

Worker Mobility in a Global Labor Market: Evidence from the United Arab Emirates*

Suresh Naidu
Columbia

Yaw Nyarko
NYU

Shing-Yi Wang
Wharton

February 2015

Abstract

In 2011, a reform in the UAE 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 UAE 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.

Keywords: Migration, Job Mobility, Labor Market Competition, Labor Contracts, Monopsony, Middle East.

*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, Dartmouth, ETH-Zurich, LSE, MIT, NBER Summer Institute Labor/Personnel, Warwick, and Wharton. Afshan Aman, Victor Archavski, Michelle Han, Minkwang Jang, Goran Lazarevski, Stefanie Gude, and Qing Zhang all 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, Wharton Global Initiatives and Wharton Dean's Research Fund.

1 Introduction

Imperfect competition has been used by economists to explain a wide variety of labor market phenomena, including the employment effects of the minimum wage, the employer-size wage effect, race and gender wage gaps, agglomeration, and patterns in firm training (Manning 2011). Indeed, since Joan Robinson's 1933 analysis, imperfect competition in labor markets has been an important complement to the standard competitive model. However, credible estimates of the direct effect of monopsony on wages and employment, even in obviously non-competitive settings, have been lacking. This paper uses policy variation in migrant labor market in the United Arab Emirates (UAE) to estimate wage and labor-supply effects of increased labor market competition

Monopsony is particularly important in migrant labor markets, which offer potentially large welfare gains given large differences in wages across countries. Migrant labor markets in virtually all countries have restrictions on competition. Ruhs (2013) shows that countries, such as the UAE, that allow the most inflows of international migrants impose stricter restriction, via employer-specific visas, on migrant mobility across employers within the host country. For example, in the United States, many visas tie workers to particular employers, and do not allow immediate job-to-job transitions after a contract expires. This includes the H-2A agricultural visas, which are employer-specific, and until 2001, the H-1B skilled worker visas. These types of visas are often criticized by advocates for restraining labor market competition, lowering migrant wages, and facilitating labor rights violations.¹ Such visa policies, by restricting job-to-job transitions, can result in substantial monopsony power for firms, even as they may make migration economically and politically feasible.

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. When their employment contracts expired, workers had two options for remaining in the UAE: 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 to 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

¹e.g. <http://www.epi.org/publication/2b-employers-congressional-allies-fighting>

their previous employer, but only after their previous contract expired. We examine whether this policy 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 reforming a visa system that ties migrant workers to employers.

The labor restrictions in the UAE can also shed light on similar institutions in the U.S. and other countries. For example, 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 (Starr, Bishara, and Prescott 2015).² Non-compete clauses have been studied by scholars in sociology and law (Marx 2011, Lobel 2013), and recent lawsuits have alleged that American firms have signed anti-competitive agreements to not recruit each other's employees (Rosenblatt 2014). Restrictions on mobility have also been studied in the context of professional baseball players (Scully 1974), who were uniquely exempted from U.S. anti-trust law. 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).

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. 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 estimates of monopsony vary widely across studies, and this may be driven by the different approaches and by differences in the types of workers and markets. The bulk of the literature examines formal labor markets in advanced economies, yet the importance of job mobility and labor market competition is likely even greater in developing countries and immigrant labor markets, given lack of formal information sharing or institutionalized wage setting.

Theoretically, modern general equilibrium models of imperfect competition generally rely on

²Starr et al. as well as recent media coverage note that non-compete clauses are expanding into low skilled jobs in the U.S. as well. See for example Jamieson 2014.

search frictions 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 job mobility are important for explaining wage variation.³ 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.

In addition to the work on imperfect competition in labor markets, this paper contributes to the growing literature that considers the effects of international mobility on workers' outcomes.⁴ However, much less attention has been paid to the labor market restrictions that migrants face in their destination countries.⁵ A recent paper by Weyl (2014) argues that the restrictions faced by Gulf Cooperation Council (GCC) migrants are actually desirable given the substantially increased wages migrants receive relative to home country incomes.

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.

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 apply to workers after their contract expires post-reform. Standard contracts were uniformly

³See Rogerson, Shimer and Wright (2005) for a more complete review.

⁴See for example, Clemens 2012 and 2013, Gibson, McKenzie and Stillman 2011, McKenzie, Stillman and Gibson 2010.

⁵We 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.

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.

To examine the effects of the reform on potential migrants to the UAE, we present a different empirical strategy, one that combines 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 being 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. This is largely driven by the monthly probability of leaving the UAE at the end of a contract, which falls by about 4 percentage points. The monthly rate of employer transitions at the end of a contract also more than doubles, but remains below a percentage point. 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.

These benefits do not hold, however, for potential migrants: 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 incumbent migrants prior to the reform. Firms' monopsony power allows them to pay incumbents approximately 56% 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 as high as 73%.

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 private workforce in the United Arab Emirates (Forstenlechner and Rutledge 2011). Employers in the UAE recruit workers from around the world with the bulk coming 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. Fees are higher for new recruits than incumbent workers. 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.

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 firing decision 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 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

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/>

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 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 contract restrictions are enforced and are seen as a constraint by workers.

These kinds of restrictions are not new. Via the Colonial Office, 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 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 after 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

⁸<http://www.desertspeak.com/viewtopic.php?t=1911> Accessed February 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 Appendix Table A.7 indicate the results cannot be driven by a policy change in a single origin country.

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 on 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 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 theoretical framework demonstrates this intuition may not hold in general, and we provide a specific closed-form example in Appendix A.

We begin by defining a standard production function for each of N identical firms as $F(l_i^I, l_i^R)$ where incumbent workers retained from those already in employed by the firm are denoted l_i^I and new recruits from source countries l_i^R . Each firm is denoted by i and has access to its own recruitment network for new migrants. We suppose this production function satisfies the usual Inada conditions in both l_i^I and l_i^R . We further suppose that the number of workers already employed by the firm, including last period’s new recruits and incumbents, is taken as given as l_i^{t-1} but the firm can choose how many of these workers to retain, so we will require that $l_i^I \leq l_i^{t-1}$, although we assume that this constraint does not bind in equilibrium. In the short-run, the total number of workers in the labor market from last period is given by: $L^{t-1} = \sum_{j=1}^N l_j^{t-1}$.

Firms choose employment for 2 periods, but optimize period by period (myopically), given last period workers l_i^{t-1} . We assume workers are forward looking, risk neutral, and discount the second period at rate β . Let W denote the net present value (PV) of wages of our workers, who make decisions based on the 2 periods, the current and the next period. Workers who are incumbents will go back to their source country at a rate $q(W) \in (0, 1)$, reflecting heterogeneity in outside options, where q is decreasing and convex in W , as higher wages reduce the rate at which workers return to their source country.¹⁰ Define the complement of this function as the staying function, $s(W) = 1 - q(W)$, the fraction of the incumbent workers who stay as a function of W .

For incumbent workers, the present value of wages will be made up of the sum of the current and next period discounted earnings, $W = w^I + \beta w^{I,\text{exp}}$, where w^I denotes the current wage and $w^{I,\text{exp}}$ denotes the expectations of incumbent wages in the next period. We suppose that expectations of future incumbent wages are proxied by the current incumbent wages, which enables us to set $w^{I,\text{exp}} = w^I$. Thus, another component in the interpretation of β , besides time preference, is the subjective expectation that future wages as an incumbent will be identical to current incumbent wages.

The pre-reform labor supply of incumbents to firm i is given by:

$$l_i^I = s((1 + \beta)w_{pre}^I)l_i^{t-1} \quad \text{so} \quad w_{pre}^I = \frac{1}{1 + \beta} s^{-1} \left(\frac{l_i^I}{l_i^{t-1}} \right), \quad (1)$$

where we use subscripts pre and post to denote the pre- and post-reform values of wages and labor.

Next we turn to the labor supply of new recruits. New recruits anticipate becoming incumbents in the second period, and so the incumbent wage is an argument in the new recruits labor supply function. Because each firm has its own pool, or recruitment network, for new migrants, firms choose employment taking the labor supply function as given. The two-period present value of wages is $W = w^R + \beta w^{I,\text{exp}}$, where w^R is the current period wage of recruits and $w^{I,\text{exp}}$ is the expectation of future incumbent wages which, as discussed earlier, we set equal to current period incumbent wage, w^I . We let the function H be the supply function of recruits and $R(\cdot)$ be the inverse of the function H , which means we have:

$$l_i^R = H(W) = H(w_{pre}^R + \beta w_{pre}^I) \quad \text{so} \quad w_{pre}^R = R(l_i^R) - \beta w_{pre}^I \left(\frac{l_i^I}{l_i^{t-1}} \right). \quad (2)$$

¹⁰Allowing for individual heterogeneity in outside options is necessary simply for there to be a quit rate that is strictly greater than zero and less than one.

Firms choose the pre-reform quantity of incumbents and recruits monopsonistically. Both types of labor are therefore employed below their competitive level, as the firm forgoes higher levels of employment for a lower wage bill.

The reform corresponds to an increase in the labor market competition that firms experience. We model this as a change from firms having monopsony power over their incumbent workers to an oligopsonistic Cournot equilibrium.¹¹ Labor is free to move across firms but firms still retain some market power.¹² We assume that the reform does not alter the degree of competition in the market for new recruits. The post-reform Cournot competition is motivated by the fact that workers are relatively homogenous, and that many of the UAE sectors, such as construction, have relatively inelastic labor demand. Thus, firms compete in the labor market primarily with their choice of quantities.¹³

The quit (q) and staying (s) functions are the same post-reform, except that they now are determined by the aggregate labor market clearing condition in the economy rather than the firm's own labor stock. Thus,

$$\sum_{j=1}^N l_j^I = s((1 + \beta)w_{post}^I) \sum_{j=1}^N l_j^{t-1}. \quad (3)$$

Inverting this we get the post reform labor supply curve facing the firm, which relates the wage to the retention choices of all N firms, relative to the sum of existing workers:

$$w_{post}^I = \frac{1}{1 + \beta} s^{-1} \left(\frac{\sum_{j=1}^N l_j^I}{\sum_{j=1}^N l_j^{t-1}} \right). \quad (4)$$

Regarding new recruits, the post reform wages are still set monopsonistically, so the new recruits' wage equation is similar to the pre-reform case. The new recruits' wage equation takes into account the new incumbent wage process which is now determined oligopsonistically:

$$w_{post}^R = R(l_i^R) - \beta w_{post}^I. \quad (5)$$

The profit function of the firm is defined as output minus the wage bill:

$$\max_{l_i^I, l_i^R} \Pi(l_i^I, l_i^R) = \max_{l_i^I, l_i^R} F(l_i^I, l_i^R) - w^I l_i^I - (w^R + v^R) l_i^R \quad (6)$$

¹¹The predictions are not sensitive to the assumption of a Cournot equilibrium in the post-reform period. Naidu, Nyarko and Wang (2014) presents a more general reduced-form model of labor market competition in a static framework that leads to the same predictions.

¹²While labor can move freely across firms, the model assumes that all firms are identical so workers enjoy the wage gains associated with increased labor mobility without moving.

¹³Cournot quantity competition could also be a reduced-form representation of price competition with capacity constraints, as in Kreps and Scheinkman (1983). In that case firms could face short-run capacity constraints. In the UAE, these could be driven by the number of visa slots allocated to the firm by the government. These slots are rarely binding in the medium term but may be operative in the month-to-month variation we are examining.

where $v_R > 0$ is the non-wage cost of recruiting and hiring a new entrant. Note that w^I and w^R are functions of l_i^I, l_i^R , and l_i^{t-1} but we suppress the additional notation for convenience. The difference between firms' optimization outcomes in the pre- and post-reform periods will be reflected in the different wage functions $w^I(\cdot)$ and $w^R(\cdot)$ which are determined by the different assumptions on labor market competition. We will express the first-order conditions in terms of elasticities, denoting $\epsilon^I = \frac{l_i^{t-1} w^I}{l_i^I} \left(\frac{dw^I}{d(l_i^I/l_i^{t-1})} \right)^{-1}$ as the elasticity of the share of incumbents that stay with respect to incumbent wages and $\epsilon^R = \frac{R(l_i^R)}{l_i^R R'(l_i^R)}$ as the labor supply elasticity for new recruits with respect to the current wage for new recruits. The pre-reform first-order conditions are given by the following:

$$\frac{\partial \Pi}{\partial l^I} = 0 \implies F_{l^I}(l_{i,pre}^I, l_{i,pre}^R) = w^I \left(1 + \left(1 - \beta \frac{l_{i,pre}^R}{l_{i,pre}^I} \right) \frac{1}{\epsilon^I} \right) \quad (7)$$

$$\frac{\partial \Pi}{\partial l^R} = 0 \implies F_{l^R}(l_{i,pre}^I, l_{i,pre}^R) = w^R \left(1 + \left(\epsilon^R - \frac{\beta w^I(l_{i,pre}^I)}{R'(l_{i,pre}^R) l_{i,pre}^R} \right)^{-1} \right) + v_R. \quad (8)$$

These first-order conditions reflect that firms set the marginal product of each type of labor equal to its marginal cost. Due to the monopsony power of employers, the marginal cost of both types of labor is higher than the wage because each additional worker increases the wage paid to all inframarginal workers as well. Monopsonistic firms underemploy workers relative to the competitive equilibrium in order to keep wages low. As ϵ^I increases, marginal products approach wages. Similarly, the first-order condition for new recruits incorporates both the contemporary costs for new recruits ϵ^R , but also, via β , what recruits expect to get upon becoming incumbent workers in the future. When $\beta = 0$, we obtain the standard Lerner monopsony condition relating the gap between marginal product and wages to the inverse of the elasticity of labor supply facing the firm.

Next, we solve for the post-reform symmetric Cournot equilibrium. Assuming N identical firms and symmetry in firms' decisions, we will have $l_j^{t-1} = l_i^{t-1}$, $l_{j,post}^R = l_{i,post}^R$, and $l_{j,post}^I = l_{i,post}^I$ for all firms i, j . We have the following post-reform first-order conditions:

$$\frac{\partial \Pi}{\partial l^I} = 0 \implies F_{l^I}(l_{i,post}^I, l_{i,post}^R) = w^I \left(1 + \left(1 - \beta \frac{l_{i,post}^R}{l_{i,post}^I} \right) \frac{1}{N \epsilon^I} \right) \quad (9)$$

$$\frac{\partial \Pi}{\partial l^R} = 0 \implies F_{l^R}(l_{i,post}^I, l_{i,post}^R) = w^R \left(1 + \left(\epsilon^R - \frac{\beta w^I(l_{i,post}^I)}{R'(l_{i,post}^R) l_{i,post}^R} \right)^{-1} \right) + v_R. \quad (10)$$

The difference here from equations 7 and 8 is that in the Cournot equilibrium, the marginal cost of incumbent workers depends on the employment of all the other firms. Specifically, the

only difference between the pre- and post-reform first-order conditions is the $\frac{1}{N}$ term on the right-hand side of the first-order condition with respect to l^I . Therefore we can analyze the change induced by the reform on firm decisions regarding how many workers to keep by simply analyzing the effect of an increase in N , where the pre-reform solution is simply the post-reform solution at $N = 1$. Indeed, as N approaches infinity, the post-reform incumbent wages will approach marginal product. A sufficient condition for these first-order conditions to define a unique equilibrium is that the profit function is strictly concave, which is guaranteed by a sufficiently concave F and/or sufficiently convex w^I and w^R . We summarize the comparative statics with the following proposition, where we make the arguments of the wage functions explicit.

Proposition: If Π is strictly concave in l^I and l^R , new recruits and incumbent workers are substitutes in F , so that $F_{l^I l^R} < 0$, and if β is sufficiently small, we have the following comparative statics that result from the reform:

- The share of incumbent workers staying with a firm goes up: $\Delta s \equiv \frac{l_{i,post}^I - l_{i,pre}^I}{l_i^I} > 0$;
- Incumbent workers' wages rise: $\Delta w^I \equiv w^I\left(\frac{l_{i,post}^I}{l_i^I}\right) - w^I\left(\frac{l_{i,pre}^I}{l_i^I}\right) > 0$;
- Employment of new recruits fall: $\Delta l_i^R \equiv l_{i,post}^R - l_{i,pre}^R < 0$;
- Wages of new recruits fall: $\Delta w^R \equiv w^R\left(l_{i,post}^R, \frac{l_{i,post}^I}{l_i^I}\right) - w^R\left(l_{i,pre}^R, \frac{l_{i,pre}^I}{l_i^I}\right) < 0$.

Proof: This follows from implicitly differentiating equation 9 with respect to N . See Appendix A for details and an example.

An increase in competition, moving from monopsony to Cournot oligopsony, for incumbent workers will correspond to a decrease in the sensitivity of the wage paid by a firm to the labor hired by that firm, as employers must recruit from the pool of all incumbent workers, not just those who were recruited by the firm. This reduces the marginal cost of incumbent workers (despite raising their wage), which lowers the marginal profitability of new recruits, and so the number of new recruits falls. The wages of incumbent workers rise, while the wages of new recruits fall. In the case of incumbent workers, this is the distinctive monopsony prediction: 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

¹⁴This prediction reflects Bresnahan's (1982) argument on identifying market power. Bresnahan argued that

How the relative share of incumbent workers and new recruits change depends on both their relative marginal productivities as well as the importance of forward looking behavior by new recruits (given by the magnitude of β).¹⁵ First, given the substitutability of the two types of labor, labor demand for new recruits and their corresponding wages fall as the marginal cost of incumbent workers has decreased. Second, the higher wages for incumbent workers allow firms to pay forward-looking new recruits less, because new recruits anticipate earning more after their first period in the UAE (given by βw^I). However, under the assumption that β is small, the second effect is dominated by the first effect.¹⁶

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.

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. The number of other firms an employer is competing with to retain incumbent workers, which goes from 0 to $N - 1$, is such a variable.

¹⁵In this model, overall firm size may fall or rise as a result of the reform. Even if the two types of labor are perfect substitutes, if incumbent workers are more productive by a factor $A > 1$, so that $F(l^I, l^R) = F(A l^I + l^R)$, then the decrease in the quantity of new recruits can lead overall firm size $l^I + l^R$ to fall even as $A l^I + l^R$ rises.

¹⁶For large values of β , the employment of new recruits may rise rather than fall because the anticipation of higher wages after the first period offsets the fall in wages in the first period.

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.¹⁷ 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.

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 in which 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.¹⁸ 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

¹⁷In 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.

¹⁸However, a limitation of the data is that we cannot distinguish voluntary worker separations (quits) from involuntary separations.

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.¹⁹ 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.²⁰ It would 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 B.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

¹⁹Freezones 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.

²⁰Total compensation includes the value specified in the contract for housing, food and transportation.

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 in the growth rate of compensation for a worker who stays in the UAE after a contract expiration following the reform.²¹

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 that are retained by their existing employers after the reform.²² Figure 3 shows the total number of employer transitions that occur at the end of a contract by the expiration date of the contract. Employer transitions prior to the reform are those for which workers received a NOC within 3 months of contract expiration. This figure provides immediate evidence that employer transitions increased following the reform. Together, these figures are consistent with workers being more likely to remain within the UAE after the reform.²³

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 generally 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.²⁴ 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

²¹Note 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.

²²Figures 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).

²³The MOL data do not directly indicate when migrants leave the UAE so we do not present a corresponding figure with country exits.

²⁴We 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.

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 a 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 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.²⁵ 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 unconditional 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.²⁶ 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.²⁷ The pool of educated workers increases substantially after the reform. 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

²⁵See Appendix B for more information on the construction of this and other variables.

²⁶Each 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.

²⁷Intermediate education is classified as some secondary schooling without completing the degree.

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.²⁸ The upper 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.²⁹ 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 four 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

²⁸That hours was the primary source of earnings variation was confirmed by conversations with UAE Exchange officials.

²⁹UAE 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 take place.

began in 2007 and 2008, respectively, and this precedes serious discussion of such reforms in the UAE.³⁰ Our specification also includes individual fixed effects which should remove any time-invariant differences across the groups. Finally, we also estimate a specification that controls for quartic polynomials in the time between contract expiration and the reform, which controls for other unobserved heterogeneity associated with the timing of contract expiration.

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 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.³¹ 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:

$$y_{it} = \sum_{k=-3}^3 \gamma_k^{Post2011} D_{it+k}^{Post} + \sum_{k=-3}^3 \gamma_k^{Pre2011} D_{it+k}^{Pre} + \delta_i + \delta_t + \epsilon_{it} \quad (11)$$

where y_{it} denotes the outcomes of interest for incumbent worker i in year-month t : 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, with a superscript denoting whether the contract is expiring before or after the reform. 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

³⁰Using 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.

³¹This is true regardless of whether they change employers or not.

$\gamma^{Post2011} - \gamma^{Pre2011}$. 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 11. 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 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 11 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 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:

$$y_{it} = \sum_{k=0}^3 \gamma_k^{Post2011} D_{it+k}^{Post} + \sum_{k=0}^3 \gamma_k^{Pre2011} D_{it+k}^{Pre} + \delta_i + \delta_t + \epsilon_{it}. \quad (12)$$

The key difference from equation 11 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 equivalent to a difference-in-differences estimate and tests whether outcomes are different following a contract expiration after the reform as compared with before the reform. We are also interested in testing $\gamma_0^{Post2011} - \gamma_0^{Pre2011} = 0$, which we show in the third row of each panel; however, given variation across individuals in exactly when within the month contracts expire, this coefficient may capture both pre and post-contract expiration weeks.

In addition to the basic specification given in equation 12, 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 effects due to timing of the contract expiration date relative to the reform. For example, workers may differentially exit the UAE depending on when their contracts expire close to the reform date.

We next include a vector of worker characteristics (age, Indian citizenship, education) interacted with year-quarter fixed effects.³² 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.

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, *Post Reform X Post Contract Expire*, which corresponds to $\sum_{k=1}^3 \gamma_k^{Post2011} - \gamma_k^{Pre2011}$ in equation 12, and *Post Reform X Period Contract Expire*, which corresponds to $\gamma_0^{Post2011} - \gamma_0^{Pre2011}$, for log earnings.³³ 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.³⁴ 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.

³²The 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.

³³Appendix 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.

³⁴Note the pooled average effect is given by $\sum_{k=1}^3 \frac{\gamma_k^{Post2011} - \gamma_k^{Pre2011}}{3}$.

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. These estimates are all significant at the 1% level.

Column 4 corresponds with column 1 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%. Column 5 estimates 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 the other estimates, 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. Given that the rate of employer transitions is extremely 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 0.49 percentage point increase in the probability of an employer transition during the month of a contract expiration, with an extra 0.66 percentage point increase per month on average over the next 3 months.³⁵ 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 time to reform 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.

In column 4, 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 an increased probability of a transition 1 and 2 months after the contract expired. In column 5, 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 the other estimates. 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 transitions remains relatively low. In equilibrium, as firms adjust their payments to workers, the

³⁵Looking at the individual lags in Appendix Table A.5, 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.

threat of changing employers can have effects on earnings without actual transitions.³⁶ 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. Informal collusion between employers or illegal withholding of worker passports could restrict mobility even in the absence of legal constraints. 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.³⁷ We implement two bounds, one wider and thus more conservative than the other more “naive” bounds.³⁸ 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

³⁶An analogy can be found in no-fault divorce laws, which increased settlement payments but did not appreciably increase the divorce rate (Peters 1986).

³⁷This selection can include exiting the UAE entirely, entering the informal market or working in freezones outside of the jurisdiction of the MOL.

³⁸The naive bounds simply assign \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. The results are shown in Appendix Table A.6

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 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.

Table 5 shows the coefficients of equation 12 using the imputed values of earnings and transitions. The table shows the estimates of the baseline specification without controls for the imputation of log earnings that recovers an upper bound on the coefficients of interest in the odd columns, while the even columns show 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 18%, while the lower bound is 3%. Thus, both remain positive and statistically significant at the 1% level. While the 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.

Columns 3 and 4 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 12, replacing D_{it} with $\tilde{D}_{it}^j \equiv D_{i,t-3j}$, where j runs from 0 to 6. We shift the contract expiration dates in this way but these shifts do not alter whether we treat the contract expiration as post-reform or pre-reform.

Appendix Figure A.4 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. Appendix Figure A.6 shows the same plot for employment changes, while Appendix Figure A.5 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 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.

We begin by estimating the following equation:

$$y_{jt} = \beta^{Post2011} \log \text{Expire}_{jt}^{Post2011} + \beta^{Pre2011} \log \text{Expire}_{jt}^{Pre2011} + \delta_j + \delta_t + \epsilon_{jt} \quad (13)$$

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 $\beta^{Post2011} - \beta^{Pre2011}$.

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, the 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. To do this, we include three leads and lags of contract expirations:

$$y_{jt} = \sum_{k=-3}^3 \beta_k^{Post2011} \log \text{Expire}_{jt+k}^{Post2011} + \sum_{k=-3}^3 \beta_k^{Pre2011} \log \text{Expire}_{jt+k}^{Pre2011} + \delta_j + \delta_t + \epsilon_{jt} \quad (14)$$

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 6 shows the estimate of *Post Reform X Log Contracts Expiring*, which corresponds to $\beta^{Post2011} - \beta^{Pre2011}$. 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 14. 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 6 presents the impact of the reform on hiring new entrants to the UAE.³⁹ The outcome is the logarithm of the number of workers hired that month who are entering the UAE for the first time. 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. The parsimonious estimates and the estimates that control for city by year-month fixed 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

³⁹We also examine whether the types of new entrants to the UAE changes with the reform. Appendix table A.8 shows the characteristics of new entrants are very similar before and after the reform.

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 6.⁴⁰ The estimates in columns 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 the high substitutability of workers 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.

8 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 is ϵ^I , the labor supply elasticity facing the monopsonistic firm. Because this only applies to incumbent workers, it can also be thought of as a retention elasticity.⁴¹

In our model, the function $w^I \left(\frac{l_i^I}{l_i^{I-1}} \right)$ and the corresponding elasticity $\epsilon^I \left(\frac{l_i^I}{l_i^{I-1}} \right)$ are the same before and after the reform. The model captures the reform as a reduction in the influence any single employer's choice of employment has on the wage that employer pays, but the overall

⁴⁰Table A.9 presents the firm-level estimates that correspond to the outcomes examined in the worker-level regressions. For parsimony, we omit the estimates for firm retention as they are very similar to country exits. The direction and significance of the estimates are generally consistent with the individual-level results.

⁴¹Given the low baseline level of quits, we report retention rather than the quit elasticities found in other papers (Manning (2011)).

labor supply curve (or quit function) stays the same. Given the upward-sloping labor supply curve, the increase in the wage is purely driven by an increase in employment. Thus, a local approximation of ϵ^I is recovered from the change in w^I relative to the change in l_i^I , holding l_i^{t-1} fixed. We can estimate this simply as $\epsilon^I = \frac{\Delta s(w^I)}{s(w_{pre}^I)}$. From the worker-level estimates, we have that the increase in the wage for a worker experiencing a contract expiration is $\Delta \log w^I = 0.11$.⁴² The total percentage increase in the probability of staying with the firm, $\frac{\Delta s(w^I)}{s(w_{pre}^I)}$, is 0.12.⁴³ This yields a labor supply elasticity for incumbent workers of 1.1.

This is a low labor supply elasticity, but this may not be exactly the labor supply elasticity faced by the monopsonist in our model because it doesn't take into account β . If $\beta > 0$, then ϵ^I must be divided by $1 - \beta \times \frac{l_{pre}^R}{l_{pre}^I} = 1 - \beta \frac{0.14}{0.83}$ to obtain the effective labor supply elasticity facing the pre-reform firm.⁴⁴ With an annual $\beta = .95$, compounded over 3 years, we get that the effective labor supply elasticity is given by $1.1 / (1 - 0.144) = 1.27$. More intuitively, this means that workers were only paid 56% of their marginal product in the pre-reform period.

By increasing the effective N faced by firms in the labor market, the reform increases the labor supply faced by the firm (rather than the elasticity in the total labor market) from ϵ^I to $N\epsilon^I$. We can get a sense of the size of the labor market by looking at the number of different firms that workers transition to from a given firm following the reform. The average number of other firms that a firm's workers transition to, conditional on a transition, is 1.4.⁴⁵ Adding this to the pre-reform level of 1 implies taking 2.4 as a measure of the post-reform effective labor market size, N . This suggests that the effective labor supply elasticity facing the firm after the reform is $\frac{N\epsilon^I}{1 - \beta(l_{post}^R/l_{post}^I)} = \frac{2.4 \times 1.1}{1 - 0.86(\frac{.025}{.96})} = 2.7$.⁴⁶ In other words, incumbent workers were paid 73% of their marginal product after the reform.

Also of interest is ϵ^R , which captures the degree of monopsony power for new recruits. Given the assumptions of the model, we cannot say very much about ϵ^R in general. The reform affects both the labor demand as well as the labor supply of new recruits. By increasing retention of incumbent workers, the reform reduces demand for new recruits. However, by raising expected future wages for new recruits, it also increases labor supply. The resulting changes in quantities and wages thus reflect both these changes. However, it may be that the dynamic effects are

⁴²Specifically, this estimate is from the first row and column of Panel A of Table 2.

⁴³To get this, we multiply the coefficient estimate from the first row and column of Panel B of Table 2, 3.8%, by 3 (the number of months post-expiration) and divided by 95, which is the average rate of staying.

⁴⁴The values of l_{pre}^R and l_{pre}^I are summary statistics of firms' average share of new entrants and the share of stayers in each month in the pre-reform period.

⁴⁵This is a summary statistic calculated from the data.

⁴⁶The values of l_{post}^R and l_{post}^I are summary statistics of firms' average share of new entrants and the share of stayers in each month in the post-reform period.

small (and β is close to zero).⁴⁷ Assuming away dynamic considerations (by setting $\beta = 0$) and allowing for the elasticity ϵ^I to vary pre- and post-reform, we calculate parameters for labor market power for new recruits and incumbents in Appendix Section C.3. In that Appendix we also discuss the relationship of both ϵ^I and ϵ^R to the structural parameters of models of dynamic monopsony.

While our context is quite different from other labor markets, our set of estimates is within or close to the range of elasticities of 1 to 1.9 reported by Falch (2010) for Norwegian teachers and 2 reported by Dal Bo, Finan, and Rossi (2014), although much larger than the 0.1 elasticity reported for VA 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 estimate implies that workers were getting about half of their marginal product prior to the reform rising to almost three-quarters after the reform.⁴⁸ While still 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.

9 Alternative Explanations

9.1 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, our results indicate that highly educated workers experience a smaller wage increase following the reform than less educated workers (Column 3 of Appendix Table A.7).⁴⁹ The quantile estimates discussed in Appendix Section C.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

⁴⁷Indeed, when we include incumbent wages as a control in our estimates from Table 6, the results are extremely similar, suggesting that at least contemporary incumbent wages are not drastically altering the labor supply of new recruits.

⁴⁸The post-reform estimates are similar to the 70-85% of marginal product found by Isen (2013) in the United States, and more than the 48% of marginal product recovered for United States slaves in 1860 by Vedder (1978).

⁴⁹Appendix Section C presents heterogeneity in the results by a variety of worker characteristics.

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 an increase in labor market competition is the primary explanation for the effects.

9.2 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.⁵⁰

Using the data on all contracts in the MOL database, Figure 8 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.⁵¹ 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

⁵⁰The last column of each panel of Table A.7 demonstrates that the results remain similar for workers younger than 35 and are not driven exclusively by old workers.

⁵¹Note 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.

migrants in the UAE.⁵² 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.

9.3 Other Issues

Another potential interpretation is that workers are not exiting to their source country but in fact are remaining in the UAE 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).⁵³

One possible concern is that the results on earnings are not reflective of the total compensation of workers or job quality.⁵⁴ 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

⁵²This story in itself would not lead to an increase in employer transitions among incumbent workers.

⁵³An 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. If almost all of the workers in the UAE took advantage of the amnesty, this would imply that roughly 5% of the workforce in the UAE 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.

⁵⁴This alternative interpretation could only potentially affect the results for earnings and not the findings on transitions and exits.

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 resources or manpower are in the name of the firm. We see no break in the trends in firm size or contract earnings 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 that 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 A.7 present 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.

10 Conclusion

The reform in the UAE that allowed any employer to renew a migrant's visa upon contract expiration without written permission from the initial employer provides a unique opportunity to study the impact of increased labor market competition in the workers' labor market outcomes. We estimate that the policy reduced the monopsony power of firms such that the share of the marginal product being paid to incumbent workers increased from 56% to 73%.

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 affected both workers in the country as well as individuals from other countries who wish to migrate. The 2011 labor reform 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 those new entrants did not experience an increase in earnings in their initial contracts following the reform.

Our results also 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. In other words, improving competition and wages for relatively high-earning incumbent migrants may come at the expense of relatively low-earning potential migrants with ambiguous effects on the overall level of migration. Which normative criteria is chosen has implications for evaluating labor market policies governing migrant workers. What weight foreign migrant welfare should have in the objective function of a government choosing policies is itself a debated question (Ruhs 2013). Even beyond this, Weyl’s (2014) idea of placing a large weight on the potential migrant population given that they are extremely poor relative to natives would judge the reform less positively than a normative perspective that highly weights Mankiw’s (2010) criteria of “just deserts” in which workers should be paid their marginal products.

These results offer insight into many other types of labor markets where workers sign contracts that tie themselves to employers, and are relevant for several current legal and policy debates. For example, immigration reformers in the U.S. have recently called for a clause allowing H-class visa holders a 6-month interval whereby they can search for a new employer without having to return to their source country. In addition, the American court system is considering cases where large firms, including Apple and Google, have signed agreements not to poach each others employees. Non-compete clauses in U.S. labor contracts may cover as many as 12.3% of the workforce, with up to 30% in fields such as engineering (Starr et al. 2015).

Additional research is needed to fully understand the implications of similar reforms. More GCC countries, such as Saudi Arabia and Qatar, are contemplating analogous 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, such reforms could potentially affect firm choices and outcomes that we cannot observe in our data sets. While the model suggests some firm outcomes (e.g., profits and 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 Model Appendix

A.1 Proofs

Proof of Proposition 1 From equation 9, we have l^I and l^R as functions of N , noting that the first-order conditions for the pre-reform period correspond to $N = 1$. We then implicitly differentiate equation 9 with respect to N . Define $C = \Pi_{l^I l^I} \Pi_{l^R l^R} - (\Pi_{l^I l^R})^2$ which is positive by the second-order condition at the maximum of the strictly concave profit function. The Jacobian of the first-order conditions yields a system of linear equations in $\frac{\partial l^I}{\partial N}$ and $\frac{\partial l^R}{\partial N}$ and applying Cramer's rule to solve for these we have that:

$$\frac{\partial l^I}{\partial N} = - \frac{\begin{vmatrix} \Pi_{l^I N} & \Pi_{l^I l^R} \\ \Pi_{l^R N} & \Pi_{l^R l^R} \end{vmatrix}}{\begin{vmatrix} \Pi_{l^I l^I} & \Pi_{l^I l^R} \\ \Pi_{l^R l^I} & \Pi_{l^R l^R} \end{vmatrix}} = - \frac{\begin{vmatrix} \Pi_{l^I N} & \Pi_{l^I l^R} \\ \Pi_{l^R N} & \Pi_{l^R l^R} \end{vmatrix}}{C} \quad (15)$$

$$\frac{\partial l^R}{\partial N} = - \frac{\begin{vmatrix} \Pi_{l^I l^I} & \Pi_{l^I N} \\ \Pi_{l^R l^I} & \Pi_{l^R N} \end{vmatrix}}{\begin{vmatrix} \Pi_{l^I l^I} & \Pi_{l^I l^R} \\ \Pi_{l^R l^I} & \Pi_{l^R l^R} \end{vmatrix}} = - \frac{\begin{vmatrix} \Pi_{l^I l^I} & \Pi_{l^I N} \\ \Pi_{l^R l^I} & \Pi_{l^R N} \end{vmatrix}}{C}. \quad (16)$$

But note that $\Pi_{l^R N} = 0$, and $\Pi_{l^I N} = \frac{w^I(1-\beta \frac{l^R}{l^I})}{N^2 \epsilon^I} > 0$

$$\begin{vmatrix} \Pi_{l^I N} & \Pi_{l^I l^R} \\ \Pi_{l^R N} & \Pi_{l^R l^R} \end{vmatrix} = \Pi_{l^I N} \Pi_{l^R l^R} = \Pi_{l^R l^R} \frac{w^I(1-\beta \frac{l^R}{l^I})}{N^2 \epsilon^I} < 0 \quad (17)$$

and thus $\frac{\partial l^I}{\partial N} > 0$.

And similarly we have

$$\begin{vmatrix} \Pi_{l^I l^I} & \Pi_{l^I N} \\ \Pi_{l^R l^I} & \Pi_{l^R N} \end{vmatrix} = -1 \times \Pi_{l^R l^I} \Pi_{l^I N} = -\Pi_{l^R l^I} \frac{(1-\beta \frac{l^R}{l^I})w^I}{N^2 \epsilon^I} > 0. \quad (18)$$

The last inequality follows since $F_{l^R l^I} < 0$ and β is small. Thus we have that $\frac{\partial l^R}{\partial N} < 0$. The wage results then follow from the definition of w^I and w^R .

A.2 Example

In this section, we provide a closed form solution of our model, with linear R and s functions. Our unambiguous empirical predictions hold only when β is small, and so we impose $\beta = 0$ at the end for tractability. We define:

$$F(l^R, l^I) = l^R + l^I - \sigma l^R l^I. \quad (19)$$

We impose $\sigma \in (0, 1)$. While this F does not satisfy the Inada conditions, it does have negative cross-partials, making the two types of labor substitutes. The resulting marginal products are given by:

$$F_{l^R} = 1 - \sigma l^I \quad \text{and} \quad F_{l^I} = 1 - \sigma l^R. \quad (20)$$

We adopt the simplest functional forms for R and s , with the labor supply of new recruits given by:

$$H(W) = w^R + \beta w^I \quad \text{so} \quad R(l^R) = l^R; \quad R'(l^R) = 1. \quad (21)$$

And the rate at which incumbent workers stay with the firm is identical to the wage:

$$s(W) = W = (1 + \beta)w^I, \quad (22)$$

so that

$$s^{-1}(x) = x, \quad s^{-1'}(x) = 1. \quad (23)$$

Note that since equilibrium W , which is $(1 + \beta)w^I$, is going to be less than the marginal product of l^I , which is given by $1 - \sigma l^I < 1$, we do not have to worry about $s(W)$ falling outside the $(0, 1)$ range for any interior solution. We also impose that $v_R \in (0, 1)$. Also note that $\epsilon^I = 1$ and $\epsilon^R = 1$. Recalling that $\epsilon^I \left(\frac{l^I}{l^{I-1}} \right) = \frac{l^{I-1}}{l^I} w^I \left(\frac{l^I}{l^{I-1}} \right) \left(\frac{dw^I \left(\frac{l^I}{l^{I-1}} \right)}{d \left(\frac{l^I}{l^{I-1}} \right)} \right)^{-1}$ and $L^{I-1} = N l^{I-1}$ we have the first-order condition from the text:

$$0 = F_{l^I} - w^I \left(\frac{l^I}{l^{I-1}} \right) \left(1 + \left(1 - \frac{\beta l^R}{l^I} \right) \frac{1}{N \epsilon^I} \right). \quad (24)$$

Substituting our functional forms and simplifying yields the following expression for l^I as a linear function of l^R , we have:

$$l^I = \frac{(1 + \beta) N l^{I-1}}{N + 1} - \left(\frac{\sigma(1 + \beta) N l^{I-1} - \beta}{N + 1} \right) l^R. \quad (25)$$

We also have the first-order condition for l^R , which, with $\epsilon^R = \frac{R(l^R)}{l^R R'(l^R)}$ as in the text, is given by:

$$F_{l^R} = w^R \left(1 + \left(\epsilon^R - \frac{\beta w^I}{l^R R'(l^R)} \right)^{-1} \right) + v_R. \quad (26)$$

Plugging in our functional forms from above and simplifying yields:

$$1 - \sigma l^I = w^R \left(1 + \left(\frac{(1 + \beta)l^{t-1}l^R - \beta l^I}{(1 + \beta)l^{t-1}l^R} \right)^{-1} \right) + v_R. \quad (27)$$

Solving for l^R as a linear function of l^I we get:

$$l^R = \frac{1 - v_R}{2} - \left(\frac{\sigma - \frac{\beta}{(1+\beta)l^{t-1}}}{2} \right) l^I. \quad (28)$$

Putting the second into the first and solving for l^I we get:

$$l^I = \frac{2 - (1 - v_R) \left(\sigma - \frac{\beta}{(1+\beta)Nl^{t-1}} \right)}{2 \frac{N+1}{(1+\beta)Nl^{t-1}} - \left(\sigma - \frac{\beta}{(1+\beta)Nl^{t-1}} \right) \left(\sigma - \frac{\beta}{(1+\beta)l^{t-1}} \right)}. \quad (29)$$

Using the above equation for l^I and equation 28 to solve for l^R we get:

$$l^R = \frac{1 - v_R}{2} - \frac{\left(\sigma - \frac{\beta}{(1+\beta)l^{t-1}} \right)}{2} \left(\frac{2 - (1 - v_R) \left(\sigma - \frac{\beta}{(1+\beta)Nl^{t-1}} \right)}{2 \frac{N+1}{(1+\beta)Nl^{t-1}} - \left(\sigma - \frac{\beta}{(1+\beta)Nl^{t-1}} \right) \left(\sigma - \frac{\beta}{(1+\beta)l^{t-1}} \right)} \right). \quad (30)$$

$$(31)$$

These are cumbersome expressions, but as $\beta \rightarrow 0$ we converge to the following more manageable closed-form solutions:

$$l^I = \frac{1 - \frac{1}{2}\sigma(1 - v_R)}{\frac{N+1}{Nl^{t-1}} - \frac{1}{2}\sigma^2} \quad (32)$$

$$l^R = \frac{1}{2} \left(1 - v_R - \sigma \left(\frac{1 - \frac{1}{2}\sigma(1 - v_R)}{\frac{N+1}{Nl^{t-1}} - \frac{1}{2}\sigma^2} \right) \right) \quad (33)$$

Since $\frac{N+1}{N}$ is decreasing in N , inspection of the solutions at $\beta = 0$ clearly reveals that l^R is decreasing in N while l^I is increasing in N . The wage of new recruits falls and the wage of incumbent workers rises by the definition of $H(W)$ and $s(W)$. Since the first-order conditions, and their derivatives with respect to N , are clearly continuous in β at $\beta = 0$, there is a $\bar{\beta}$ such that these comparative statics hold for $\beta \in [0, \bar{\beta})$, i.e. β positive and sufficiently small.

B Data Appendix

B.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 contract match. When workers change employer or sign new contracts with their existing 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.

B.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.

B.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.

B.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.

B.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.

C Additional Empirical Results

C.1 Heterogeneity by Worker Characteristics

We examine heterogeneity by the worker characteristics in our data. Table A.7 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 A.7 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 A.7 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 C in Table A.7. 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 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.

C.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

$$RIF_\tau(y) = q_\tau + \frac{\tau - \mathbf{1}(y \leq q_\tau)}{\widehat{f}(y)}. \quad (34)$$

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 $\tau = 10, 20, \dots, 90$:

$$RIF_\tau(y_{it}) = \sum_{k=-3}^3 \gamma_{k\tau}^{Post2011} D_{it+k}^{Post} + \sum_{k=-3}^3 \gamma_{k\tau}^{Pre2011} D_{it+k}^{Pre} + \beta_t X_{it} + \delta_i + \delta_t + \epsilon_{it}. \quad (35)$$

Figure A.7 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 the 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).

C.3 Reduced-form Parameter Estimates ($\beta = 0$)

If we assume $\beta = 0$, we can then recover ϵ^R by using the firm-level estimates of the change in contract expirations interacted with the reform as an exogenous shock to the marginal product of potential migrants. Using contract expirations as exogenous variation in the quantity of new entrants, we can estimate a reduced form ϵ^R which is the labor supply elasticity for new recruits:

$$\Delta \log(w^R) = \frac{1}{\epsilon^R} \Delta \log l^R. \quad (36)$$

Then we can use the empirical equation $\Delta \log l^R = \widehat{\gamma} \Delta \log l^I$ as a first-stage equation, because the only way l^I affects w^R is via l^R . From our firm-level estimates we have that $\frac{\Delta \log w^R}{\Delta \log l^I} = -0.034$ and $\frac{\Delta \log l^R}{\Delta \log l^I} = -0.035$.⁵⁵ The IV estimate can simply be expressed as the ratio $\epsilon^R = \frac{\Delta \log l^R}{\Delta \log w^R} =$

⁵⁵The estimate of -0.034 is from the first column of Panel B in Table 6, and the estimate of -0.035 is from the first column of Panel A. Note 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.

$\frac{\frac{\Delta \log l^r}{\Delta \log l^I}}{\frac{\Delta \log w^r}{\Delta \log w^I}} = \frac{0.035}{0.034} = 1.03$. This is not too far away from the pre-reform estimate of ϵ^I recovered under the model, which is reassuring because the distribution of outside options for new recruits and incumbent workers prior to the reform should be quite similar. This implies that wages for new recruits are roughly one-half of marginal product.

Continuing with the assumption that $\beta = 0$, we can also move away from the particular imperfect competition model presented earlier, and work with a more general specification that merely assumes that the reform increases the labor supply elasticity facing the firm for incumbent workers. If we assume the two types of labor are perfect substitutes (with identical marginal products) in the production function, this alternative model will have all the same comparative statics as our previous model.⁵⁶ Then we have $F'(l_i^R + l_i^I) = w^I(1 + \frac{1}{\epsilon_{I,t}}) = w^R(1 + \frac{1}{\epsilon^R}) + v_r$. The only effect of differential contract expirations before and after the reform on new recruits occurs through lowered costs of retention of incumbent workers, via $\epsilon^{I,post} > \epsilon^{I,pre}$, and can be assumed to be orthogonal to all other determinants of the new recruits' wage.

These assumptions yield the following expression for the change in $\log(1 + \frac{1}{\epsilon^I})$, defined as:

$$\Delta \log \left(1 + \frac{1}{\epsilon^I}\right) \equiv \log \left(1 + \frac{1}{\epsilon^{I,post}}\right) - \log \left(1 + \frac{1}{\epsilon^{I,pre}}\right) = \Delta \log w^R - \Delta \log w^I = -0.14. \quad (37)$$

We make use of an estimate of that recruitment costs are approximately 20% of a worker's annual earnings (Gutcher 2013), and hold $\frac{w^R}{v_r}$ constant before and after the reform.⁵⁷ This implies that $\epsilon^{I,post}$ is given by $\frac{1}{e^{-0.14}1 + \frac{e^{-0.14}}{\epsilon^{I,pre}} - 1}$. If we further assume that $\epsilon^{I,pre} = \epsilon^R = 1.1$, as the distribution of outside options prior to reform was similar to that of the new recruits, then we have that the labor supply elasticity facing the firm went from 1.1 to 1.52, almost a 50% increase. If we instead use the first-order condition together with sample statistics, we can write $\epsilon^{I,pre} = \left(\frac{w^{R,pre}}{w^{I,pre}} \left(1 + \frac{1}{\epsilon^R} + \frac{v_r}{w^R}\right) - 1\right)^{-1}$, and so we get $\epsilon^{I,pre} = \frac{1}{0.99(1 + \frac{1}{1.1} + \frac{0.2}{3}) - 1} = 1.05$, resulting in a post-reform $\epsilon^{I,post}$ of 1.43. As can be seen, both approaches to estimating $\epsilon^{I,pre}$ yield very similar answers, and both are similar to the model-specific estimated ϵ^I calculated above. Our static, reduced-form estimates suggest workers were getting roughly 50% of their marginal product before the reform, increasing to up to 60% following the reform.

These estimates can be related to the steady-state labor-supply elasticities discussed in the dynamic monopsony literature (Manning 2003). In steady-state, the number of new recruits must equal the number of quits, and so we have $(1 - s(w^I))l_i^{t-1} = l_i^R$. And so the steady-state labor-supply elasticity is the recruitment elasticity minus the quit elasticity. Given the mechanically

⁵⁶See Naidu, Nyarko, and Wang (2014) for this alternative model.

⁵⁷Note 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.

high level of the quit elasticity, this will be large, approximately 21. If we instead use the negative of our retention elasticity to approximate the quit elasticity, we get a steady-state labor supply elasticity of 2.1-2.2, increasing to between 2.48 and 2.6 after the reform, both of which are much closer to the estimates in literature. However we believe the steady-state assumption is a poor one given our use of short-run variation, potential productivity differences between new recruits and incumbent workers, and the difficulty of obtaining a clear “quit elasticity” in a context where quits are largely prohibited.

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.
- 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. (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.
- DAL BÓ, E., F. FINAN, AND M. A. ROSSI (2013): “Strengthening State Capabilities: The Role of Financial Incentives in the Call to Public Service,” *The Quarterly Journal of Economics*, 128, 1169–1218.
- 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.
- 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.
- JAMIESON, D. (2014): “Jimmy John’s Makes Low-Wage Workers Sign ‘Oppressive’ Noncompete Agreements,” http://www.huffingtonpost.com/2014/10/13/jimmy-johns-non-compete_n_5978180.html, accessed December 2014.
- KREPS, D. M. AND J. A. SCHEINKMAN (1983): “Quantity Precommitment and Bertrand Competition yield Cournot Outcomes,” *The Bell Journal of Economics*, 326–337.
- LOBEL, O. (2013): *Talent Wants to be Free: Why We Should Learn to Love Leaks, Raids, and Free Riding*, Yale University Press.
- LONGVA, A. N. (1999): “Keeping Migrant Workers in Check: The Kafala System in the Gulf,” *Middle East Report*, 29, 20–22.
- MANKIW, N. G. (2010): “Spreading the wealth around: reflections inspired by Joe the Plumber,” *Eastern Economic Journal*, 36, 285–298.
- 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 (2014): “Distortions in the international migrant labor market: Evidence from Filipino migration and wage responses to destination country economic shocks,” *American Economic Journal: Applied Economics*, 6, 49–75.
- NAIDU, S. (2010): “Recruitment Restrictions and Labor Markets: Evidence from the Postbellum US South,” *Journal of Labor Economics*, 28, 413–445.
- NAIDU, S., Y. NYARKO, AND S.-Y. WANG (2014): “Worker Mobility in a Global Labor Market: Evidence from the United Arab Emirates,” *NBER Working Paper 20388*.

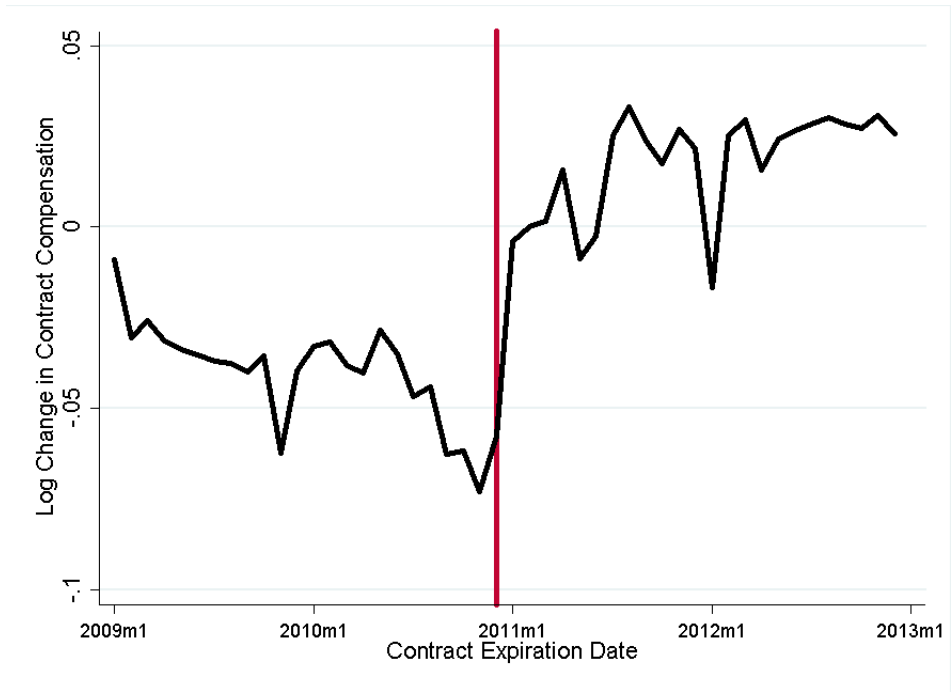
- NAIDU, S. AND N. YUCHTMAN (2013): “Coercive Contract Enforcement: Law and the Labor Market in Nineteenth Century Industrial Britain,” *American Economic Review*, 103, 107–44.
- NATIONS, U. (2012): “World Population Prospects: The 2012 Revision,” http://esa.un.org/unpd/wpp/unpp/panel_indicators.htm, 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.
- ROSENBLATT, J. (2014): “Evidence of Alleged Apple-Google No-Poaching Deal Triggers More Lawsuits,” <http://www.bloomberg.com/news/2014-11-19/apple-google-no-poaching-evidence-triggers-more-lawsuits.html>, accessed December 2014.
- 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.
- STARR, E., N. BISHARA, AND J. PRESCOTT (2015): “Noncompetes in the U.S. Labor Force,” *Working Paper*.
- VEDDER, R. K. (1975): “The Slave Exploitation (Expropriation) Rate,” *Explorations in Economic History*, 12, 453–457.
- 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.

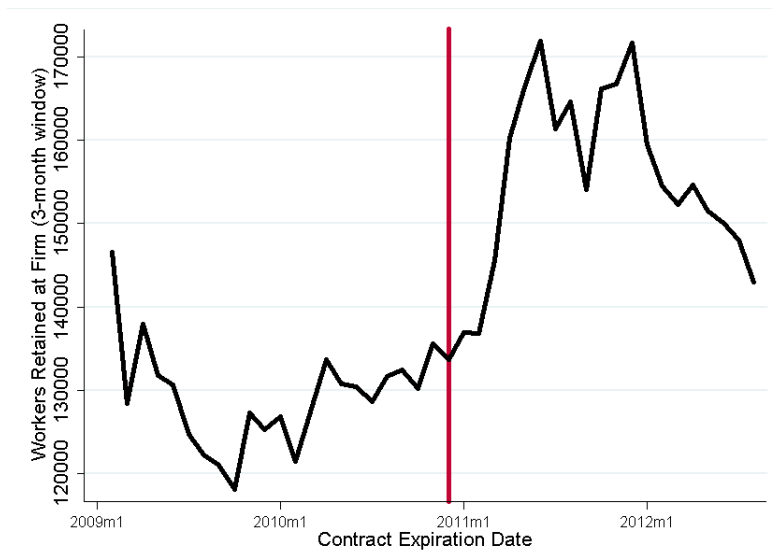
WEYL, E. G. (2014): “The Openness-Equality Trade-Off in Global Redistribution,” *Working Paper*.

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



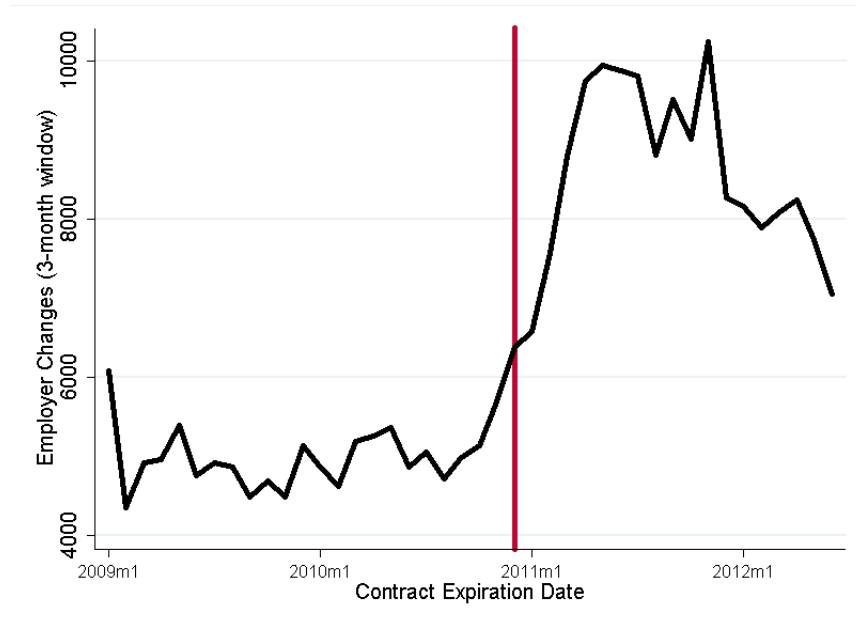
Notes: This shows the average log change in real compensation from one contract in time t to the next contract in $t+1$ where t is the month of expiration of the preceding contract. Compensation includes the value of earnings and benefits defined in the contract. The vertical line indicates the announcement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

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



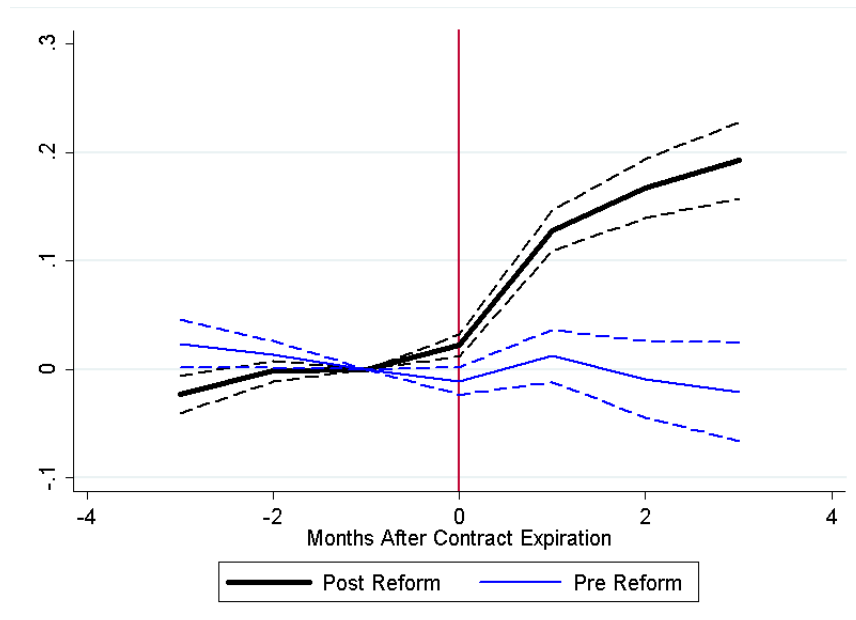
Notes: This shows the total number of workers that sign a new contract with their existing firm by contract expiration date. This is smoothed over the two adjacent months. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data.

Figure 3: Total Changes in Employer by Contract Expiration Date



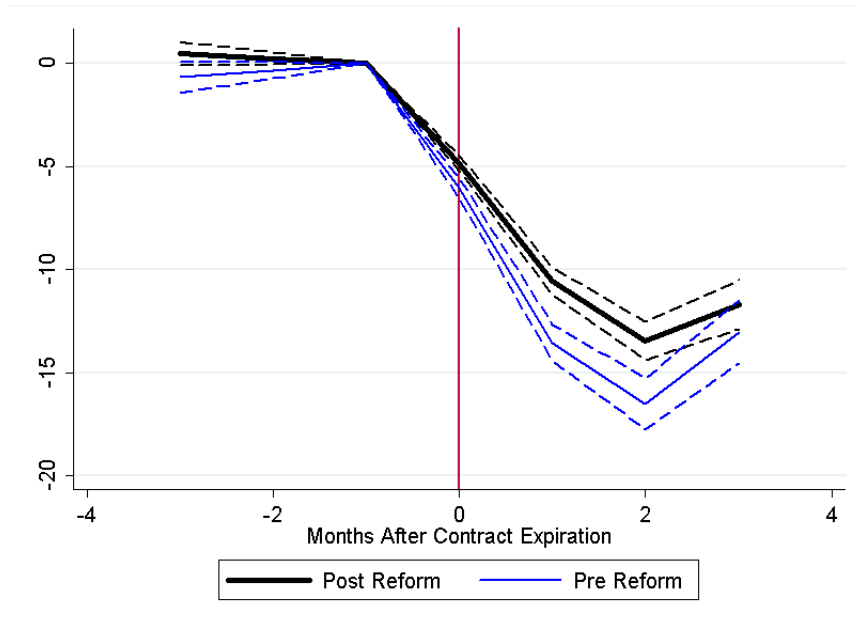
Notes: This shows the total number of employer changes that occur by contract expiration date. The total includes employer transitions that occur within three months of the contract expiration. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data.

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



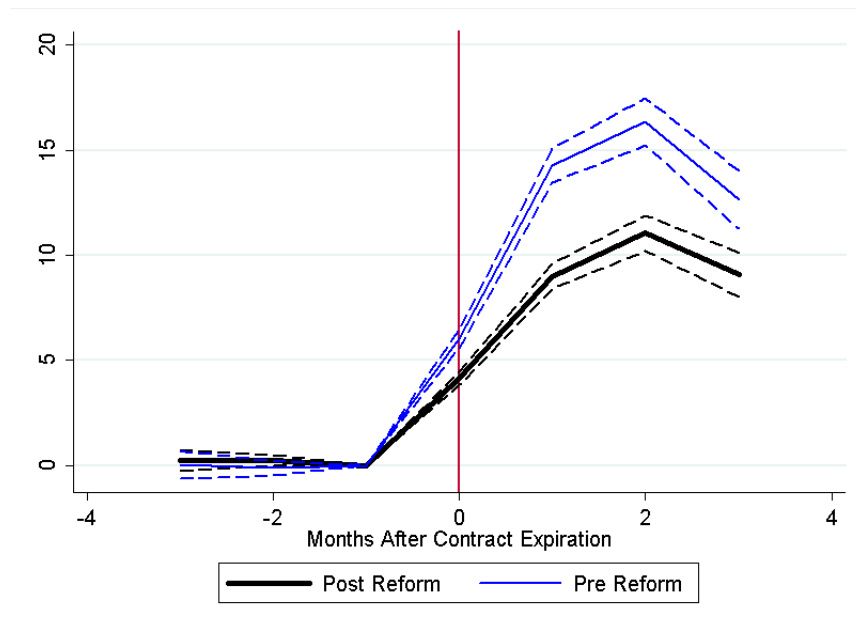
Notes: This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is log earnings. The regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker's contract expires.

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



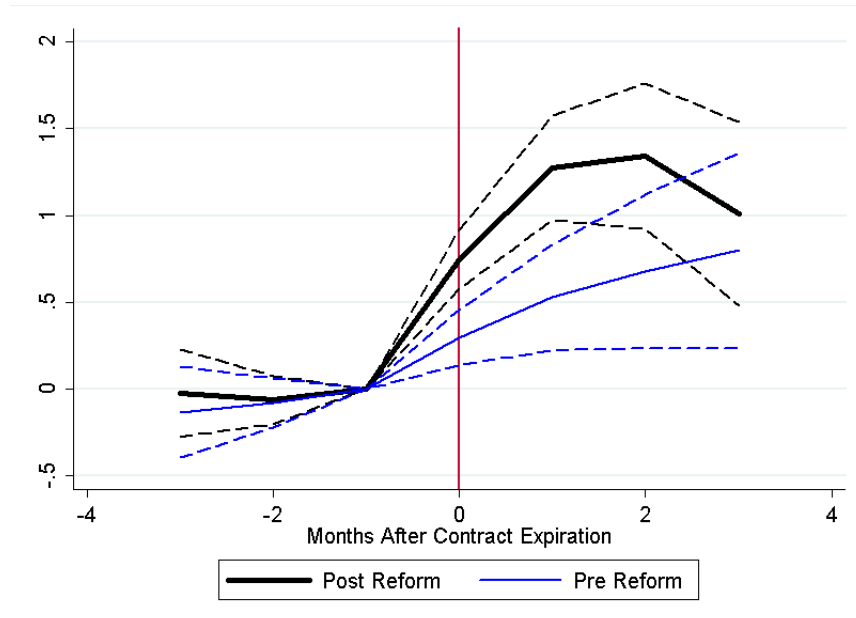
Notes: This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is firm retention. The regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker’s contract expires.

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



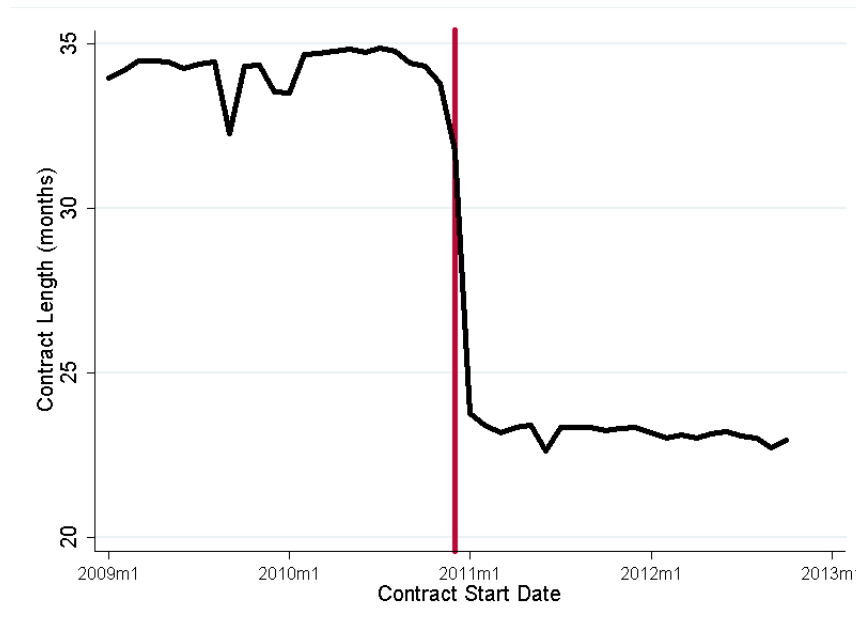
Notes: This displays the coefficient estimates of each period around a contract expiration separately for expirations that occur pre-reform and post-reform. The dependent variable is country exits. The regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker’s contract expires.

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



Notes: 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 regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the month immediately prior to the month of expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker's contract expires.

Figure 8: Average Contract Length by Contract Start Date



Notes: This shows the average contract length (in months) by the start date of the contract. The vertical line indicates the announcement of the NOC reform. The sample is the 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
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

Notes: The table shows the mean, standard deviation and number of observations for each variable. Pre-reform pools all months of data prior to the implementation of the NOC reform in January 2011. Post-reform pools all months of data after and including January 2011. Earnings are in real 2007 dirham.

Table 2: Effect on Log Earnings and Firm Retention

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Log Earnings					
Post Reform X Post Contract Expire	0.113*** (0.009)	0.110*** (0.009)	0.109*** (0.011)	0.091*** (0.010)	0.139*** (0.012)
Post Reform X Period Contract Expire	0.015*** (0.005)	0.013** (0.005)	0.016** (0.006)	0.049*** (0.005)	0.017*** (0.006)
N	529502	529502	342555	463312	447394
Number of Clusters	111006	111006	69239	88290	105606
R-squared	0.017	0.017	0.023	0.010	0.019
Panel B: Staying with Firm					
Post Reform X Post Contract Expire	3.832*** (0.304)	4.333*** (0.321)	6.387*** (0.405)	3.106*** (0.282)	4.270*** (0.393)
Post Reform X Period Contract Expire	1.642*** (0.205)	1.881*** (0.224)	2.743*** (0.242)	0.223** (0.114)	1.759*** (0.266)
N	525737	525737	343503	466806	445200
Number of Clusters	110120	110120	68931	88293	105448
R-squared	0.082	0.082	0.370	0.093	0.081
Polynomials in Time to Reform	No	Yes	Yes	No	No
Worker Characteristics	No	No	Yes	No	No

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table 3: Effect on Hours Variables

	Hours Upper Estimate		Hours Lower Estimate	
	(1)	(2)	(3)	(4)
Post Reform X Post Contract Expire	-1.487 (1.194)	-0.094 (1.680)	-1.239 (0.995)	-0.078 (1.400)
Post Reform X Period Contract Expire	-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

Notes: In calculating hours, we assume that overtime hours are paid at a rate of 1.25 in columns 1 and 2, and 1.5 in columns 3 and 4. 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 sample includes all months from January 2009 to October 2012. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table 4: Effect on Exits from UAE and Employer Transitions

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Exits from UAE					
Post Reform X Post Contract Expire	-4.408*** (0.271)	-4.749*** (0.287)	-6.608*** (0.370)	-3.570*** (0.255)	-4.756*** (0.355)
Post Reform X Period Contract Expire	-1.822*** (0.184)	-1.983*** (0.200)	-3.094*** (0.227)	-0.201** (0.089)	-2.065*** (0.239)
N	550933	550933	356203	477737	465333
Number of Clusters	111319	111319	69442	88290	106789
R-squared	0.084	0.084	0.347	0.092	0.083
Panel B: Employer Changes					
Post Reform X Post Contract Expire	0.663*** (0.102)	0.639*** (0.109)	0.255** (0.117)	0.327*** (0.097)	0.633*** (0.129)
Post Reform X Period Contract Expire	0.491*** (0.070)	0.478*** (0.078)	0.311*** (0.076)	-0.311*** (0.047)	0.546*** (0.088)
N	514606	514606	335281	459035	434276
Number of Clusters	109388	109388	68495	88293	104121
R-squared	0.006	0.006	0.153	0.008	0.006
Polynomials in Time to Reform	No	Yes	Yes	No	No
Worker Characteristics	No	No	Yes	No	No

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table 5: Effect on Imputed Outcome Variables

	Log Earnings Imp.		Employer Change Imp.	
	High (1)	Low (2)	High (3)	Low (4)
Post Reform X Post Contract Expire	0.185*** (0.009)	0.031*** (0.009)	0.644*** (0.098)	0.558*** (0.098)
Post Reform X Period Contract Expire	0.046*** (0.005)	-0.019*** (0.005)	0.480*** (0.067)	0.445*** (0.067)
N	550920	550920	536024	536024
Number of Clusters	111319	111319	110337	110337
R-squared	0.016	0.012	0.005	0.005

Notes: The dependent variable in the first four columns is log earnings, and the last four columns is the probability of changing employers. For the high imputations, we impute the 90th percentile value for pre-reform pre-expiration and for post-reform post-expiration and we impute the 10th percentile value for pre-reform post-expiration and post-reform pre-expiration. For the low imputations, we impute the 10th percentile value for pre-reform pre-expiration and for post-reform post-expiration and we impute the 90th percentile value for pre-reform post-expiration and post-reform pre-expiration. 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table 6: Impact of the Reform on New Entrants' Outcomes

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Log Entrants					
Post Reform \times Log Contracts Expiring	-0.035*	-0.030	0.019	-0.065***	-0.074***
	(0.021)	(0.020)	(0.013)	(0.022)	(0.028)
Post Reform \times Leads Contracts Expiring			-0.021**		
			(0.008)		
Post Reform \times Lags Contracts Expiring			-0.013*		
			(0.007)		
N	224646	224646	111478	121295	89448
Number of Clusters	17891	17891	10055	5692	5473
R-squared	0.131	0.143	0.044	0.156	0.148
Mean of Dep. Var.	0.137	0.137	0.102	0.157	0.160
Std. Dev. of Dep. Var.	0.462	0.462	0.393	0.511	0.523
Panel B: Log Entrant Earnings					
Post Reform \times Log Contracts Expiring	-0.034*	-0.034*	-0.014	-0.022	0.002
	(0.019)	(0.019)	(0.023)	(0.021)	(0.025)
Post Reform \times Leads Contracts Expiring			-0.022**		
			(0.010)		
Post Reform \times Lags Contracts Expiring			0.013		
			(0.009)		
N	27437	27437	9749	15189	10945
Number of Clusters	12868	12868	3592	4936	3943
R-squared	0.010	0.024	0.027	0.009	0.008
Mean of Dep. Var.	7.065	7.065	7.008	7.052	7.042
Std. Dev. of Dep. Var.	0.753	0.753	0.753	0.715	0.713
Leads and Lags	No	No	Yes	No	No
City X Year-Month Fixed Effects	No	Yes	No	No	No

Notes: The dependent variable is indicated by the panel label. 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. Each observation in the regression is a firm and year-month. 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Log Contracts Expiring is the log of the number of contracts expiring at the firm in that month. Leads Contracts Expiring is the total effect of the log of the number of contracts expiring in the three months prior to that month. Lags Contracts Expiring is the total effect of the log of the number of contracts expiring in the three months after that month.

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

	Stayers		Full Sample	
	(1)	(2)	(3)	(4)
Post Reform X Post Contract Expire	0.114*** (0.008)	0.109*** (0.010)	0.120*** (0.008)	0.107*** (0.010)
Post Reform X Period Contract Expire	0.013*** (0.005)	0.015*** (0.006)	0.018*** (0.005)	0.015*** (0.006)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	0.155*** (0.006)	0.133*** (0.008)	0.161*** (0.006)	0.133*** (0.008)
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	0.137*** (0.005)	0.124*** (0.007)	0.142*** (0.005)	0.124*** (0.006)
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	0.108*** (0.004)	0.100*** (0.005)	0.111*** (0.004)	0.100*** (0.005)
Contract Expire X Post Reform (γ_0^{Post})	0.010*** (0.003)	0.010*** (0.004)	0.013*** (0.003)	0.010*** (0.004)
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	0.014* (0.008)	0.003 (0.010)	0.011 (0.008)	0.005 (0.010)
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	0.017*** (0.007)	0.006 (0.008)	0.015** (0.007)	0.008 (0.008)
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	0.030*** (0.005)	0.021*** (0.006)	0.028*** (0.005)	0.022*** (0.006)
Contract Expire X Pre Reform (γ_0^{Pre})	-0.003 (0.004)	-0.005 (0.004)	-0.004 (0.004)	-0.005 (0.004)
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

Notes: 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. The full sample includes both individuals who do and do not change firms. ***, **, * denotes significance at the 1, 5, and 10% levels, respectively. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires. The coefficients corresponding with each contract period interacted with post- or pre-reform is shown in the second panel.

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

	Full Sample			Both Sides		Trimmed
	(1)	(2)	(3)	(4)	(5)	
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	0.157*** (0.007)	0.160*** (0.008)	0.134*** (0.010)	0.110*** (0.007)	0.185*** (0.009)	
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	0.138*** (0.006)	0.141*** (0.006)	0.125*** (0.008)	0.099*** (0.006)	0.162*** (0.007)	
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	0.109*** (0.004)	0.111*** (0.005)	0.101*** (0.006)	0.080*** (0.005)	0.127*** (0.006)	
Contract Expire X Post Reform (γ_0^{Post})	0.012*** (0.003)	0.013*** (0.004)	0.011** (0.005)	0.068*** (0.003)	0.016*** (0.004)	
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	0.016* (0.009)	0.023** (0.009)	0.004 (0.010)	0.001 (0.010)	0.013 (0.011)	
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	0.019** (0.008)	0.024*** (0.007)	0.007 (0.008)	0.001 (0.008)	0.014 (0.009)	
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	0.031*** (0.006)	0.035*** (0.006)	0.022*** (0.006)	0.014** (0.006)	0.030*** (0.007)	
Contract Expire X Pre Reform (γ_0^{Pre})	-0.003 (0.004)	-0.000 (0.004)	-0.005 (0.005)	0.019*** (0.004)	-0.001 (0.005)	
Polynomials in Time to Reform	No	Yes	Yes	No	No	
Worker Characteristics	No	No	Yes	No	No	
N	529502	529502	342555	463312	447394	
Number of Clusters	111006	111006	69239	88290	105606	
R-squared	0.017	0.017	0.023	0.010	0.019	

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010.

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

	Full Sample			Both Sides		Trimmed
	(1)	(2)	(3)	(4)	(5)	
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	-10.935*** (0.214)	-11.065*** (0.227)	-6.100*** (0.289)	-7.969*** (0.204)	-10.452*** (0.275)	
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	-12.855*** (0.195)	-12.961*** (0.209)	-7.493*** (0.313)	-10.560*** (0.190)	-12.196*** (0.246)	
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	-10.190*** (0.163)	-10.268*** (0.177)	-5.818*** (0.254)	-8.016*** (0.159)	-9.490*** (0.206)	
Contract Expire X Post Reform (γ_0^{Post})	-4.663*** (0.117)	-4.712*** (0.128)	-2.652*** (0.148)	0.357*** (0.068)	-4.300*** (0.150)	
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	-14.080*** (0.323)	-14.833*** (0.326)	-12.331*** (0.381)	-9.855*** (0.302)	-14.107*** (0.420)	
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	-17.324*** (0.302)	-17.938*** (0.315)	-14.385*** (0.339)	-14.247*** (0.294)	-16.646*** (0.384)	
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	-14.070*** (0.254)	-14.522*** (0.269)	-11.857*** (0.284)	-11.761*** (0.248)	-14.195*** (0.331)	
Contract Expire X Pre Reform (γ_0^{Pre})	-6.304*** (0.172)	-6.593*** (0.183)	-5.395*** (0.192)	0.134 (0.095)	-6.059*** (0.223)	
Polynomials in Time to Reform	No	Yes	Yes	No	No	
Worker Characteristics	No	No	Yes	No	No	
N	525737	525737	343503	466806	445200	
Number of Clusters	110120	110120	68931	88293	105448	
R-squared	0.082	0.082	0.370	0.093	0.081	

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010.

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

	Full Sample			Both Sides		Trimmed
	(1)	(2)	(3)	(4)	(5)	
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	9.464*** (0.190)	9.616*** (0.198)	5.613*** (0.274)	7.408*** (0.180)	9.080*** (0.242)	
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	11.344*** (0.174)	11.467*** (0.184)	7.103*** (0.294)	9.748*** (0.169)	10.777*** (0.218)	
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	9.189*** (0.144)	9.280*** (0.155)	5.699*** (0.231)	7.324*** (0.142)	8.609*** (0.180)	
Contract Expire X Post Reform (γ_0^{Post})	4.220*** (0.101)	4.278*** (0.110)	2.487*** (0.141)	-0.085* (0.051)	3.937*** (0.129)	
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	12.610*** (0.292)	13.185*** (0.300)	11.772*** (0.348)	9.282*** (0.279)	12.738*** (0.387)	
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	16.327*** (0.281)	16.795*** (0.297)	14.212*** (0.314)	13.967*** (0.280)	15.699*** (0.362)	
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	14.285*** (0.236)	14.631*** (0.252)	12.255*** (0.265)	11.941*** (0.236)	14.299*** (0.311)	
Contract Expire X Pre Reform (γ_0^{Pre})	6.042*** (0.156)	6.261*** (0.167)	5.580*** (0.178)	0.116 (0.078)	6.002*** (0.204)	
Polynomials in Time to Reform	No	Yes	Yes	No	No	
Worker Characteristics	No	No	Yes	No	No	
N	550933	550933	356203	477737	465333	
Number of Clusters	111319	111319	69442	88290	106789	
R-squared	0.084	0.084	0.347	0.092	0.083	

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010.

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

	Full Sample			Both Sides		Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 3 Contract Expire X Post Reform (γ_3^{Post})	0.973*** (0.104)	0.885*** (0.114)	0.541*** (0.116)	0.476*** (0.101)	0.806*** (0.129)		
Lag 2 Contract Expire X Post Reform (γ_2^{Post})	1.315*** (0.093)	1.243*** (0.103)	0.715*** (0.105)	0.930*** (0.092)	1.164*** (0.116)		
Lag 1 Contract Expire X Post Reform (γ_1^{Post})	1.263*** (0.080)	1.210*** (0.089)	0.664*** (0.089)	0.903*** (0.079)	1.097*** (0.099)		
Contract Expire X Post Reform (γ_0^{Post})	0.750*** (0.060)	0.715*** (0.067)	0.466*** (0.065)	-0.292*** (0.041)	0.707*** (0.076)		
Lag 3 Contract Expire X Pre Reform (γ_3^{Pre})	0.602*** (0.105)	0.544*** (0.073)	0.493*** (0.088)	0.504*** (0.108)	0.478*** (0.097)		
Lag 2 Contract Expire X Pre Reform (γ_2^{Pre})	0.528*** (0.087)	0.480*** (0.063)	0.357*** (0.067)	0.451*** (0.090)	0.371*** (0.077)		
Lag 1 Contract Expire X Pre Reform (γ_1^{Pre})	0.432*** (0.069)	0.396*** (0.054)	0.307*** (0.058)	0.372*** (0.070)	0.319*** (0.065)		
Contract Expire X Pre Reform (γ_0^{Pre})	0.258*** (0.047)	0.236*** (0.040)	0.155*** (0.039)	0.020 (0.039)	0.161*** (0.045)		
Polynomials in Time to Reform	No	Yes	Yes	No	No	No	No
Worker Characteristics	No	No	Yes	No	No	No	No
N	514606	514606	335281	459035	434276		
Number of Clusters	109388	109388	68495	88293	104121		
R-squared	0.006	0.006	0.153	0.008	0.006		

Notes: 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010.

Table A.6: Effect on Imputed Outcome Variables (Fine Bounds)

	Log Earnings Imp.		Employer Change Imp.	
	High (1)	Low (2)	High (3)	Low (4)
Post Reform X Post Contract Expire	0.123*** (0.009)	0.095*** (0.009)	0.638*** (0.098)	0.618*** (0.098)
Post Reform X Period Contract Expire	0.020*** (0.005)	0.009* (0.005)	0.479*** (0.067)	0.469*** (0.067)
N	550920	550920	536024	536024
Number of Clusters	111319	111319	110337	110337
R-squared	0.007	0.022	0.005	0.005

Notes: The dependent variable in the first four columns is log earnings, and the last four columns is the probability of changing employers. For the high imputations, we assume that the dependent variable has the 90th percentile value. For the low imputations, we assume that the dependent variable has the 10th percentile value. 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table A.7: 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 Reform X Post Contract Expire	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 Reform X Period Contract Expire	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 Reform X Post Contract Expire	-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 Reform X Period Contract Expire	-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 Reform X Post Contract Expire	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 Reform X Period Contract Expire	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

Notes: The dependent variable is given by the panel label. The columns specify the group to which the sample is limited. 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 sample includes all months from January 2009 to October 2012. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Post Contract Expire equals one for the periods after the individual's contract expires. Period Contract Expire equals one in the month that the individual's contract expires.

Table A.8: Summary Statistics of New Entrants to the UAE

	Pre-Reform			Post-Reform		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Age	32.64	9.100	1761686	32.42	8.772	2004360
Male	0.919	0.273	1760946	0.924	0.265	2001018
High Education	0.505	0.500	1700661	0.500	0.500	1982450
Indian	0.387	0.487	1761686	0.361	0.480	2004360

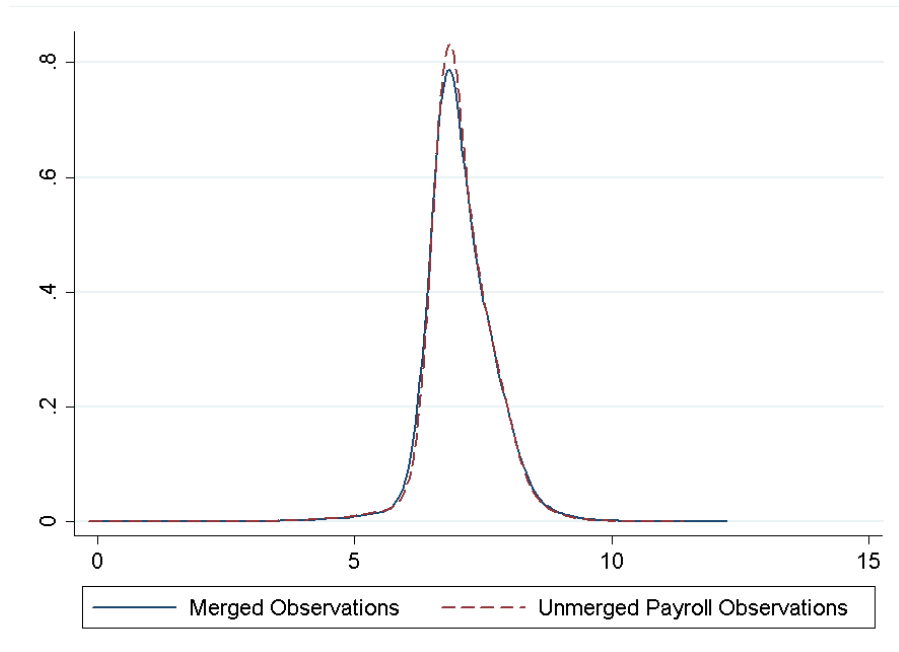
Notes: The table shows the mean, standard deviation and number of observations for each variable. Pre-reform shows the characteristics of new workers who are entering the UAE in 2010. Post-reform shows the characteristics of new workers who are entering the UAE after the implementation of the reform in 2011. The sample is the MOL data.

Table A.9: Other Firm Hiring Responses to the Reform

	Full Sample			Both Sides	Trimmed
	(1)	(2)	(3)	(4)	(5)
Panel A: Log Earnings					
Post 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 Reform \times Leads Contracts Expiring			0.005** (0.002)		
Post Reform \times Lags Contracts Expiring			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 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 Reform \times Leads Contracts Expiring			-0.030*** (0.006)		
Post Reform \times Lags Contracts Expiring			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 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 Reform \times Leads Contracts Expiring			0.002** (0.001)		
Post Reform \times Lags Contracts Expiring			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

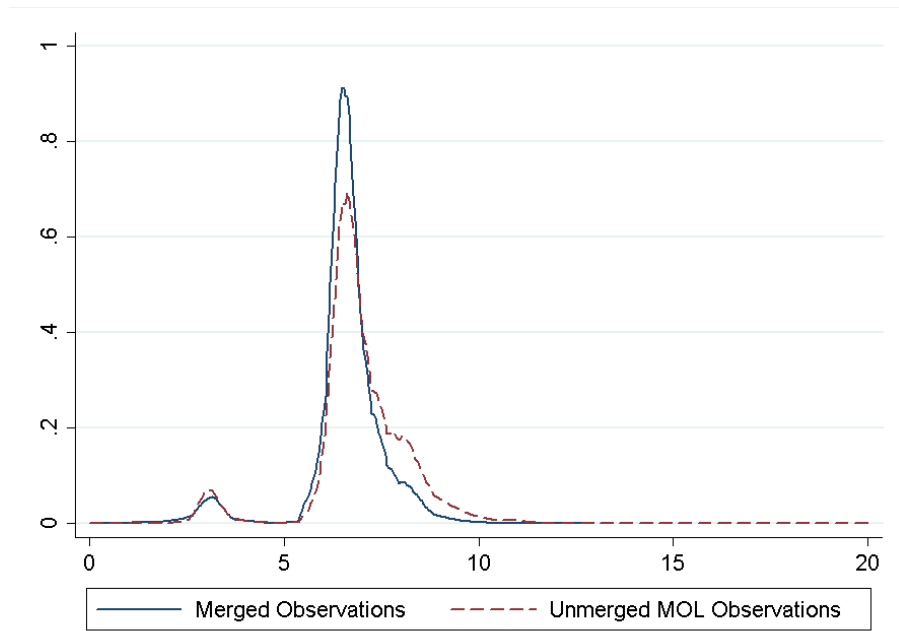
Notes: The dependent variable is given by the panel labels. All specifications include firm and year-month fixed effects. Standard errors clustered by firm in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively. Each observation in the regression is a firm and year-month. 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. Post Reform is an indicator that equals one after the announcement of the NOC reform in December 2010. Log Contracts Expiring is the log of the number of contracts expiring at the firm in that month. Leads Contracts Expiring is the total effect of the log of the number of contracts expiring in the three months prior to that month. Lags Contracts Expiring is the total effect of the log of the number of contracts expiring in the three months after that month.

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



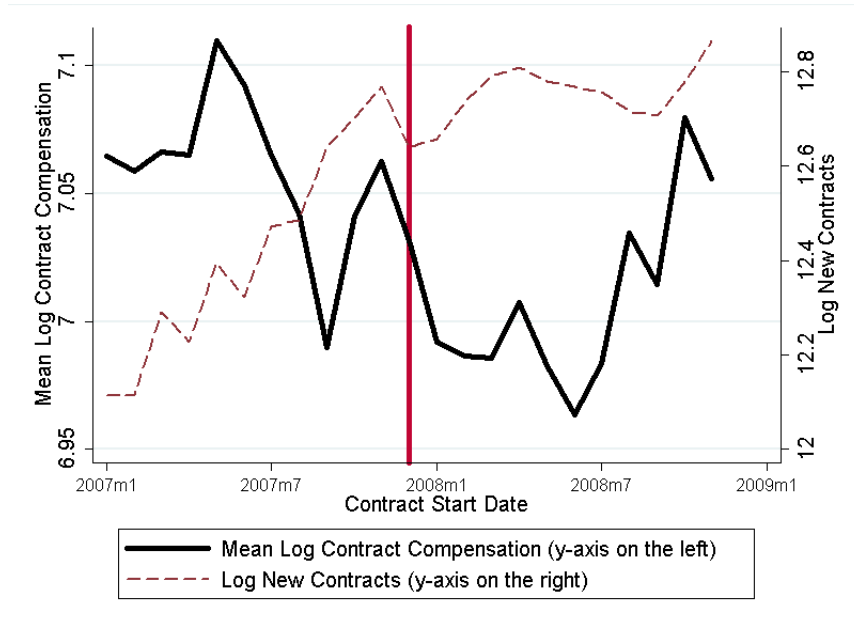
Notes: This shows the kernel density of the distribution of log earnings in the payroll data by whether the observation matches into the MOL data or not.

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



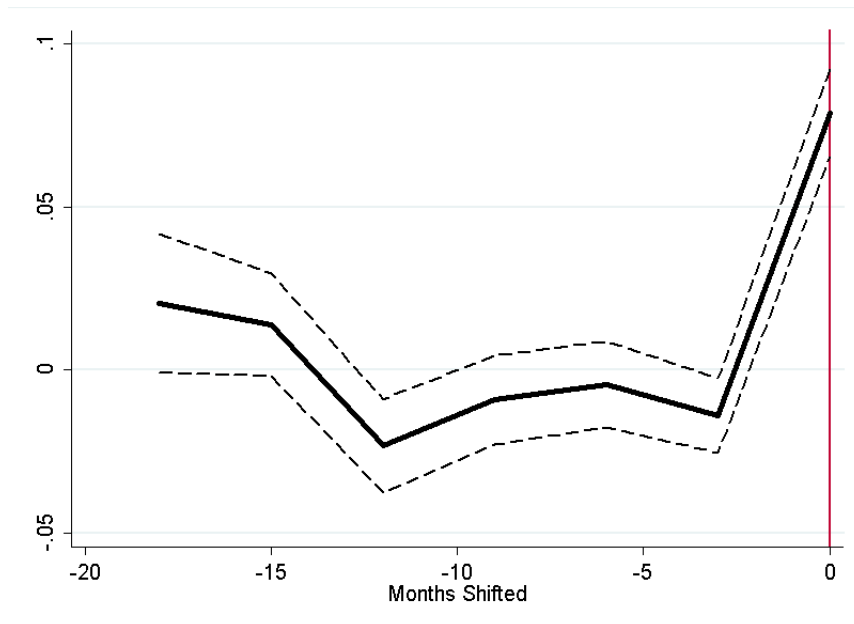
Notes: This shows the kernel density of the distribution of log contract earnings in the MOL data by whether the observation matches into the payroll data or not.

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



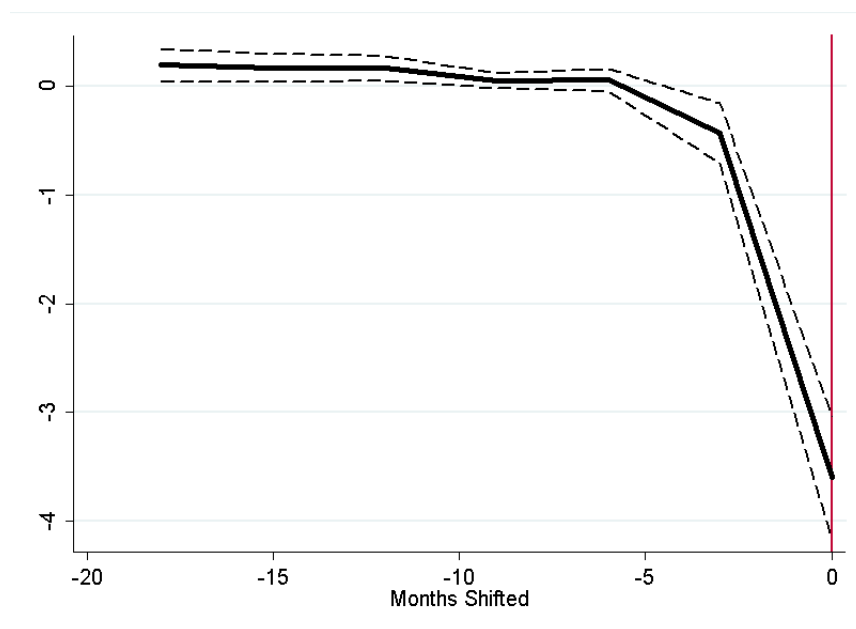
Notes: This figure displays the average log contract compensation by the start date of the contract in the bold solid line. The dashed line shows the logarithm of the total number of new contracts by the start date of the contract. The vertical line corresponds to exactly three years before the announcement of the reform. The sample is the MOL data.

Figure A.4: Effects of Time-Shifted Placebos on Log Earnings



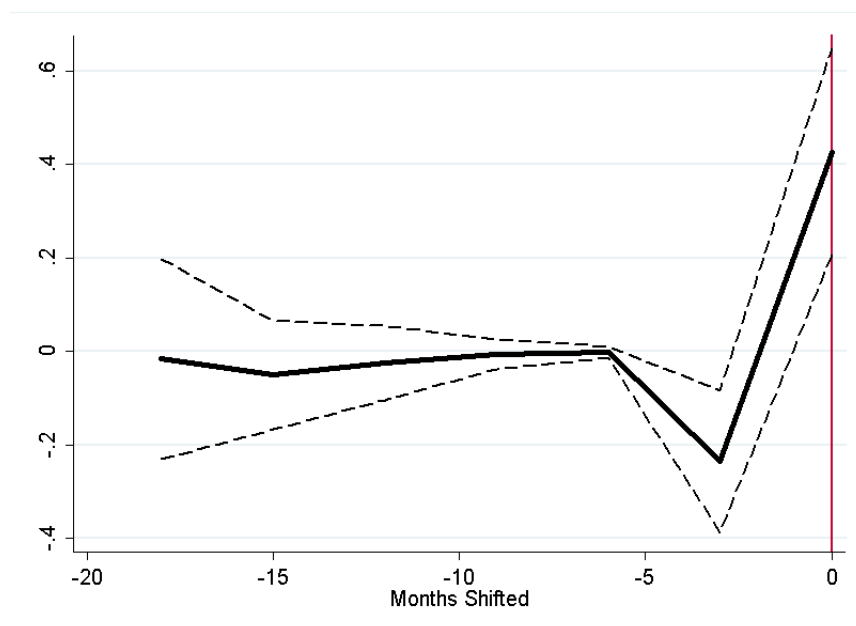
Notes: This displays the average 3-month post-contract expiration effect of the reform on log earnings where the timing of the contract expiration is shifted by the months given on the x-axis. The regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the three months prior to contract expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker’s contract expires.

Figure A.5: Effects of Time-Shifted Placebos on Exits from the UAE



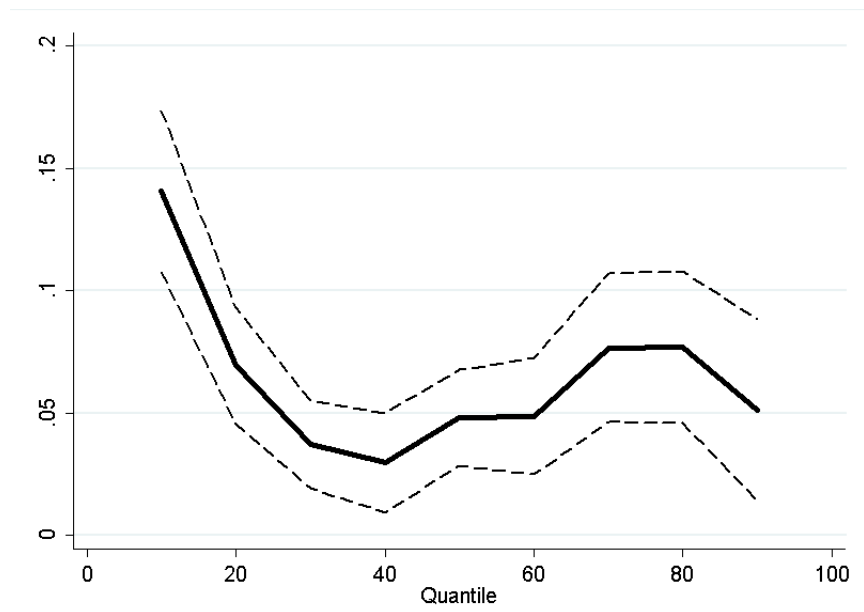
Notes: This displays the average 3-month post-contract expiration effect of the reform on country exits where the timing of the contract expiration is shifted by the months given on the x-axis. The regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the three months prior to contract expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker's contract expires.

Figure A.6: Effects of Time-Shifted Placebos on Employer Changes



Notes: 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 regressions include individual fixed effects, year-month fixed effects and a constant term. The omitted category is the three months prior to contract expiration. The dotted lines give the 95% confidence interval. The vertical line indicates the month that the worker's contract expires.

Figure A.7: Unconditional Quantile Estimates of Earnings



Notes: Each of the 10 points is a coefficient estimate from a quantile regression where the dependent variable is the conditional distribution of log earnings at each decile. The regressions include individual fixed effects, year-month fixed effects, polynomials in time to reform and worker characteristics, and a constant term. The omitted category is the three months prior to contract expiration. The dotted lines give the 95% confidence interval.