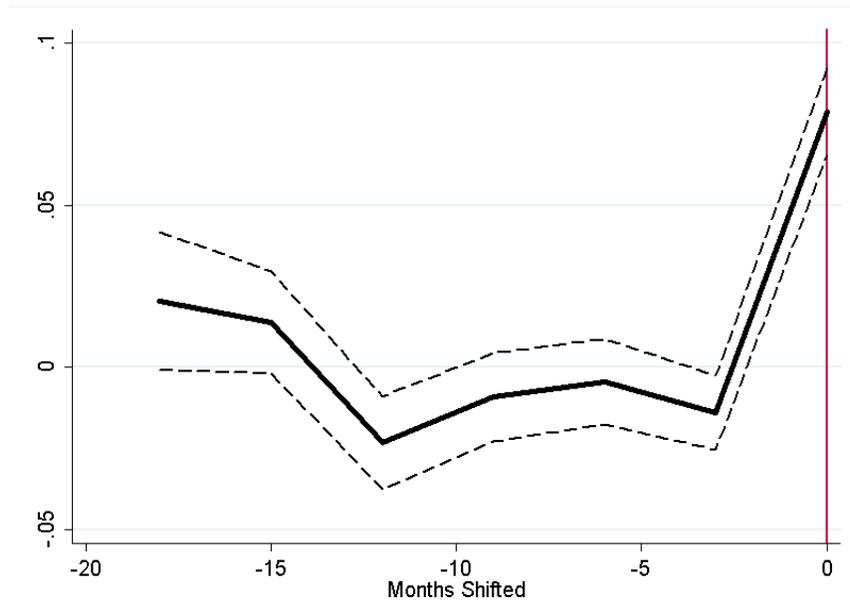
This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is country exits. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval.

Figure 7: Impact of a Contract Expiration on Employer Changes: Pre- and Post-reform



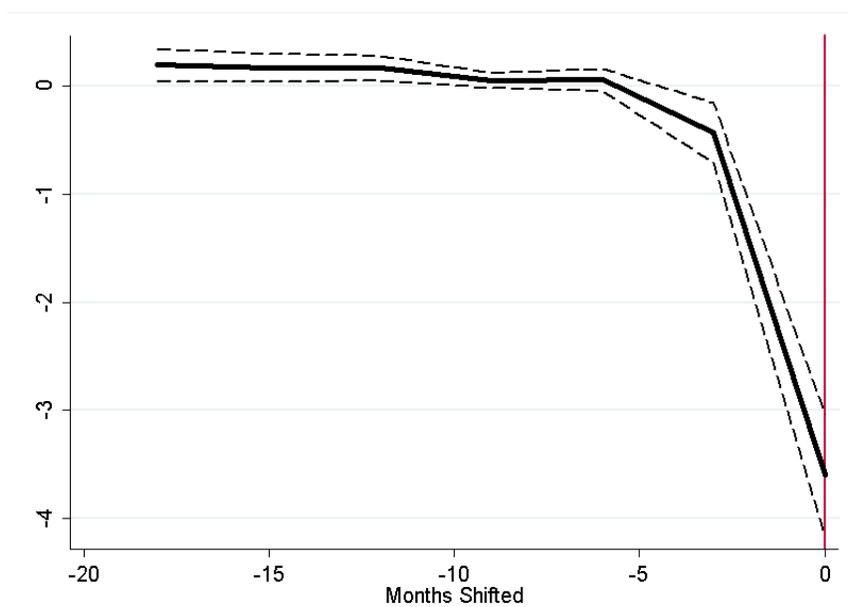
This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is employer changes. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval.

Figure 8: Effects of Time-Shifted Placebos on Log Earnings



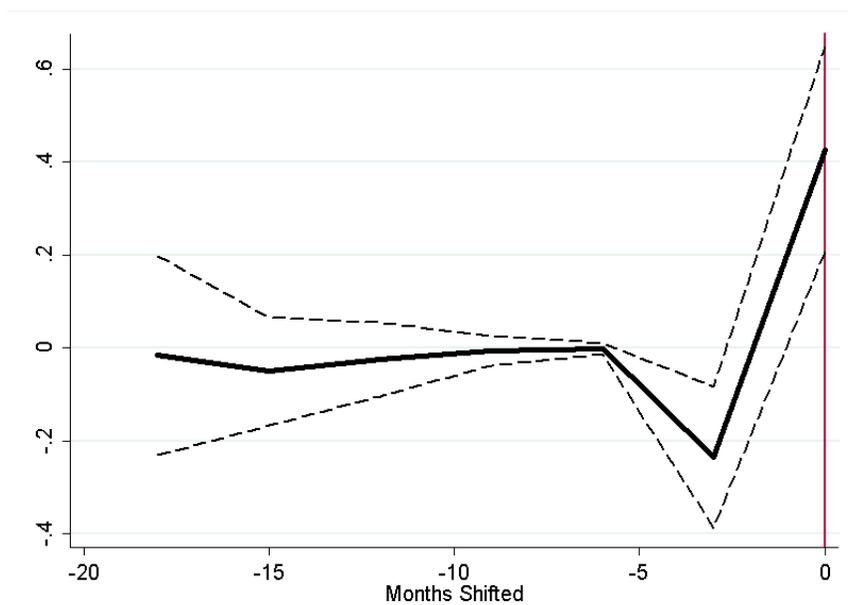
This displays the average 3-month post-contract expiration effect of the reform on log earnings where the timing of the contract expiration is shifted by the months given on the x-axis. The dotted lines give the 95% confidence interval.

Figure 9: Effects of Time-Shifted Placebos on Exits from the UAE



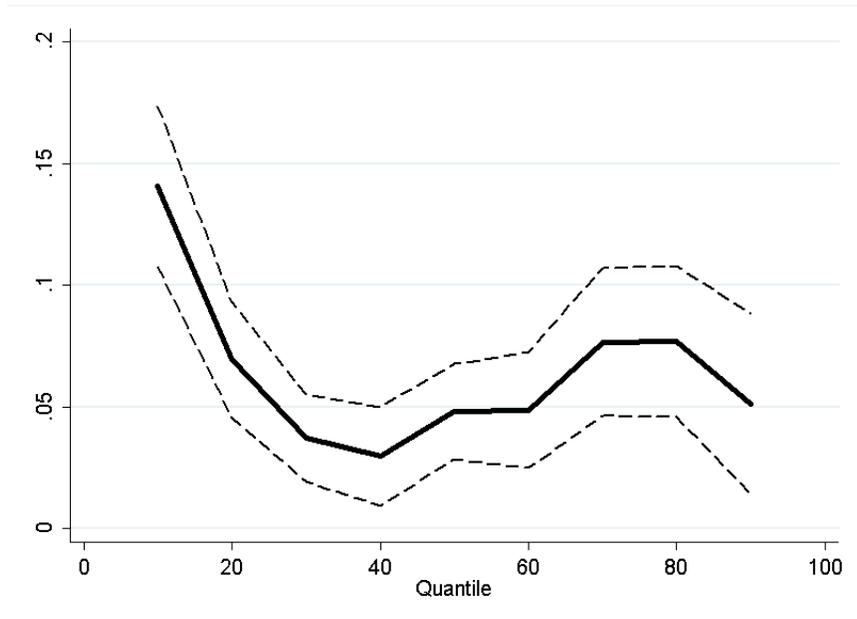
This displays the average 3-month post-contract expiration effect of the reform on country exits where the timing of the contract expiration is shifted by the months given on the x-axis. The dotted lines give the 95% confidence interval.

Figure 10: Effects of Time-Shifted Placebos on Employer Changes



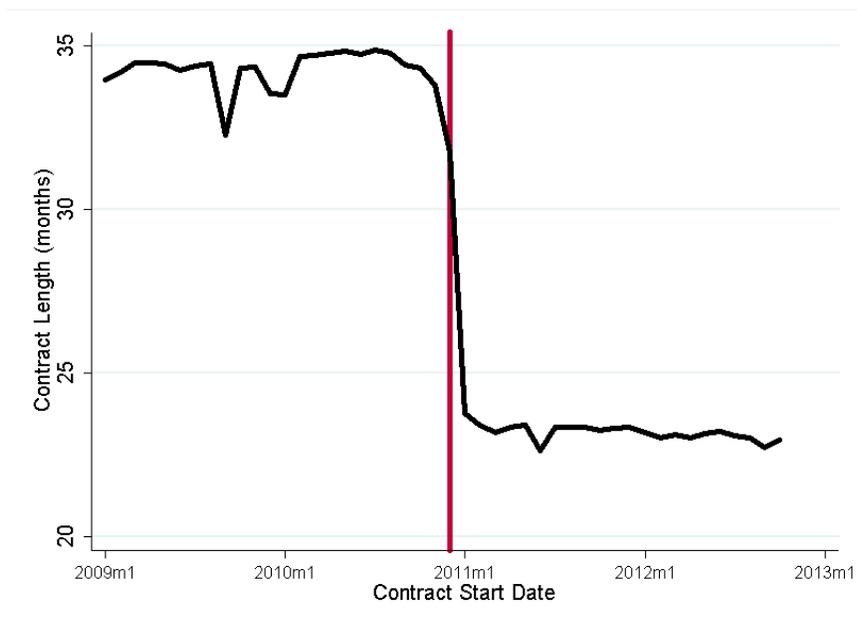
This displays the average 3-month post-contract expiration effect of the reform on employer changes where the timing of the contract expiration is shifted by the months given on the x-axis. The dotted lines give the 95% confidence interval.

Figure 11: Unconditional Quantile Estimates of Earnings



Notes: Each of the 10 points is a coefficient estimate from a quantile regression where the dependent variable is log earnings. The controls include polynomials in time to reform and worker characteristics. The dotted lines give the 95% confidence interval.

Figure 12: Average Contract Length by Contract Start Date



Sample: MOL data

Table 1: Summary Statistics By Person-Month

	Pre-Reform			Post-Reform		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Log Monthly Earnings	7.013	0.560	183543	7.039	0.652	345959
Stay with Firm (x100)	94.60	22.60	179656	95.84	19.97	346081
Exit UAE (x100)	4.852	21.49	192906	3.368	18.04	358027
Employer Change (x100)	0.194	4.400	177858	0.693	8.298	336748
Stayer	0.973	0.161	193972	0.962	0.192	361415
Construction	0.705	0.456	144524	0.565	0.496	217693
Age	36.68	8.304	193972	35.89	8.417	361415
Muslim	0.400	0.490	184307	0.434	0.496	347403
Male	0.998	0.0389	193972	0.994	0.0794	361400
Educated	0.276	0.447	181559	0.388	0.487	354998
Dubai-Sharjah	0.652	0.476	193960	0.604	0.489	361415
Indian	0.507	0.500	190617	0.446	0.497	358899
Hours (Lowerbound)	254.3	50.66	100246	238.2	54.62	202225
Hours (Upperbound)	264.3	60.67	100246	244.9	65.46	202225

Table 2: Effect on Log Earnings and Firm Retention

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Log Earnings								
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_0^{Pre}}{3}$)	0.113*** (0.009)	0.110*** (0.009)	0.109*** (0.011)	0.057*** (0.010)	0.091*** (0.010)	0.092*** (0.010)	0.139*** (0.012)	0.139*** (0.013)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.015*** (0.005)	0.013** (0.005)	0.016** (0.006)	0.031*** (0.006)	0.049*** (0.005)	0.053*** (0.006)	0.017*** (0.006)	0.019*** (0.007)
N	529502	529502	342555	342555	463312	306022	447394	288206
Number of Clusters	111006	111006	69239	69239	88290	57021	105606	66202
R-squared	0.017	0.017	0.023	0.087	0.010	0.015	0.019	0.024
Panel B: Staying with Firm								
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_0^{Pre}}{3}$)	3.832*** (0.304)	4.333*** (0.321)	6.387*** (0.405)	5.962*** (0.353)	3.106*** (0.282)	5.119*** (0.334)	4.270*** (0.393)	1.998*** (0.526)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	1.642*** (0.205)	1.881*** (0.224)	2.743*** (0.242)	2.585*** (0.214)	0.223** (0.114)	0.459*** (0.118)	1.759*** (0.266)	1.254*** (0.276)
N	525737	525737	343503	343503	466806	309909	445200	289675
Number of Clusters	110120	110120	68931	68931	88293	57023	105448	66472
R-squared	0.082	0.082	0.370	0.372	0.093	0.375	0.081	0.458
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table 3: Effect on Hours Variables

	Hours Upper Estimate		Hours Lower Estimate	
	(1)	(2)	(3)	(4)
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	-1.487 (1.194)	-0.094 (1.680)	-1.239 (0.995)	-0.078 (1.400)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-0.388 0.649	-0.820 0.911	-0.324 0.541	-0.683 0.759
N	302471	186812	302471	186812
Number of Clusters	72897	44295	72897	44295
R-squared	0.005	0.013	0.005	0.013

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Table 4: Effect on Exits from UAE and Employer Transitions

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Exits from UAE								
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	-4.408*** (0.271)	-4.749*** (0.287)	-6.608*** (0.370)	-6.356*** (0.329)	-3.570*** (0.255)	-5.290*** (0.314)	-4.756*** (0.355)	-2.419*** (0.486)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-1.822*** (0.184)	-1.983*** (0.200)	-3.094*** (0.227)	-2.972*** (0.201)	-0.201** (0.089)	-0.507*** (0.099)	-2.065*** (0.239)	-1.572*** (0.269)
N	550933	550933	356203	356203	477737	315506	465333	299705
Number of Clusters	111319	111319	69442	69442	88290	57021	106789	66983
R-squared	0.084	0.084	0.347	0.348	0.092	0.353	0.083	0.443
Panel B: Employer Changes								
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	0.663*** (0.102)	0.639*** (0.109)	0.255** (0.117)	0.269*** (0.102)	0.327*** (0.097)	0.182* (0.094)	0.633*** (0.129)	0.350** (0.144)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.491*** (0.070)	0.478*** (0.078)	0.311*** (0.076)	0.301*** (0.067)	-0.311*** (0.047)	-0.069 (0.043)	0.546*** (0.088)	0.377*** (0.091)
N	514606	514606	335281	335281	459035	304030	434276	281713
Number of Clusters	109388	109388	68495	68495	88293	57024	104121	65522
R-squared	0.006	0.006	0.153	0.156	0.008	0.142	0.006	0.223
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table 5: Effect on Imputed Outcome Variables

	Earnings Imp. High		Earnings Imp. Low		Emp. Change High		Emp. Change Low	
	Coarse (1)	Fine (2)	Coarse (3)	Fine (4)	Coarse (5)	Fine (6)	Coarse (7)	Fine (8)
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	0.185*** (0.009)	0.123*** (0.009)	0.031*** (0.009)	0.095*** (0.009)	0.644*** (0.098)	0.638*** (0.098)	0.558*** (0.098)	0.618*** (0.098)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.046*** (0.005)	0.020*** (0.005)	-0.019*** (0.005)	0.009* (0.005)	0.480*** (0.067)	0.479*** (0.067)	0.445*** (0.067)	0.469*** (0.067)
N	550920	550920	550920	550920	536024	536024	536024	536024
Number of Clusters	111319	111319	111319	111319	110337	110337	110337	110337
R-squared	0.016	0.007	0.012	0.022	0.005	0.005	0.005	0.005

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. Columns 1, 3, 5, and 7 are the coarse bounds described in the text, while Columns 2,4,6, and 8 are the finer bounds.

Table 6: Heterogeneous Effects on Log Earnings, Employer Transitions and Country Exits

	Indian (1)	Not Indian (2)	Educated (3)	Construction (4)	Dubai-Sharjah (5)	Age < 35 (6)
Panel A: Log Earnings						
(Post-Pre Reform) X Post Expire ($\sum_{k=1}^3 \gamma_k^{Post-\gamma_k^{Pre}}$)	0.169*** (0.014)	0.058*** (0.012)	0.062*** (0.018)	0.152*** (0.014)	0.074*** (0.011)	0.109*** (0.013)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.031*** (0.008)	-0.001 (0.007)	-0.004 (0.010)	0.031*** (0.008)	-0.001 (0.006)	0.003 (0.007)
N	242894	281171	178704	223739	327747	272723
Number of Clusters	51730	58104	38777	45304	67960	57862
R-squared	0.023	0.017	0.015	0.028	0.011	0.021
Panel B: Exits from the UAE						
(Post-Pre Reform) X Post Expire ($\sum_{k=1}^3 \gamma_k^{Post-\gamma_k^{Pre}}$)	-4.476*** (0.426)	-3.862*** (0.351)	-3.662*** (0.578)	-5.311*** (0.397)	-4.951*** (0.359)	-5.895*** (0.435)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-2.535*** (0.282)	-0.919*** (0.243)	-1.119*** (0.353)	-2.315*** (0.271)	-2.181*** (0.236)	-1.844*** (0.282)
N	254513	290595	185956	233322	342021	283944
Number of Clusters	51867	58266	38904	45406	68143	58022
R-squared	0.102	0.067	0.082	0.094	0.093	0.088
Panel C: Employment Transitions						
(Post-Pre Reform) X Post Expire ($\sum_{k=1}^3 \gamma_k^{Post-\gamma_k^{Pre}}$)	0.931*** (0.134)	0.370** (0.175)	1.102*** (0.252)	0.224** (0.111)	1.174*** (0.106)	0.845*** (0.159)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.738*** (0.095)	0.242** (0.112)	0.631*** (0.162)	0.444*** (0.083)	0.710*** (0.076)	0.712*** (0.106)
N	236339	272882	173046	219192	321830	263445
Number of Clusters	50916	57304	38230	45035	67582	56823
R-squared	0.007	0.006	0.012	0.003	0.008	0.007

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Table 7: Impact of the Reform on New Entrants' Outcomes

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Log Entrants					
(Post-Pre Reform) \times Log Contracts Expiring	-0.038*	-0.036*	0.017	-0.070***	-0.082***
	(0.021)	(0.021)	(0.013)	(0.023)	(0.029)
(Post-Pre Reform) \times Leads $(\frac{\sum_{k=-3}^{-1} \beta_k^{post} - \beta_k^{pre}}{3})$			-0.018**		
			(0.009)		
(Post-Pre Reform) \times Lags $(\frac{\sum_{k=1}^3 \beta_k^{post} - \beta_k^{pre}}{3})$			-0.011		
			(0.008)		
N	220752	220752	99425	118801	87918
Number of Clusters	17884	17884	9270	5557	5412
R-squared	0.133	0.142	0.040	0.158	0.150
Mean of Dep. Var.	0.139	0.139	0.110	0.159	0.162
Std. Dev. of Dep. Var.	0.466	0.466	0.410	0.515	0.526
Panel B: Log Entrant Earnings					
(Post-Pre Reform) \times Log Contracts Expiring	-0.034*	-0.034*	-0.015	-0.023	0.001
	(0.019)	(0.019)	(0.023)	(0.021)	(0.025)
(Post-Pre Reform) \times Leads $(\frac{\sum_{k=-3}^{-1} \beta_k^{post} - \beta_k^{pre}}{3})$			-0.022**		
			(0.010)		
(Post-Pre Reform) \times Lags $(\frac{\sum_{k=1}^3 \beta_k^{post} - \beta_k^{pre}}{3})$			0.013		
			(0.010)		
N	27437	27437	9302	15072	10881
Number of Clusters	12868	12868	3392	4849	3897
R-squared	0.010	0.024	0.027	0.009	0.007
Mean of Dep. Var.	7.065	7.065	7.007	7.052	7.042
Std. Dev. of Dep. Var.	0.753	0.753	0.749	0.715	0.714
Leads and Lags	No	No	Yes	No	No
City X Year-Month Fixed Effects	No	Yes	No	No	No

All specifications include firm and year-month fixed effects. Standard errors clustered by firm in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Table A.1: Effects for Stayers and with Firm Fixed Effects

	Stayers		Full Sample	
	(1)	(2)	(3)	(4)
(Post-Pre Reform) X Post Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_0^{Pre}}{3}$)	0.114*** (0.008)	0.109*** (0.010)	0.120*** (0.008)	0.107*** (0.010)
(Post-Pre Reform) X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.013*** (0.005)	0.015*** (0.006)	0.018*** (0.005)	0.015*** (0.006)
Polynomials in Time to Reform	No	Yes	No	Yes
Worker Characteristics	No	Yes	No	Yes
Firm Fixed Effects	No	No	Yes	Yes
N	513780	334269	529502	342555
R-squared	0.017	0.023	0.748	0.723

All specifications include individual fixed effects, year-month fixed effects and a constant term. The sub-sample of stayers refers to individuals who do not change firms. ***, **, * denotes significance at the 1, 5, and 10% levels, respectively.

Table A.2: All Coefficients in Estimates of Log Earnings

	Full Sample				Both Sides		Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	0.157*** (0.007)	0.160*** (0.008)	0.134*** (0.010)	0.071*** (0.008)	0.110*** (0.007)	0.096*** (0.009)	0.185*** (0.009)	0.165*** (0.012)
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	0.138*** (0.006)	0.141*** (0.006)	0.125*** (0.008)	0.058*** (0.007)	0.099*** (0.006)	0.090*** (0.007)	0.162*** (0.007)	0.152*** (0.009)
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	0.109*** (0.004)	0.111*** (0.005)	0.101*** (0.006)	0.049*** (0.005)	0.080*** (0.005)	0.075*** (0.006)	0.127*** (0.006)	0.122*** (0.007)
Contract Expire X Post Reform (γ_0^{Post})	0.012*** (0.003)	0.013*** (0.004)	0.011** (0.005)	0.025*** (0.004)	0.068*** (0.003)	0.067*** (0.004)	0.016*** (0.004)	0.017*** (0.005)
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	0.016* (0.009)	0.023** (0.009)	0.004 (0.010)	0.005 (0.009)	0.001 (0.010)	-0.012 (0.009)	0.013 (0.011)	0.002 (0.011)
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	0.019** (0.008)	0.024*** (0.007)	0.007 (0.008)	-0.005 (0.007)	0.001 (0.008)	-0.010 (0.008)	0.014 (0.009)	0.002 (0.009)
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	0.031*** (0.006)	0.035*** (0.006)	0.022*** (0.006)	0.008 (0.006)	0.014** (0.006)	0.007 (0.006)	0.030*** (0.007)	0.018** (0.007)
Contract Expire X Pre Reform (γ_0^{Pre})	-0.003 (0.004)	-0.000 (0.004)	-0.005 (0.005)	-0.005 (0.004)	0.019*** (0.004)	0.014*** (0.004)	-0.001 (0.005)	-0.002 (0.005)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
N	529502	529502	342555	342555	463312	306022	447394	288206
Number of Clusters	111006	111006	69239	69239	88290	57021	105606	66202
R-squared	0.017	0.017	0.023	0.087	0.010	0.015	0.019	0.024

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table A.3: All Coefficients in Estimates of Firm Retention

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	-10.935*** (0.214)	-11.065*** (0.227)	-6.100*** (0.289)	-5.975*** (0.257)	-7.969*** (0.204)	-4.624*** (0.239)	-10.452*** (0.275)	-5.877*** (0.323)
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	-12.855*** (0.195)	-12.961*** (0.209)	-7.493*** (0.313)	-7.397*** (0.280)	-10.560*** (0.190)	-6.305*** (0.271)	-12.196*** (0.246)	-7.181*** (0.343)
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	-10.190*** (0.163)	-10.268*** (0.177)	-5.818*** (0.254)	-5.890*** (0.228)	-8.016*** (0.159)	-4.622*** (0.208)	-9.490*** (0.206)	-5.368*** (0.278)
Contract Expire X Post Reform (γ_0^{Post})	-4.663*** (0.117)	-4.712*** (0.128)	-2.652*** (0.148)	-2.613*** (0.131)	0.357*** (0.068)	0.246*** (0.062)	-4.300*** (0.150)	-2.350*** (0.164)
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	-14.080*** (0.323)	-14.833*** (0.326)	-12.331*** (0.381)	-11.584*** (0.325)	-9.855*** (0.302)	-8.995*** (0.321)	-14.107*** (0.420)	-7.674*** (0.487)
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	-17.324*** (0.302)	-17.938*** (0.315)	-14.385*** (0.339)	-13.926*** (0.294)	-14.247*** (0.294)	-11.972*** (0.293)	-16.646*** (0.384)	-8.970*** (0.507)
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	-14.070*** (0.254)	-14.522*** (0.269)	-11.857*** (0.284)	-11.637*** (0.249)	-11.761*** (0.248)	-9.942*** (0.247)	-14.195*** (0.331)	-7.776*** (0.425)
Contract Expire X Pre Reform (γ_0^{Pre})	-6.304*** (0.172)	-6.593*** (0.183)	-5.395*** (0.192)	-5.197*** (0.169)	0.134 (0.095)	-0.213** (0.101)	-6.059*** (0.223)	-3.604*** (0.228)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
N	525737	525737	343503	343503	466806	309909	445200	289675
Number of Clusters	110120	110120	68931	68931	88293	57023	105448	66472
R-squared	0.082	0.082	0.370	0.372	0.093	0.375	0.081	0.458

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table A.4: All Coefficients in Estimates of Exiting the UAE

	Full Sample			Both Sides		Trimmed		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	9.464*** (0.190)	9.616*** (0.198)	5.613*** (0.274)	5.646*** (0.251)	7.408*** (0.180)	4.707*** (0.235)	9.080*** (0.242)	5.380*** (0.309)
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	11.344*** (0.174)	11.467*** (0.184)	7.103*** (0.294)	7.092*** (0.271)	9.748*** (0.169)	6.369*** (0.266)	10.777*** (0.218)	6.774*** (0.324)
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	9.189*** (0.144)	9.280*** (0.155)	5.699*** (0.231)	5.701*** (0.213)	7.324*** (0.142)	4.635*** (0.198)	8.609*** (0.180)	5.260*** (0.252)
Contract Expire X Post Reform (γ_0^{Post})	4.220*** (0.101)	4.278*** (0.110)	2.487*** (0.141)	2.504*** (0.125)	-0.085* (0.051)	-0.056 (0.057)	3.937*** (0.129)	2.209*** (0.158)
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	12.610*** (0.292)	13.185*** (0.300)	11.772*** (0.348)	11.354*** (0.300)	9.282*** (0.279)	8.927*** (0.297)	12.738*** (0.387)	7.409*** (0.443)
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	16.327*** (0.281)	16.795*** (0.297)	14.212*** (0.314)	13.971*** (0.273)	13.967*** (0.280)	12.217*** (0.277)	15.699*** (0.362)	9.037*** (0.472)
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	14.285*** (0.236)	14.631*** (0.252)	12.255*** (0.265)	12.183*** (0.233)	11.941*** (0.236)	10.437*** (0.235)	14.299*** (0.311)	8.225*** (0.408)
Contract Expire X Pre Reform (γ_0^{Pre})	6.042*** (0.156)	6.261*** (0.167)	5.580*** (0.178)	5.476*** (0.157)	0.116 (0.078)	0.451*** (0.081)	6.002*** (0.204)	3.781*** (0.223)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
N	550933	550933	356203	356203	477737	315506	465333	299705
Number of Clusters	111319	111319	69442	69442	88290	57021	106789	66983
R-squared	0.084	0.084	0.347	0.348	0.092	0.353	0.083	0.443

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table A.5: All Coefficients in Estimates of Employer Changes

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	0.973*** (0.104)	0.885*** (0.114)	0.541*** (0.116)	0.495*** (0.100)	0.476*** (0.101)	0.351*** (0.093)	0.806*** (0.129)	0.425*** (0.156)
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	1.315*** (0.093)	1.243*** (0.103)	0.715*** (0.105)	0.712*** (0.091)	0.930*** (0.092)	0.593*** (0.086)	1.164*** (0.116)	0.591*** (0.144)
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	1.263*** (0.080)	1.210*** (0.089)	0.664*** (0.089)	0.704*** (0.077)	0.903*** (0.079)	0.536*** (0.073)	1.097*** (0.099)	0.543*** (0.120)
Contract Expire X Post Reform (γ_0^{Post})	0.750*** (0.060)	0.715*** (0.067)	0.466*** (0.065)	0.448*** (0.057)	-0.292*** (0.041)	-0.097*** (0.036)	0.707*** (0.076)	0.422*** (0.082)
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	0.602*** (0.105)	0.544*** (0.073)	0.493*** (0.088)	0.445*** (0.077)	0.504*** (0.108)	0.403*** (0.076)	0.478*** (0.097)	0.272*** (0.099)
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	0.528*** (0.087)	0.480*** (0.063)	0.357*** (0.067)	0.350*** (0.059)	0.451*** (0.090)	0.294*** (0.058)	0.371*** (0.077)	0.111 (0.072)
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	0.432*** (0.069)	0.396*** (0.054)	0.307*** (0.058)	0.309*** (0.050)	0.372*** (0.070)	0.236*** (0.049)	0.319*** (0.065)	0.128*** (0.062)
Contract Expire X Pre Reform (γ_0^{Pre})	0.258*** (0.047)	0.236*** (0.040)	0.155*** (0.039)	0.147*** (0.035)	0.020 (0.039)	-0.028 (0.022)	0.161*** (0.045)	0.044 (0.043)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
N	514606	514606	335281	335281	459035	304030	434276	281713
Number of Clusters	109388	109388	68495	68495	88293	57024	104121	65522
R-squared	0.006	0.006	0.153	0.156	0.008	0.142	0.006	0.223

All specifications include individual fixed effects, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, *, denote significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table A.6: Other Firm Hiring Responses to the Reform

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Log Earnings					
(Post-Pre Reform) \times Log Contracts Expiring	0.020*** (0.005)	0.021*** (0.005)	0.005 (0.004)	0.023*** (0.006)	0.024*** (0.007)
(Post-Pre Reform) \times Leads $(\frac{\sum_{k=-3}^{-1} \beta_k^{post} - \beta_k^{pre}}{3})$			0.005** (0.002)		
(Post-Pre Reform) \times Lags $(\frac{\sum_{k=1}^3 \beta_k^{post} - \beta_k^{pre}}{3})$			0.004*** (0.001)		
N	222000	222000	103218	118991	87876
Number of Clusters	18073	18073	9551	5582	5438
R-squared	0.003	0.004	0.003	0.003	0.004
Mean of Dep. Var.	7.075	7.075	6.998	7.001	6.987
Std. Dev. of Dep. Var.	0.672	0.672	0.641	0.659	0.658
Panel B: Log Country Exiters					
(Post-Pre Reform) \times Log Contracts Expiring	-0.081*** (0.015)	-0.081*** (0.015)	-0.055*** (0.012)	-0.077*** (0.015)	-0.073*** (0.019)
(Post-Pre Reform) \times Leads $(\frac{\sum_{k=-3}^{-1} \beta_k^{post} - \beta_k^{pre}}{3})$			-0.030*** (0.006)		
(Post-Pre Reform) \times Lags $(\frac{\sum_{k=1}^3 \beta_k^{post} - \beta_k^{pre}}{3})$			0.010 (0.006)		
N	226785	226785	104213	121390	89596
Number of Clusters	18137	18137	9588	5596	5452
R-squared	0.052	0.095	0.092	0.065	0.051
Mean of Dep. Var.	0.168	0.168	0.249	0.241	0.262
Std. Dev. of Dep. Var.	0.471	0.471	0.582	0.574	0.597
Panel C: Log Transitioners					
(Post-Pre Reform) \times Log Contracts Expiring	0.007** (0.003)	0.007** (0.003)	0.009*** (0.003)	0.004 (0.003)	0.004 (0.004)
(Post-Pre Reform) \times Leads $(\frac{\sum_{k=-3}^{-1} \beta_k^{post} - \beta_k^{pre}}{3})$			0.002** (0.001)		
(Post-Pre Reform) \times Lags $(\frac{\sum_{k=1}^3 \beta_k^{post} - \beta_k^{pre}}{3})$			0.001 (0.001)		
N	226785	226785	104213	121390	89596
Number of Clusters	18137	18137	9588	5596	5452
R-squared	0.008	0.009	0.013	0.008	0.007
Mean of Dep. Var.	0.006	0.006	0.007	0.007	0.007
Std. Dev. of Dep. Var.	0.074	0.074	0.079	0.076	0.080
Leads and Lags	No	No	Yes	No	No
City X Year-Month Fixed Effects	No	Yes	No	No	No

All specifications include firm and year-month fixed effects. Standard errors clustered by firm in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Figure A.1: Density of Matched Payroll-MOL Log Earnings and Unmatched Payroll Log Earnings

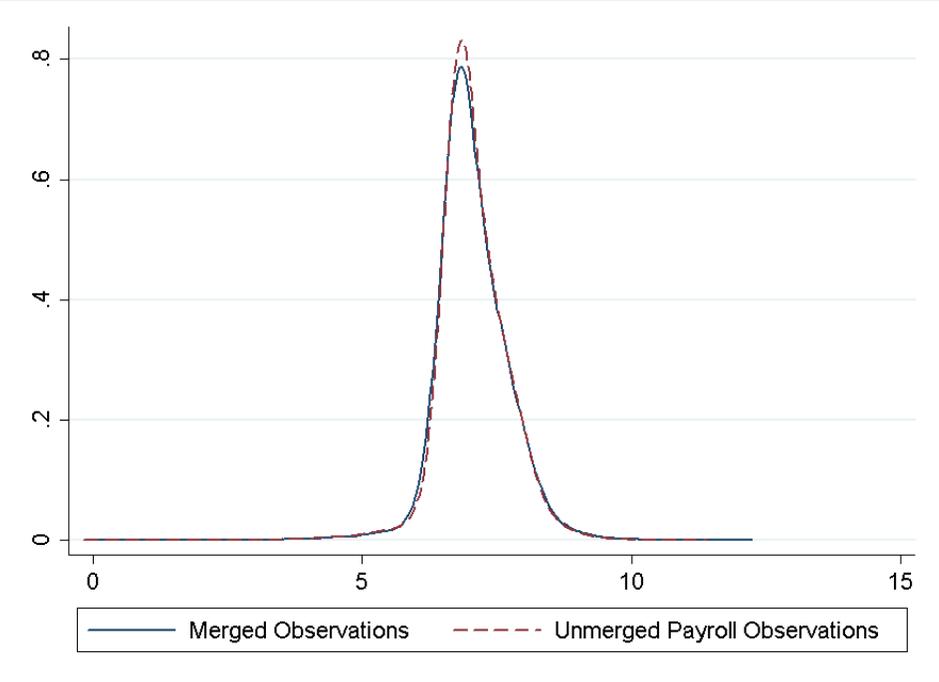


Figure A.2: Density of Matched Payroll-MOL Log Contract Salary and Unmatched MOL Log Contract Salary

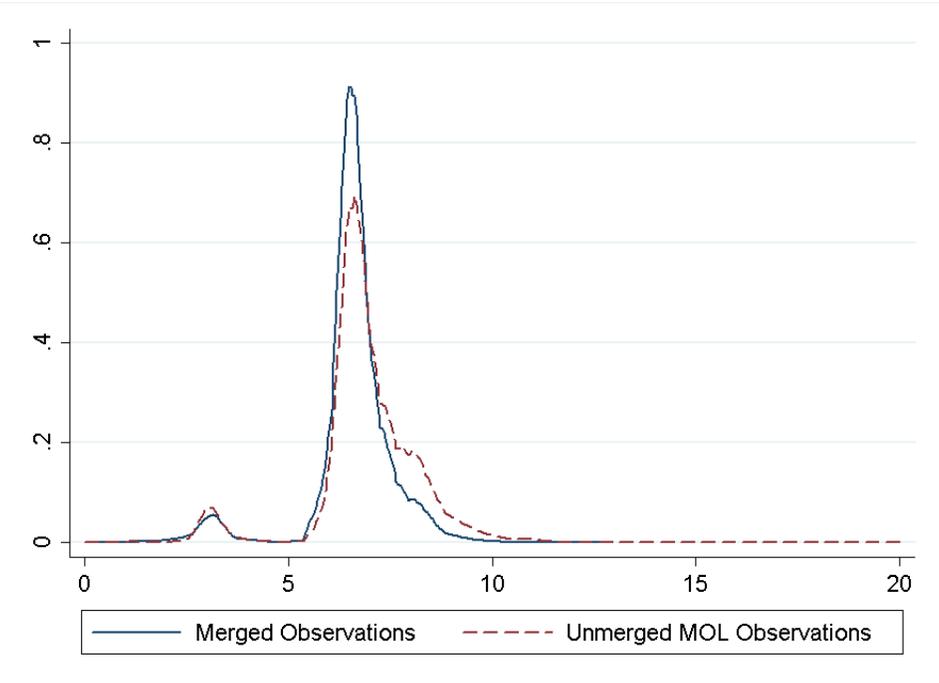


Figure A.3: Contract Compensation and Number of New Contracts Three Years Prior

