

# Monopsony Power in Migrant Labor Markets: Evidence from the United Arab Emirates

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Suresh Naidu

*Columbia University*

Yaw Nyarko

*New York University and New York University Abu Dhabi*

Shing-Yi Wang

*University of Pennsylvania*

By exploiting a reform in the United Arab Emirates that relaxed restrictions on employer transitions, we provide new estimates of the monopsony power of firms over migrant workers. Our results show that the reform increased incumbent migrants' earnings and firm retention. This occurs despite an increase in employer transitions and is driven by a fall in country exits. While the outcomes of incumbents improved, the reform decreased demand for new migrants and lowered their earnings. These results are consistent with a model of monopsony in which firms face upward-sloping labor supply curves for both new recruits in source countries and incumbent migrants.

## I. Introduction

Imperfect competition has been used by economists to explain a wide variety of labor market phenomena, including the employment effects

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of the minimum wage, the employer-size wage effect, race and gender wage gaps, agglomeration, and patterns in firm training (Manning 2011). Indeed, since 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 noncompetitive settings, have been lacking. This paper uses a policy change in the migrant labor market in the United Arab Emirates (UAE) to estimate the 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 for restraining labor market competition, lowering migrant wages, and facilitating labor rights violations (e.g., <http://www.epi.org/publication/2b-employers-congressional-allies-fighting>). 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 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 workers were each

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tioned to one employer for the duration of their multiyear 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 their previous employer, but only after their previous contracts 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 United States 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).<sup>1</sup> 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 anticompetitive 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 US antitrust 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 2010; Naidu and Yuchtman 2013). 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 stipulat-

<sup>1</sup> Starr et al. (2015) and recent media coverage note that noncompete clauses are expanding into low-skilled jobs in the United States as well (see, e.g., Jamieson 2014).

ing 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 US 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 the lack of formal information sharing or institutionalized wage setting.

Theoretically, modern general equilibrium models of imperfect competition generally rely on 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, Manning (2003), Cahuc, Postel-Vinay, and Robin (2006), and Hornstein, Krusell, and Violante (2011), while methodologically very diverse, all suggest that job mobility is important for explaining wage variation.<sup>2</sup> However, despite the strong predictions made by economic theory, 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 (see, e.g., McKenzie, Stillman, and Gibson 2010; Gibson, McKenzie, and Stillman 2011; Clemens 2013, 2016). However, much less attention has been paid to the labor market restrictions that migrants face in their destination countries.<sup>3</sup> A recent paper by Weyl (forthcoming) 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-à-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 compe-

<sup>2</sup> See Rogerson, Shimer, and Wright (2005) for a more complete review.

<sup>3</sup> We 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.

tion 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 apply to workers only after their contract expires after the reform. Standard contracts were uniformly 3 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 minimize 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 percent-

age 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 restricting the data to various subsamples. 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 in which 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 quantities results are robust to the inclusion of controls to address time-varying changes though the quantities results are more sensitive to analyzing various subsamples.

We use the estimates from the 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 51 percent of their prereform 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 72 percent.

## II. Institutional Background

The UAE, with an 89 percent 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 (United Nations 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, on the basis of 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 im-

pact of policy reforms that have been proposed and undertaken in Gulf countries in recent years.

Migrant workers make up 96 percent of the private workforce in the UAE (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 UAE firms directly. A signed contract and a passport (a nontrivial 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 2–3 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 paying lower fees for labor cards. Fees are higher for new recruits than for incumbent workers. The government regulates contract lengths by the types of visas granted. Before 2011, standard contracts were 3 years long; since 2011, this was shortened to 2 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 in which 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.<sup>4</sup> 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 an NOC from their existing employer in order to change employers either

<sup>4</sup> While the number of migrants in the UAE without a valid visa is unknown, it is thought to be quite small (around 5 percent of the total population) as police regularly stop workers and ask them for their papers.

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 reentering 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 in which expatriate workers trade advice for dealing with visa issues in the UAE (e.g., <http://www.dubaiforums.com/dubai-visa/> or <http://www.desertspeak.com/>). 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 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" (<http://www.desertspeak.com/viewtopic.php?t=1911>). 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 twentieth century. NOCs 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.

*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 an-

nounced the reform in December 2010, and it took effect in January 2011 (Cabinet Resolution no. 25 of 2010).<sup>5</sup> 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 applied only at the end of contracts, while in an existing contract, workers still needed an NOC to change employers without exiting the country for 6 months. Other components of the reform included some changes to visa fees for skilled workers, a shortening of the duration of standard contracts from 3 years to 2 years, as well as a lowering of the age of eligible workers from 65 to 60. The change in the duration of contracts applied only 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 for them to go, now we will lose this control” (Salem 2010b).

### III. 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 in which 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

<sup>5</sup> Our 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 App. table A1 indicate that the results cannot be driven by a policy change in a single-origin country.

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 that this intuition may not hold in general, and we provide a specific closed-form example in online appendix section 1.3.

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 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 that 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 two periods but optimize period by period (myopically), given last-period workers  $l_i^{t-1}$ . Workers similarly make decisions on the basis of current wages.<sup>6</sup> Incumbent workers will return to their source country at a rate  $q(W) \in (0, 1)$ , reflecting heterogeneity in outside options;  $q$  is decreasing and convex in  $W$ , as higher wages reduce the rate at which workers return to their source country.<sup>7</sup> The complement of this function is the staying function  $s(W) = 1 - q(W)$ , the fraction of the incumbent workers who stay as a function of  $W$ .

For incumbent workers, we let  $w^I$  denote the current wage. The pre-reform labor supply of incumbents to firm  $i$  is given by

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

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

Next we turn to the labor supply of new recruits. Because each firm has its own pool, or recruitment network, for new migrants, firms choose employment, taking the labor supply function as given. We let  $w^R$  denote the current-period wage of recruits.

We let the function  $H(\cdot)$  be the supply function of recruits and  $R(\cdot)$  be the inverse of the function  $H(\cdot)$ , which means we have

<sup>6</sup> A model with forward-looking workers is presented in online app. sec. 1.1.

<sup>7</sup> Allowing 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.

$$l_i^R = H(w_{pre}^R) \text{ so } w_{pre}^R = R(l_i^R).$$

Firms choose the prereform 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.<sup>8</sup> Labor is free to move across firms, but firms still retain some market power.<sup>9</sup> We assume that the reform does not alter the degree of competition in the market for new recruits. The postreform Cournot competition is motivated by the fact that workers are relatively homogeneous 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.<sup>10</sup>

The quit ( $q$ ) and staying ( $s$ ) functions are the same after 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,

$$\sum_{j=1}^N l_j^I = s(w_{post}^I) \sum_{j=1}^N l_j^{I-1}.$$

Inverting this, we get the postreform 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:

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

Regarding new recruits, the postreform wages are still set monopsonistically, so the new recruits’ wage equation is similar to the prereform case:

$$w_{post}^R = R(l_i^R).$$

<sup>8</sup> The predictions are not sensitive to the assumption of a Cournot equilibrium in the postreform period. Naidu, Nyarko, and Wang (2014) present a more general reduced-form model of labor market competition that leads to the same predictions.

<sup>9</sup> While 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.

<sup>10</sup> Cournot 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.

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

$$\max_{l_i^I, l_i^R} \Pi(l_i^I, l_i^R) = \max_{l_i^I, l_i^R} F(l_i^I, l_i^R) - w^I l_i^I - (w^R + v_R) l_i^R, \quad (1)$$

where  $v_R > 0$  is the nonwage 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  $l_i^{I-1}$ , but we suppress the additional notation for convenience. The difference between firms' optimization outcomes in the pre- and postreform 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

$$\epsilon^I = \frac{l_i^{I-1} w^I}{l_i^I} \left[ \frac{dw^I}{d(l_i^I / l_i^{I-1})} \right]^{-1}$$

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 prereform first-order conditions are given by

$$\frac{\partial \Pi}{\partial l^I} = 0 \Rightarrow F_{l^I}(l_{i,\text{pre}}^I, l_{i,\text{pre}}^R) = w^I \left( 1 + \frac{1}{\epsilon^I} \right), \quad (2)$$

$$\frac{\partial \Pi}{\partial l^R} = 0 \Rightarrow F_{l^R}(l_{i,\text{pre}}^I, l_{i,\text{pre}}^R) = w^R \left( 1 + \frac{1}{\epsilon^R} \right) + v_R. \quad (3)$$

These first-order conditions reflect that firms set the marginal product of each type of labor equal to its marginal cost. Owing 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  $\epsilon^I$  increases, marginal products approach wages.

Similarly, the first-order condition for new recruits incorporates the elasticity of labor supply,  $\epsilon^R$ , as well as the cost of recruitment,  $v_R$ . We have 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 postreform symmetric Cournot equilibrium. Assuming  $N$  identical firms and symmetry in firms' decisions, we will have

$l_j^{I-1} = l_i^{I-1}$ ,  $l_{j,\text{post}}^R = l_{i,\text{post}}^R$ , and  $l_{j,\text{post}}^I = l_{i,\text{post}}^I$  for all firms  $i, j$ . We have the following postreform first-order conditions:

$$\frac{\partial \Pi}{\partial l^I} = 0 \Rightarrow F_{l^I}(l_{i,\text{post}}^I, l_{i,\text{post}}^R) = w^I \left( 1 + \frac{1}{N\epsilon^I} \right) \tag{4}$$

and

$$\frac{\partial \Pi}{\partial l^R} = 0 \Rightarrow F_{l^R}(l_{i,\text{post}}^I, l_{i,\text{post}}^R) = w^R \left( 1 + \frac{1}{\epsilon^R} \right) + v_R. \tag{5}$$

The difference here from equations (2) and (3) 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 postreform first-order conditions is the  $1/N$  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 prereform solution is simply the postreform solution at  $N = 1$ . Indeed, as  $N$  approaches infinity, the postreform 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  $\Pi$  is strictly concave in  $l^I$  and  $l^R$  and new recruits and incumbent workers are substitutes in  $F$ , so that  $F_{l^I l^R} < 0$ , we have the following comparative statics that result from the reform:

- The share of incumbent workers staying with a firm goes up:

$$\Delta s \equiv \frac{l_{i,\text{post}}^I - l_{i,\text{pre}}^I}{l_i^{I-1}} > 0;$$

- incumbent workers' wages rise:

$$\Delta w^I \equiv w^I \left( \frac{l_{i,\text{post}}^I}{l_i^{I-1}} \right) - w^I \left( \frac{l_{i,\text{pre}}^I}{l_i^{I-1}} \right) > 0;$$

- employment of new recruits falls:  $\Delta l^R \equiv l_{i,\text{post}}^R - l_{i,\text{pre}}^R < 0$ ;
- wages of new recruits fall:

$$\Delta w^R \equiv w^R \left( l_{i,\text{post}}^R, \frac{l_{i,\text{post}}^I}{l_i^{I-1}} \right) - w^R \left( l_{i,\text{pre}}^R, \frac{l_{i,\text{pre}}^I}{l_i^{I-1}} \right) < 0.$$

*Proof.* This follows from implicitly differentiating equation (4) with respect to  $N$ . See online appendix sections 1.2 and 1.3 for details and an example, respectively.

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. The reason is that 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.<sup>11</sup>

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. In contrast to 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 reflect the fact that labor market competition has been reduced for incumbent workers only, but these two types of workers are substitutes.

## IV. Data

### A. *Salary Disbursal Data*

The data on wage disbursements of migrant workers are from a company in the UAE called UAE Exchange.<sup>12</sup> 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 percent of the total migrant workforce in the country. Many firms, including quite

<sup>11</sup> This prediction reflects Bresnahan's (1982) argument on identifying market power. Bresnahan argued that exogenous variables that changed the elasticity (i.e., the slopes) but did not affect the level of demand or supply should have no effect in competitive markets, but should alter prices and quantities in markets with oligopsonistic power. The number of other firms an employer is competing with to retain incumbent workers, which goes from zero to  $N - 1$ , is such a variable.

<sup>12</sup> See Joseph, Nyarko, and Wang (2015) for additional information on the data sets.

large and small ones, use a payroll-processing firm in order to adhere to the wage protection system, which was implemented by the government in 2009 and 2010 (with larger firms required to implement the system earlier) to protect workers by creating electronic records of wage payments.

We have monthly payroll disbursements for the period from January 2009 to October 2012. Recall that the reform went into effect in January 2011, so the data span both sides of the reform. The entire sample of earnings disbursements includes 427,265 unique individuals working in 20,366 firms. In the UAE, salaries are paid out on a monthly basis.<sup>13</sup> 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.

### *B. 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 allow 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 they allow us to identify whether a worker who disappears from the UAE Exchange data set

<sup>13</sup> In the less than 5 percent 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.

switches to another firm that does not use UAE Exchange for payroll processing or leaves the MOL data entirely.<sup>14</sup> 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 include only migrant labor contracts that fall under the jurisdiction of the MOL. Domestic workers fall under the jurisdiction of the Ministry of the Interior, as do any workers who work in free-zone areas of the UAE.<sup>15</sup> A comparison of the MOL data to UN population figures for migrant workers in the UAE in 2012 suggests that the MOL data cover approximately 80 percent of all migrant workers in the country.

In addition to the start and end dates of contracts, the MOL data also include other details of each labor contract, including contracted hours, earnings, and total compensation.<sup>16</sup> 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 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 identification number. Thus, the matching between the two data sets is straightforward and is outlined in more detail in online appendix section 2.1. We are able to match 81 percent of the observations in the payroll data with their corresponding contracts in the MOL data, and Appendix figure A1 shows that the earnings densities between the matched and unmatched payroll observations are virtually identical. Appendix figure A2 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 are more oriented toward the median and lower end of the salary distribution of migrants and undersample migrant workers at the top end of the salary distribution.

<sup>14</sup> However, a limitation of the data is that we cannot distinguish voluntary worker separations (quits) from involuntary separations.

<sup>15</sup> Free zones are industrial parks throughout the UAE that provide special incentives for foreign investments, such as tax breaks and fewer restrictions on foreign ownership. The bulk of the free zones are in the vicinity of the cities of Dubai and Sharjah.

<sup>16</sup> Total compensation includes the value specified in the contract for housing, food, and transportation.

## V. Descriptive Statistics

### A. *Administrative Contracts Data*

We begin by showing summary statistics from the MOL contracts data, which provide the universe of labor contracts under the jurisdiction of the MOL. Figure 1 shows the real change in the compensation stipulated in the new contract compared to the previous contract by the expiration date of the previous contract. Compensation includes both earnings and the value of employer-provided housing and transportation. This includes both employer transitions and individuals who re-sign contracts with their previous employers. The vertical line indicates December 2010, the date on which 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.<sup>17</sup>

In figure 2, we show the total number of workers who re-sign contracts with their previous employers by the expiration date of the contract. We see an increase in the number of workers who are retained by their existing employers after the reform.<sup>18</sup> 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 an 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 in the UAE after the reform.<sup>19</sup>

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 is generally quite large.

### B. *Salary Disbursal and Administrative Contracts Merged Data*

Table 1 presents the summary statistics for the sample used in our estimation. Columns 1–3 show the mean, standard deviation, and number of observations for the person-month for the months between January 2009

<sup>17</sup> Note 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.

<sup>18</sup> Figures 2 and 3 include only employer stays and transitions that occur within 3 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 are the results of this and disappear when we use only immediate transitions (available on request).

<sup>19</sup> The MOL data do not directly indicate when migrants leave the UAE, so we do not present a corresponding figure with country exits.

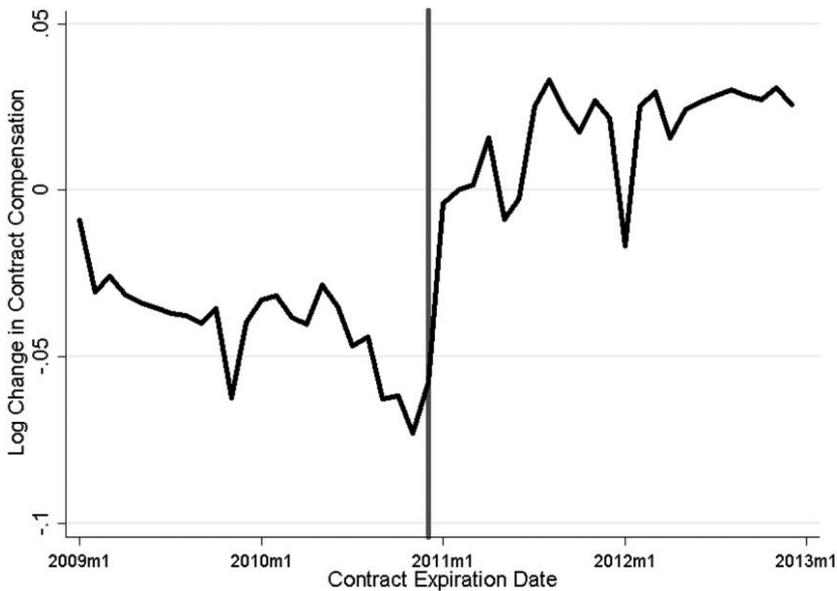


FIG. 1.—Average change in log real compensation by contract expiration date. The figure shows the average log change in real compensation from one contract in time  $t$  to the next contract in  $t + 1$ , where  $t$  is the month of expiration of the preceding contract. Compensation includes the value of earnings and benefits defined in the contract. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data. Color version available as an online enhancement.

and December 2010. Columns 4–6 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 disbursement that the worker received.<sup>20</sup> The average log earnings is a little over seven; this corresponds to about 1,100 dirham or US\$300. This is the actual earnings disbursement reported by the payroll-processing firm and does not include the value of in-kind benefits. A simple pre-post comparison shows a small increase in average real earnings following the reform.

Stay with firm is a time-varying variable that is 100 if the individual stays with the same firm as in the previous period, and zero otherwise.<sup>21</sup> 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 percent of individuals stay with the same

<sup>20</sup> We 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.

<sup>21</sup> See online app. sec. 2.2 for more information on the construction of this and other variables.

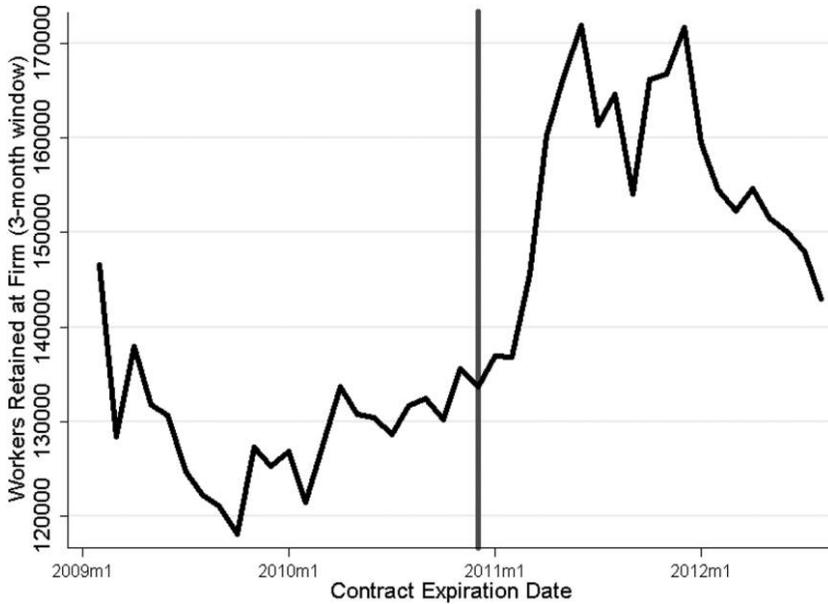


FIG. 2.—Total workers retained by firm by contract expiration date. The figure shows the total number of workers who sign a new contract with their existing firm by contract expiration date. This is smoothed over the two adjacent months. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data. Color version available as an online enhancement.

firm in the months observed prior to the reform, and this increases slightly to 96 percent after the reform.

Individuals who do not stay with their existing firm either exit the UAE or switch employers. Exit UAE is a variable that equals 100 if the individual leaves the sample for at least 6 months, and zero otherwise. There is some noise in this measure as individuals may move within the UAE but out of the jurisdiction of the MOL to a free-zone area and would be counted as exiting the UAE. The rate of exiting prior to the reform was 4.8 percent per month; after the reform, this falls to 3.3 percent per month.

Employer change equals 100 if the individual changed firms within the past 3 months, and zero otherwise. Prior to the reform, the rates of employer change are quite low at 0.2 percent per month (or 2.4 percent per year), which translates to only two workers per thousand who changed employers each month. This low rate should not be that surprising in the prereform 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.

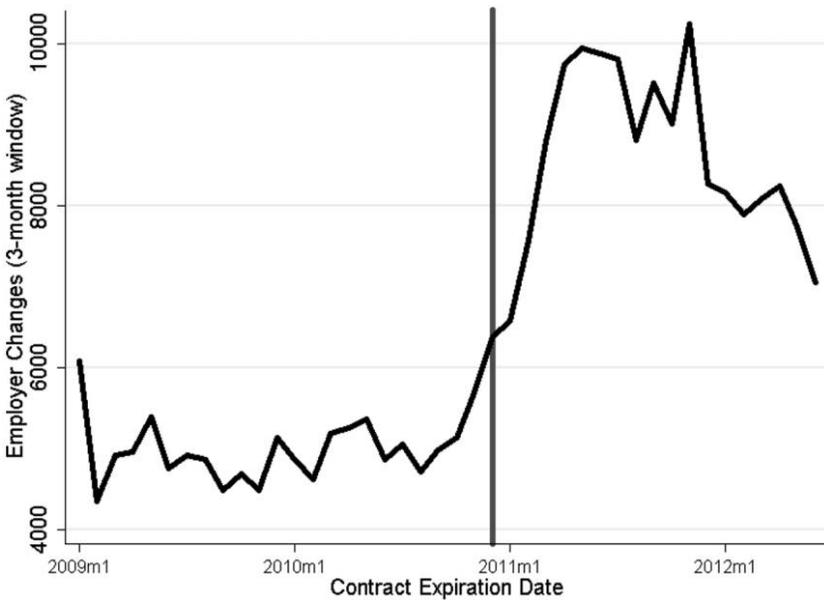


FIG. 3.—Total changes in employer by contract expiration date. The figure shows the total number of employer changes that occur by contract expiration date. The total includes employer transitions that occur within 3 months of the contract expiration. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data. Color version available as an online enhancement.

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.<sup>22</sup> The average age of workers is mid-30s, and the vast majority of them are male. Educated is an indicator variable that equals one if the person has higher than intermediate education.<sup>23</sup> The pool of educated workers increases substantially after the reform. Over 60 percent 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

<sup>22</sup> The industry of each 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.

<sup>23</sup> Intermediate education is classified as some secondary schooling without completing the degree.

TABLE 1  
SUMMARY STATISTICS BY PERSON-MONTH

	PREREFORM			POSTREFORM		
	Mean (1)	Standard Deviation (2)	Observations (3)	Mean (4)	Standard Deviation (5)	Observations (6)
Log monthly earnings	7.013	.560	183,543	7.039	.652	345,959
Stay with firm ( $\times 100$ )	94.60	22.60	179,656	95.84	19.97	346,081
Exit UAE ( $\times 100$ )	4.852	21.49	192,906	3.368	18.04	358,027
Employer change ( $\times 100$ )	.194	4.400	177,858	.693	8.298	336,748
Stayer	.973	.161	193,972	.962	.192	361,415
Construction	.705	.456	144,524	.565	.496	217,693
Age	36.68	8.304	193,972	35.89	8.417	361,415
Male	.998	.0389	193,972	.994	.0794	361,400
Educated	.276	.447	181,559	.388	.487	354,998
Dubai-Sharjah	.652	.476	193,960	.604	.489	361,415
Indian	.507	.500	190,617	.446	.497	358,899
Hours (lower bound)	254.3	50.66	100,246	238.2	54.62	202,225
Hours (upper bound)	264.3	60.67	100,246	244.9	65.46	202,225

NOTE.—The table shows the mean, standard deviation, and number of observations for each variable. Prereform pools all months of data prior to the implementation of the NOC reform in January 2011. Postreform pools all months of data after and including January 2011. Earnings are in real 2007 dirham.

in the selection of individuals into or out of the country over time. We address the concern that the results may be driven by changes in the characteristics of workers in two ways. First, we allow for time-varying effects of observable worker characteristics. However, there may also be changes in characteristics that we cannot observe. In Section VI.E, 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 on the basis of the assumption that variation in earnings month to month is a reflection of variation in hours. The upper bound of hours worked per month is constructed on the basis of 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.<sup>24</sup> The average number of hours worked per month falls from around 260 in the prereform period to 240 in the postreform period.

<sup>24</sup> UAE law stipulates rates of overtime between 1.25 and 1.5 depending on the time of the day and day of the week when the extra hours take place.

## VI. Estimation Strategy and Results

### A. 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 4 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 change 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. The reason is that individuals whose contracts expired in 2010 versus in 2011 have contracts that began in 2007 and 2008, respectively, and this precedes serious discussion of such reforms in the UAE.<sup>25</sup> Our specification also includes individual fixed effects, which removes any time-invariant differences across the groups. Finally, we also estimate a specification that controls for pre- and post-reform effects of 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 seven periods per individual. We look at the 3 months leading up to an individual's contract expiration, the period of the contract expiration, and the 3 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 three leads and lags around the time of the contract expiration. First, it allows us to examine whether there are pre-expiration trends that sug-

<sup>25</sup> Using the MOL data, online app. fig. 1 shows that there is no break in either average contract compensation or the number of new contracts 3 years prior to the announcement of the reform.

gest 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 1–2 months following a contract expiration.<sup>26</sup> Thus, any post–contract expiration effects may not show up in just 1 month.

*B. Empirical Specifications*

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

$$y_{it} = \sum_{k=-3}^3 \gamma_k^{\text{Post2011}} D_{it+k}^{\text{Post}} + \sum_{k=-3}^3 \gamma_k^{\text{Pre2011}} D_{it+k}^{\text{Pre}} + \delta_i + \delta_t + \epsilon_{it}, \tag{6}$$

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.<sup>27</sup> 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 seven contiguous months centered around a contract expiration, so  $k = -3$  corresponds to three periods before the contract expires and  $k = 3$  corresponds to three periods after the previous contract expired. Thus, the coefficient  $\gamma_k^{\text{Pre2011}}$  provides the effect of the contract expiration prior to the 2011 reform, and the coefficient  $\gamma_k^{\text{Post2011}}$  provides the effect of the contract expiration after the 2011 reform. We are most interested in whether the effects of contract expirations after the reform are different from the effects prior to the reform, and that is given by the estimates of  $\gamma^{\text{Post2011}} - \gamma^{\text{Pre2011}}$ . We also include year-month fixed effects and individual fixed effects. The standard errors are robust and are clustered by individual.

*C. Graphical Representation of Estimates*

Given the large number of coefficients, we show graphical plots of  $\gamma_k^{\text{Post2011}}$  and  $\gamma_k^{\text{Pre2011}}$  from estimates of equation (6). Figure 4 displays the coefficients together with 95 percent 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 postreform coefficient ( $\gamma^{\text{Post2011}}$ ) while the other line presents the prereform coefficient ( $\gamma^{\text{Pre2011}}$ ). The figure shows that prior to the re-

<sup>26</sup> This is true regardless of whether they change employers or not.

<sup>27</sup> The first three outcome variables correspond roughly with  $w^l$ ,  $s(W)$ , and  $q(W)$ , respectively. Recall that employer transitions are not explicitly modeled.

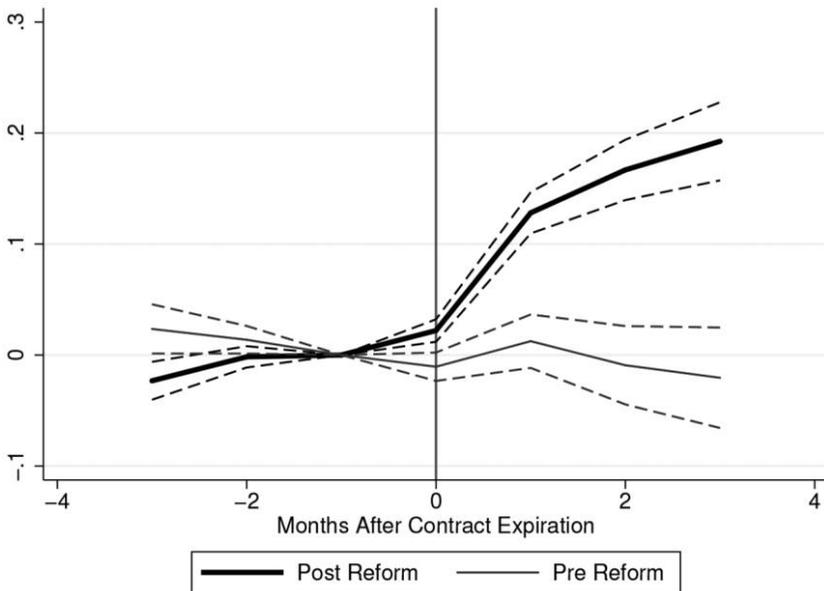


FIG. 4.—Impact of a contract expiration on log earnings: pre- and postreform. The figure displays the coefficient estimates of each period around a contract expiration separately for expirations that occur before and after reform. The dependent variable is log earnings. The regressions include individual 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 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

form, 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 an 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 postreform effects in the periods prior to the expiration.

Figure 5 presents the estimates in which 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 in which the dependent variable is whether the individual exits the UAE. Consistent with the limited op-

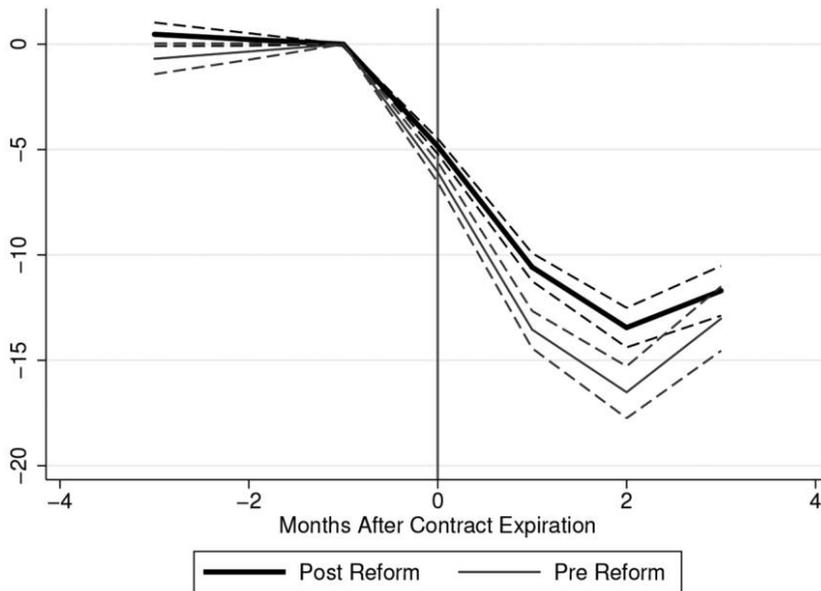


FIG. 5.—Impact of a contract expiration on firm retention: pre- and postreform. The figure displays the coefficient estimates of each period around a contract expiration separately for expirations that occur before and after reform. The dependent variable is firm retention. The regressions include individual 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 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

tions 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 (6) in which the dependent variable is employer transitions. In both the prereform and postreform periods, the pre-contract expiration trends show no employer transitions in the 3 months prior to the contract expiration. There is a significant increase in the probability of employer transitions in the prereform period. In the postreform period, there is a significantly larger probability of employer transitions relative to the prereform period that occurs immediately in the month of expiration but then declines 3 months after the expiration.

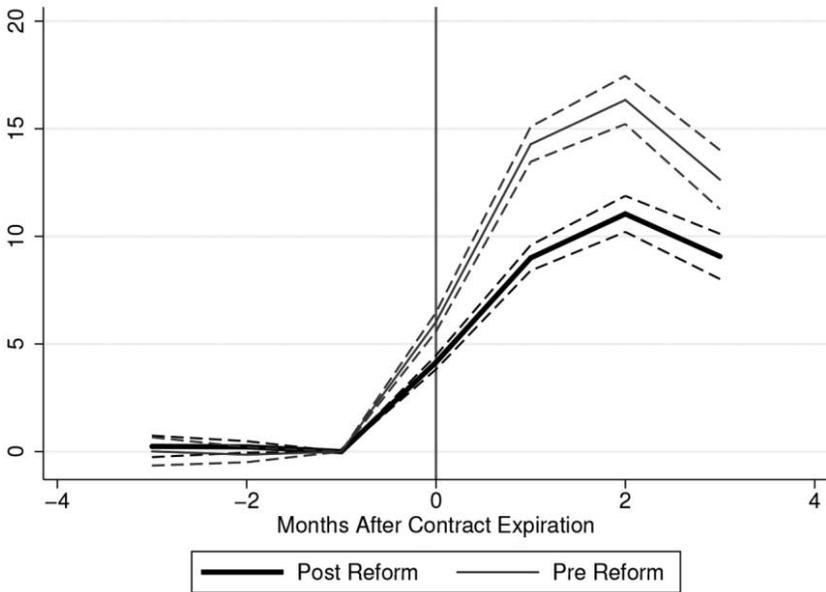


FIG. 6.—Impact of a contract expiration on country exits: pre- and postreform. The figure displays the coefficient estimates of each period around a contract expiration separately for expirations that occur before and after reform. The dependent variable is country exits. The regressions include individual 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 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

Overall, these results are consistent with the predictions of the impact of reducing monopsony power of firms and moving toward 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 should 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 the 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

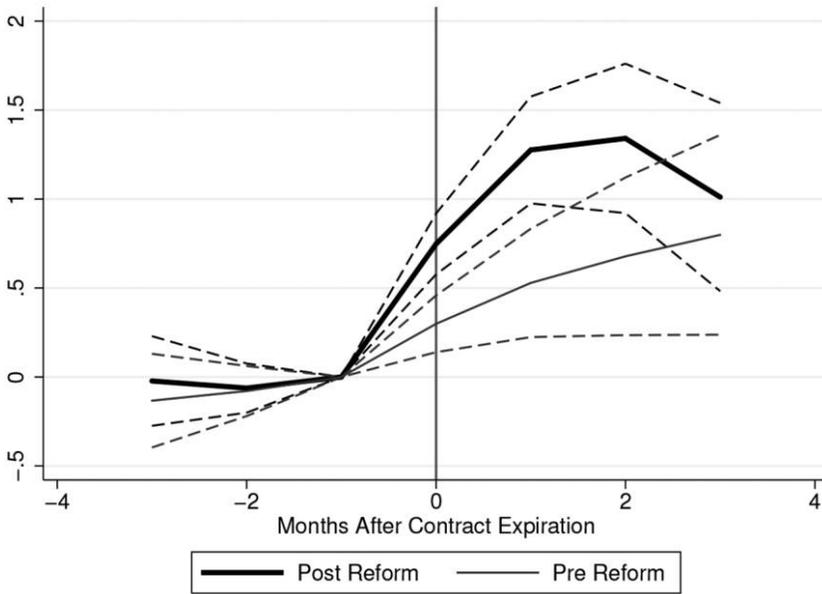


FIG. 7.—Impact of a contract expiration on employer changes: pre- and postreform. The figure displays the coefficient estimates of each period around a contract expiration separately for expirations that occur before and after reform. The dependent variable is employer changes. The regressions include 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 percent confidence interval. The vertical line indicates the month in which the worker’s contract expires. Color version available as an online enhancement.

than being driven by transitions increasing the match quality between workers and firms.

*D. Estimates of Reform Effects*

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

$$y_{it} = \sum_{k=0}^3 \gamma_k^{\text{Post2011}} D_{it+k}^{\text{Post}} + \sum_{k=0}^3 \gamma_k^{\text{Pre2011}} D_{it+k}^{\text{Pre}} + \delta_i + \delta_t + \epsilon_{it}. \tag{7}$$

The key difference from equation (6) is that we omit the leads to contract expiration ( $-3 \leq k < 0$ ), so the coefficient estimates are relative to all 3 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 parsimo-

nious enough to display in tables. The main hypothesis to be tested is whether  $\sum_{k=1}^3 \gamma_k^{\text{Post2011}} - \gamma_k^{\text{Pre2011}} = 0$  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  $\gamma_0^{\text{Post2011}} - \gamma_0^{\text{Pre2011}} = 0$ , which we show in the third row of each panel of tables 2–4. 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 (7), 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 on which the current contract expires and the reform, separately for before and after the reform. We do this 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.<sup>28</sup> 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 subsample in which we discard the data in the first and last calendar quarters of the sample, together with the quarters immediately preceding and immediately following the reform (quarter 4 of 2010 and quarter 1 of 2011). Dropping the first and last quarters 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, postreform  $\times$  post-contract expire, which corresponds to  $\sum_{k=1}^3 \gamma_k^{\text{Post2011}} - \gamma_k^{\text{Pre2011}}$  in equa-

<sup>28</sup> The last columns of online app. table 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.

TABLE 2  
EFFECT ON LOG EARNINGS AND FIRM RETENTION

	FULL SAMPLE			BOTH SIDES	TRIMMED
	(1)	(2)	(3)		
A. Log Earnings					
Postreform × post–contract expire	.113*** (.009)	.110*** (.009)	.109*** (.011)	.091*** (.010)	.139*** (.012)
Postreform × period contract expire	.015*** (.005)	.013** (.005)	.016** (.006)	.049*** (.005)	.017*** (.006)
Observations	529,502	529,502	342,555	463,312	447,394
Number of clusters	111,006	111,006	69,239	88,290	105,606
R <sup>2</sup>	.017	.017	.023	.010	.019
B. Staying with Firm					
Postreform × post–contract expire	3.832*** (.304)	4.333*** (.321)	6.387*** (.405)	3.106*** (.282)	4.270*** (.393)
Postreform × period contract expire	1.642*** (.205)	1.881*** (.224)	2.743*** (.242)	.223** (.114)	1.759*** (.266)
Observations	525,737	525,737	343,503	466,806	445,200
Number of clusters	110,120	110,120	68,931	88,293	105,448
R <sup>2</sup>	.082	.082	.370	.093	.081
Polynomials in time to reform	No	Yes	Yes	No	No
Worker characteristics	No	No	Yes	No	No

NOTE.—All specifications include individual fixed effects, year-month fixed effects, and a constant term. Standard errors clustered by individual are in parentheses. The full sample includes all months from January 2009 to October 2012. The both sides subsample restricts attention to workers with wage observations both before and after the reform. The trimmed subsample excludes the last quarters of 2010 and 2012 and the first quarters of 2009 and 2011. Postreform × post–contract expire corresponds to  $\sum_{k=1}^3 \gamma_k^{\text{Post2011}} - \gamma_0^{\text{Pre2011}}$ , and postreform × period contract expire corresponds to  $\gamma_0^{\text{Post2011}} - \gamma_0^{\text{Pre2011}}$ . Postreform 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 in which the individual’s contract expires.

- \* Significant at the 10 percent level.
- \*\* Significant at the 5 percent level.
- \*\*\* Significant at the 1 percent level.

tion (7), and postreform × period contract expire, which corresponds to  $\gamma_0^{\text{Post2011}} - \gamma_0^{\text{Pre2011}}$ , for log earnings.<sup>29</sup> This corresponds to  $\Delta w^l$  in the theoretical model. Column 1 shows results from the specification with no controls. The top row of the table shows the pooled effect of all 3 months after the contract expiration interacted with the postreform dummy, minus the pooled effect of all 3 months after the contract expiration inter-

<sup>29</sup> Online app. tables 2–5 display all of the corresponding estimates of  $\gamma_k^{\text{Post2011}}$  and  $\gamma_k^{\text{Pre2011}}$  for the four main outcomes of interest. The first four coefficients are  $\gamma_3^{\text{Post2011}} \dots \gamma_0^{\text{Post2011}}$ , while the next four are  $\gamma_3^{\text{Pre2011}} \dots \gamma_0^{\text{Pre2011}}$ . Clearly, in online app. table 2, there is a differential increase in earnings following a contract expiration after the labor reform. This occurs both in the month of expiration and in the 3 months afterward.

acted with the prereform indicator.<sup>30</sup> The standard error is reported below the coefficient. The second row shows the differential effect on the month of expiration, and this is always positive and significant, as well as always smaller than the average effect in the subsequent 3 months, consistent with substantial heterogeneity in exactly when in the month contracts expire.

The magnitudes of the effects are substantial. We find an 11 percent effect on real monthly earnings; that is, monthly earnings grow by about 11 percent in the 3 months following a contract expiration after the reform, with an increase of 1.4 percent in the month of contract expiration. Column 2 includes polynomials in time to reform, and column 3 includes both the time to reform polynomials and the time-varying effects of individual characteristics. The coefficients are very similar across specifications, with a 1.3–1.5 percent additional increase in earnings in the month of a contract expiration following the reform and a mean increase of 11 percent in the 3 months following a contract expiration after 2011. These estimates are all significant at the 1 percent level.

Column 4 corresponds with column 1 except that the sample is restricted to workers with at least one earnings observation before and after the reform. The coefficients on the month of postreform contract expiration increase to around 5 percent, while the mean earnings increase over the subsequent 3 months is roughly 9 percent. Column 5 estimates the same specifications, but now the sample omits the first and last quarters 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 subsamples remain significant at the 1 percent level.

One possible concern is that we are observing only earnings, 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 in which the dependent variable is the probability of staying with the same firm (times 100) in panel B of table 2. This corresponds to  $\Delta s$  in the model. After the reform, workers are more

<sup>30</sup> Note that the pooled average effect is given by

$$\sum_{k=1}^3 \frac{\gamma_k^{\text{Post2011}} - \gamma_k^{\text{Pre2011}}}{3}.$$

TABLE 3  
EFFECT ON HOURS VARIABLES

	HOURS UPPER ESTIMATE		HOURS LOWER ESTIMATE	
	(1)	(2)	(3)	(4)
Postreform $\times$ post-contract expire	-1.487 (1.194)	-.094 (1.680)	-1.239 (.995)	-.078 (1.400)
Postreform $\times$ period contract expire	-.388 (.649)	-.820 (.911)	-.324 (.541)	-.683 (.759)
Observations	302,471	186,812	302,471	186,812
Number of clusters	72,897	44,295	72,897	44,295
$R^2$	.005	.013	.005	.013

NOTE.—In calculating hours, we assume that overtime hours are paid at a rate of 1.25 in cols. 1 and 2 and 1.5 in cols. 3 and 4. All specifications include individual fixed effects, year-month fixed effects, and a constant term. Standard errors clustered by individual are in parentheses. The sample includes all months from January 2009 to October 2012. Postreform  $\times$  post-contract expire corresponds to  $\sum_{k=1}^3 \gamma_k^{\text{Post}2011} - \gamma_k^{\text{Pre}2011}$ , and postreform  $\times$  period contract expire corresponds to  $\gamma_0^{\text{Post}2011} - \gamma_0^{\text{Pre}2011}$ . Postreform 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 in which the individual's contract expires.

likely to stay with their existing firm. Across the various specifications and subsamples, the effect size range implies that workers are 3–6.4 percentage points (22–44 percent) more likely to continue working for the same employer following a contract expiration. This is significant at the 1 percent 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 table 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 percent 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 in which 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 in which 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 ex-

TABLE 4  
EFFECT ON EXITS FROM UAE AND EMPLOYER TRANSITIONS

	FULL SAMPLE			BOTH SIDES (4)	TRIMMED (5)
	(1)	(2)	(3)		
A. Exits from UAE					
Postreform $\times$ post-contract expire	-4.408*** (.271)	-4.749*** (.287)	-6.608*** (.370)	-3.570*** (.255)	-4.756*** (.355)
Postreform $\times$ period contract expire	-1.822*** (.184)	-1.983*** (.200)	-3.094*** (.227)	-.201** (.089)	-2.065*** (.239)
Observations	550,933	550,933	356,203	477,737	465,333
Number of clusters	111,319	111,319	69,442	88,290	106,789
R <sup>2</sup>	.084	.084	.347	.092	.083
B. Employer Changes					
Postreform $\times$ post-contract expire	.663*** (.102)	.639*** (.109)	.255** (.117)	.327*** (.097)	.633*** (.129)
Postreform $\times$ period contract expire	.491*** (.070)	.478*** (.078)	.311*** (.076)	-.311*** (.047)	.546*** (.088)
Observations	514,606	514,606	335,281	459,035	434,276
Number of clusters	109,388	109,388	68,495	88,293	104,121
R <sup>2</sup>	.006	.006	.153	.008	.006
Polynomials in time to reform	No	Yes	Yes	No	No
Worker characteristics	No	No	Yes	No	No

NOTE.—All specifications include individual fixed effects, year-month fixed effects, and a constant term. Standard errors clustered by individual are in parentheses. The full sample includes all months from January 2009 to October 2012. The both sides subsample restricts attention to workers with wage observations both before and after the reform. The trimmed subsample excludes the last quarters of 2010 and 2012 and the first quarters of 2009 and 2011. Postreform  $\times$  post-contract expire corresponds to  $\sum_{k=1}^3 \gamma_k^{\text{Post2011}} - \gamma_k^{\text{Pre2011}}$ , and postreform  $\times$  period contract expire corresponds to  $\gamma_0^{\text{Post2011}} - \gamma_0^{\text{Pre2011}}$ . Postreform 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 in which the individual's contract expires.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

piration, with an extra 0.66 percentage point increase per month on average over the next 3 months.<sup>31</sup> These estimates are significant at the 1 percent level.

Alternatively, one could consider the magnitude of the effects on employer transitions on a per-contract basis rather than on a per-month

<sup>31</sup> Looking at the individual lags in online app. table 5, we can see that the postreform coefficient is smallest in the last lag, consistent with the relatively short window workers have to find a new employer.

basis. Individuals can exit a contract (for another firm or to leave the country) only once per contract. The per-contract impact is given by  $\sum_{k=1}^3 \gamma_k^{\text{Post2011}} - \gamma_k^{\text{Pre2011}}$ . 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 percent but still significant at the 1 percent 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 threat of changing employers can have effects on earnings without actual transitions.<sup>32</sup> 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 multiyear contract without needing approval from their previous employers increased employer

<sup>32</sup> An analogy can be found in no-fault divorce laws, which increased settlement payments but did not appreciably increase the divorce rate (Peters 1986).

transitions, worker retention, and earnings and decreased the likelihood of leaving the UAE for at least 6 months.

*E. 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.<sup>33</sup> We implement two bounds, one wider and thus more conservative than the other more “naive” bounds.<sup>34</sup> 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 seven-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 before and after the reform. In other words, we calculate  $\tilde{y}_v^{tw}$ , where  $t$  denotes pre- or postreform,  $v$  denotes the 90th or 10th percentile, and  $w$  equals one for after contract expiration and zero for before contract expiration.

To impute an upper bound on our coefficients, we assume that all exits following a contract expiration after the reform have the 90th percentile value,  $\tilde{y}_{90}^{\text{Post1}}$ , and all exits prior to a contract expiration, but after the reform, have the 10th percentile value,  $\tilde{y}_{10}^{\text{Post0}}$ . Similarly, we impute  $\tilde{y}_{90}^{\text{Pre0}}$  for all pre-contract expiration exits prior to the reform and  $\tilde{y}_{10}^{\text{Pre1}}$  for all the post-contract expiration exits prior to the reform. For the lower bound, we impute  $\tilde{y}_{90}^{\text{Post0}}$  to all exits prior to a contract expiration but following the reform and  $\tilde{y}_{10}^{\text{Post1}}$  to all exits following a contract expiration after the reform. The parallel assignment is done with  $\tilde{y}_{90}^{\text{Pre1}}$  and  $\tilde{y}_{10}^{\text{Pre0}}$  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 (7) 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

<sup>33</sup> This selection can include exiting the UAE entirely, entering the informal market, or working in free zones outside of the jurisdiction of the MOL.

<sup>34</sup> The 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 online app. table 6.

TABLE 5  
EFFECT ON IMPUTED OUTCOME VARIABLES

	LOG EARNINGS IMPUTED		EMPLOYER CHANGE IMPUTED	
	High (1)	Low (2)	High (3)	Low (4)
Postreform × post-contract expire	.185*** (.009)	.031*** (.009)	.644*** (.098)	.558*** (.098)
Postreform × period contract expire	.046*** (.005)	-.019** (.005)	.480** (.067)	.445*** (.067)
Observations	550,920	550,920	536,024	536,024
Number of clusters	111,319	111,319	110,337	110,337
R <sup>2</sup>	.016	.012	.005	.005

NOTE.—The dependent variable in cols. 1 and 2 is log earnings, and in cols. 3 and 4, the dependent variable is the probability of changing employers. For the high imputations, we impute the 90th percentile value for prereform pre-expiration and for postreform post-expiration and we impute the 10th percentile value for prereform postexpiration and post-reform pre-expiration. For the low imputations, we impute the 10th percentile value for prereform pre-expiration and for postreform postexpiration and we impute the 90th percentile value for prereform postexpiration and postreform pre-expiration. All specifications include individual fixed effects, year-month fixed effects, and a constant term. Standard errors clustered by individual are in parentheses. Postreform × post-contract expire corresponds to  $\sum_{k=1}^3 \gamma_k^{Post2011} - \gamma_k^{Pre2011}$ , and postreform × period contract expire corresponds to  $\gamma_0^{Post2011} - \gamma_0^{Pre2011}$ . Postreform 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 in which the individual’s contract expires.

- \* Significant at the 10 percent level.
- \*\* Significant at the 5 percent level.
- \*\*\* Significant at the 1 percent level.

the odd columns, while the even columns show lower bounds 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 percent, while the lower bound is 3 percent. Thus, both remain positive and statistically significant at the 1 percent 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.

F. *Time-Shifted Placebos*

In order to rule out further sources of unobserved trends driving the results, we conduct a falsification exercise in which we assume that con-

tract expiration dates are uniformly shifted backward by multiples of 3 months from 0 to 18. Formally, we reestimate equation (7), 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 postreform or prereform.

Appendix figure A3 shows the resulting plots of

$$\sum_{k=1}^3 \frac{\tilde{\gamma}_{i,t+k}^{\text{Post2011},j} - \tilde{\gamma}_{i,t+k}^{\text{Pre2011},j}}{3}$$

for log earnings. Consistent with our previous results, the only positive and significant coefficient occurs where  $j = 0$ , which corresponds to our main specification. Appendix figure A5 shows the same plot for employment changes, while figure A4 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.

## VII. 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 3 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 is 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:

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

where  $\log \text{Expire}_{jt}$  is the logarithm of the number of labor contracts expiring in period  $t$  at firm  $j$ . The regression also includes firm fixed effects

and year-month fixed effects. Errors are clustered at the firm level. We are interested in whether the impact of worker contract expirations on firms' hiring outcomes changes after the reform as compared with prior to the reform. This is given by the estimate of  $\beta^{\text{Post2011}} - \beta^{\text{Pre2011}}$ .

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 remain equally high after the reform as compared to before the reform. In contrast, the firm may anticipate periods in which there are higher levels of contract expirations and can change its 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:

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

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

The data used in this analysis aggregate the worker-level data to the firm level and include 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  $\text{postreform} \times \log \text{contracts expiring}$ , which corresponds to  $\beta^{\text{Post2011}} - \beta^{\text{Pre2011}}$ . Column 1 presents the parsimonious specification. Column 2 includes fixed effects for each city by year-month to control for time-varying city-level changes. Column 3 includes the three leads and lags of the logarithm of contract expirations corresponding to equation (9). Column 4 limits the sample to firms with observations both before and after the reform. The sample specification removes firms that do not exist prior to the reform and firms that die after the reform. Finally, column 5 is the trimmed sample that omits the first and last calendar quarters of the data as well as the quarters immediately surrounding the announcement and implementation of the reform.

TABLE 6  
IMPACT OF THE REFORM ON NEW ENTRANTS' OUTCOMES

	FULL SAMPLE			BOTH SIDES (4)	TRIMMED (5)
	(1)	(2)	(3)		
A. Log Entrants					
Postreform × log contracts expiring	-.035* (.021)	-.030 (.020)	.019 (.013)	-.065*** (.022)	-.074*** (.028)
Postreform × leads contracts expiring			-.021** (.008)		
Postreform × lags contracts expiring			-.013* (.007)		
Observations	224,646	224,646	111,478	121,295	89,448
Number of clusters	17,891	17,891	10,055	5,692	5,473
R <sup>2</sup>	.131	.143	.044	.156	.148
Mean of dependent variable	.137	.137	.102	.157	.160
Standard deviation of dependent variable	.462	.462	.393	.511	.523
B. Log Entrant Earnings					
Postreform × log contracts expiring	-.034* (.019)	-.034* (.019)	-.014 (.023)	-.022 (.021)	.002 (.025)
Postreform × leads contracts expiring			-.022** (.010)		
Postreform × lags contracts expiring			.013 (.009)		
Observations	27,437	27,437	9,749	15,189	10,945
Number of clusters	12,868	12,868	3,592	4,936	3,943
R <sup>2</sup>	.010	.024	.027	.009	.008
Mean of dependent variable	7.065	7.065	7.008	7.052	7.042
Standard deviation of dependent variable	.753	.753	.753	.715	.713
Leads and lags	No	No	Yes	No	No
City × year-month fixed effects	No	Yes	No	No	No

NOTE.—The dependent variable is indicated by the panel label. All specifications include firm and year-month fixed effects. Standard errors clustered by firm are in parentheses. 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 subsample restricts attention to workers with wage observations both before and after the reform. The trimmed subsample excludes the last quarters of 2010 and 2012 and the first quarters of 2009 and 2011. Postreform is an indicator that equals one after the announcement of the NOC reform in December 2010. Postreform × log contracts expiring corresponds to  $\beta^{\text{Post2011}} - \beta^{\text{Pre2011}}$ . 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 3 months prior to that month. Lags contracts expiring is the total effect of the log of the number of contracts expiring in the 3 months after that month.

- \* Significant at the 10 percent level.
- \*\* Significant at the 5 percent level.
- \*\*\* Significant at the 1 percent level.

Panel A of table 6 presents the impact of the reform on hiring new entrants to the UAE, or  $\Delta l_i^R$  in the model.<sup>35</sup> 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 percentage 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 include only firms that exist on both sides of the reform and in the trimmed sample, and the results are significant at the 1 percent level. With the inclusion of the leads and lags of contract expirations (col. 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 impacts of the leads are jointly negative and significant at the 5 percent level. This suggests that firms may anticipate the effects of their workers' contract expirations in the months leading up to them and adjust their decisions on hiring new entrants before the actual contract expirations are realized.

We examine the impact of the reform on the earnings paid to new entrants, which is  $\Delta w^R$  in the model, in panel B of table 6.<sup>36</sup> 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 percent 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 3 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

<sup>35</sup> We also examine whether the types of new entrants to the UAE change with the reform. Online app. table 7 shows that the characteristics of new entrants are very similar before and after the reform.

<sup>36</sup> Online app. table 8 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.

higher job mobility, higher earnings, and declines in their rates of leaving the UAE, the firm-level results demonstrate that firms respond to the reform 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.

### VIII. 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  $\epsilon^l$ , the labor supply elasticity facing the monopsonistic firm. Because this applies only to incumbent workers, it can also be thought of as a retention elasticity.<sup>37</sup> We summarize all the relevant parameters and standard errors in table 7.<sup>38</sup>

In our model, the function  $w^l(l_i^l/l_i^{l-1})$  and the corresponding elasticity  $\epsilon^l(l_i^l/l_i^{l-1})$  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 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  $\epsilon^l$  is recovered from the change in  $w^l$  relative to the change in  $l_i^l$ , holding  $l_i^{l-1}$  fixed. We can estimate this simply as

$$\epsilon^l = \frac{\Delta s(w^l)/s(w_{pre}^l)}{\Delta \log w^l}.$$

From the worker-level estimates, we have that the increase in the wage for a worker experiencing a contract expiration is  $\Delta \log w^l = 0.11$ .<sup>39</sup> The total percentage increase in the probability of staying with the firm,  $\Delta s(w^l)/s(w_{pre}^l)$ , is 0.12.<sup>40</sup> This yields a labor supply elasticity for incum-

<sup>37</sup> Given the low baseline level of quits, we report retention rather than the quit elasticities found in other papers (Manning 2011).

<sup>38</sup> For parameter estimates that do not come directly out of a regression, we calculate the standard errors via the delta method.

<sup>39</sup> Specifically, this estimate is from the first row and column of panel A of table 2.

<sup>40</sup> To get this, we multiply the coefficient estimate from the first row and column of panel B of table 2, 3.8 percent, by 3 (the number of months after expiration) and divide by 95, which is the average rate of staying in the prereform period.

TABLE 7  
LABOR MARKET POWER PARAMETERS

Parameter	Estimate	Description
$\Delta \log(w)$	.113 (.009)	% change in log incumbent earnings
$\Delta s$	3.832 (.304)	% change in remaining with a firm
$s^{\text{pre}}$	96.855 (.013) <sup>+</sup>	Probability of remaining with a firm prereform
$\epsilon_{\text{pre}}^I$	1.052 (.120) <sup>+</sup>	Prereform labor supply elasticity for incumbents
$\text{share}_{\text{pre}}^I = \epsilon_{\text{pre}}^I / (\epsilon_{\text{pre}}^I + 1)$	.513 (.029) <sup>+</sup>	Prereform share of marginal product paid to incumbents
$\epsilon_{\text{post}}^I = n \times \epsilon_{\text{pre}}^I$	2.549 (.292) <sup>+</sup>	Postreform labor supply elasticity for incumbents
$\text{share}_{\text{post}}^I = \epsilon_{\text{post}}^I / (\epsilon_{\text{post}}^I + 1)$	.718 (.023) <sup>+</sup>	Postreform share of marginal product paid to incumbents
$\Delta \log(w^R)$	-.034 (.019)	% change in log earnings of new recruits
$\Delta \log(l^R)$	-.035 (.021)	% change in hiring of new recruits
$\epsilon^R = \Delta \log l^R / \Delta \log w^R$	1.033	Labor supply elasticity for new recruits
$\text{share}^R = \epsilon^R / (\epsilon^R + 1)$	.508	Share of marginal product paid to new recruits

NOTE.—Standard errors are in parentheses. Those denoted by a + are calculated via the delta method, whereas all other standard errors are taken directly from regression tables. Confidence intervals are unbounded for  $\epsilon^R$  and  $\text{share}^R$ , and so standard errors are omitted.

bent workers of 1.05. Using the Lerner condition, we can recover the share of marginal product paid to workers as  $\text{share}_{\text{pre}}^I = 1.05 / (1.05 + 1)$ , which implies that workers were paid 51 percent of their marginal product prior to the reform. Both the labor supply elasticity and estimate of the share they are paid of their marginal product are significant at the 1 percent level.

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 to the entire labor market) from  $\epsilon^I$  to  $N\epsilon^I$ . While this is special to the Cournot model, it allows us to estimate the postreform incumbent labor supply elasticity facing the firm and calculate how much the reform itself changed competition.<sup>41</sup> 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.<sup>42</sup> Adding this to the prereform level of 1 implies taking 2.4

<sup>41</sup> Other models of competition would result in different changes to the postreform labor supply elasticity facing the firm, as would heterogeneity in worker utility across firms.

<sup>42</sup> This is a summary statistic calculated from the data.

as a measure of the postreform effective labor market size,  $N$ . This suggests that the effective labor supply elasticity facing the firm after the reform is  $N\epsilon^l = 2.4 \times 1.1 = 2.5$ . In other words, incumbent workers were paid 71 percent of their marginal product after the reform. Both of these estimates are significant at the 1 percent level.

Also of interest is  $\epsilon^R$ , which captures the degree of monopsony power for new recruits. We can recover  $\epsilon^R$  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.<sup>43</sup> Using contract expirations as exogenous variation in the quantity of new entrants, we can estimate a reduced form  $\epsilon^R$ , which is the labor supply elasticity for new recruits:

$$\Delta \log(w^R) = \frac{1}{\epsilon^R} \Delta \log l^R. \quad (10)$$

Then we can use the empirical equation  $\Delta \log w^R = \hat{\gamma} \Delta \log l^R$  as a first-stage equation, because the only way  $l^R$  affects  $w^R$  is via  $l^R$ . From our firm-level estimates we have that  $\Delta \log w^R / \Delta \log l^R = -0.034$  and  $\Delta \log l^R / \Delta \log l^l = -0.035$ .<sup>44</sup> The elasticity can then be expressed as the ratio

$$\epsilon^R = \frac{\Delta \log w^R}{\Delta \log w^l} = \frac{\Delta \log l^R / \Delta \log l^l}{\Delta \log w^R / \Delta \log l^l} = \frac{0.035}{0.034} = 1.03.$$

Because of problems that arise in estimating a ratio with a denominator that is not statistically far from zero, we do not report standard errors. Quantitatively, however, this is not too far away from the prereform estimate of  $\epsilon^l$  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. Calculating the implied share<sup>R</sup> in table 7 yields that wages for new recruits are roughly one-half of marginal product.

<sup>43</sup> In the online appendix, with forward-looking workers, we argue that the reform is not excludable to the wage equation, as it affects the supply of new recruits as well as demand. However, we believe that forward-looking worker behavior is not extremely important in our context. Indeed, 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.

<sup>44</sup> The estimate of  $-0.034$  is from col. 1 of panel B in table 6, and the estimate of  $-0.035$  is from col. 1 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 one, a 100 percent increase in expirations corresponds to one extra expiration on average.

While our context is quite different from other labor markets, our set of estimates is within or close to the range of elasticities of 1–1.9 reported by Falch (2010) for Norwegian teachers and 2 reported by Dal Bó, Finan, and Rossi (2013), although much larger than the 0.1 elasticity reported for Veterans Administration hospital nurses by Staiger et al. (2010). These results are also different from the effectively infinite elasticities for low-wage nurse's 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.<sup>45</sup> 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.

## IX. Alternative Explanations

### A. 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 (col. 3 of App. table A1).<sup>46</sup> The quantile estimates discussed in online appendix section 3.2 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 results 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 online appendix table 1. Given that the stayers represent 97 percent 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 et al. (2006). This

<sup>45</sup> The postreform estimates are similar to the 70–85 percent of marginal product found by Isen (2013) in the United States and more than the 48 percent of marginal product recovered for US slaves in 1860 by Vedder (1975).

<sup>46</sup> Online app. sec. 3.1 discusses heterogeneity in the results by a variety of worker characteristics.

also highlights the fact that the earnings results are not driven only 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.

### *B. 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 the highly educated in the reform, they do not affect the vast majority of workers in our sample.<sup>47</sup>

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 in which the reforms on contract length and on the NOC requirements were implemented.<sup>48</sup> 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 that the change in contract length would bias our results downward.

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 toward incumbent workers and increase competition for workers in the UAE. This explanation is consistent with the results for the hiring and earnings of new entrants and for the results on the earnings and country exits of incumbent migrants in the UAE.<sup>49</sup> However, the magnitudes of the estimates indicate that this is unlikely to be

<sup>47</sup> Column 6 of each panel of table A1 demonstrates that the results remain similar for workers younger than 35 and are not driven exclusively by old workers.

<sup>48</sup> Note that the full MOL contracts data include 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.

<sup>49</sup> This story in itself would not lead to an increase in employer transitions among incumbent workers.

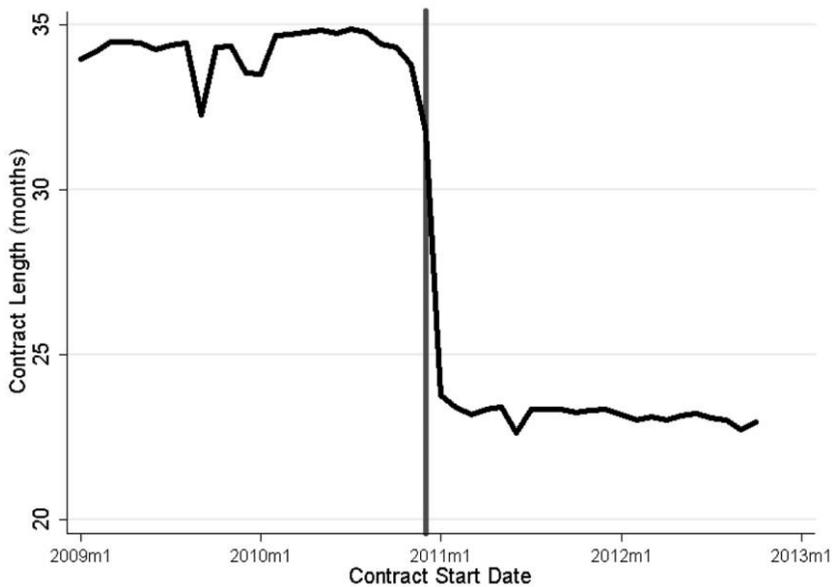


FIG. 8.—Average contract length by contract start date. The figure shows the average contract length (in months) by the start date of the contract. The vertical line indicates the announcement of the NOC reform. The sample is the MOL data. Color version available as an online enhancement.

to 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 percent more every year far exceed the reported estimate of recruitment costs, particularly when the high rate of renewal and low age of workers are considered.<sup>50</sup>

### C. 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 VI.E, 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

<sup>50</sup> Firms report paying recruitment firms about 15–20 percent of a worker's earnings for 1 year (Gutcher 2013).

quite harsh (50,000 dirham for employers hiring workers without a valid visa).<sup>51</sup>

One possible concern is that the results on earnings are not reflective of the total compensation of workers or job quality.<sup>52</sup> This is unlikely to be an important concern as the observed reduction in exits suggests that the observed increases in earnings are not completely offset by reductions in other sources of compensation or declines in job quality. However, the data also allow us to consider this more carefully. We examine whether in-kind transfers, which usually include employer-provided housing and food, may explain the results. We observe the value of in-kind benefits associated with contracts in the MOL data. We find that the ratio of in-kind benefits to contract earnings does not change after the reform. This suggests that the increase in earnings associated with the reform is not completely offset by a decrease in the value of in-kind transfers.

Another concern is that changes in recruitment costs occurring at the same time as the change in mobility requirements increased demand for incumbent migrants already in the UAE. While there was a reduction in visa fees (down to 300 dirham), it was only for workers with university degrees (who are very few in our sample) and for firms with greater than 15 percent emirati workers and greater than 20 percent skilled workers. This is certainly not enough to account for a 10 percent increase in earnings across our sample of largely low-wage workers. We consider whether a large shift in nonvisa 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.

<sup>51</sup> An amnesty in the UAE in 2007 had up to 342,000 workers take advantage of it. Baker (2015) writes that almost 100 percent of illegal immigrants applied for a similar amnesty in the United States. If almost all of the workers in the UAE took advantage of the amnesty, this would imply that roughly 5 percent of the workforce in the UAE is illegal. Given that 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 fact that “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>).

<sup>52</sup> This alternative interpretation could potentially affect only the results for earnings and not the findings on transitions and exits.

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 incumbents 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 A1 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 have 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.

## X. 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 51 percent to 72 percent.

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 and 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 percent, 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 criterion 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 (forthcoming) 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) criterion of "just deserts" in which workers should be paid their marginal products.

These results offer insight into many other types of labor markets in which workers sign contracts that tie them to employers and are relevant for several current legal and policy debates. For example, immigration reformers in the United States 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 in which large firms, including Apple and Google, have signed agreements not to poach each other's employees. Noncompete clauses in US labor contracts may cover as many as 12.3 percent of the workforce, with up to 30 percent 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 United States that do offer migrants a path toward citizenship may face a more complex set of political considerations.

Appendix

TABLE A1  
HETEROGENEOUS EFFECTS ON LOG EARNINGS, EMPLOYER TRANSITIONS, AND COUNTRY EXITS

	Indian (1)	Non-Indian (2)	Educated (3)	Construction (4)	Dubai-Sharjah (5)	Age <35 (6)
A. Log Earnings						
Postreform × post-contract expire	.169*** (.014)	.058*** (.012)	.062*** (.018)	.152*** (.014)	.074*** (.011)	.109*** (.013)
Postreform × period contract expire	.031*** (.008)	-.001 (.007)	-.004 (.010)	.031*** (.008)	-.001 (.006)	.003 (.007)
Observations	242,894	281,171	178,704	223,739	327,747	272,723
Number of clusters	51,730	58,104	38,777	45,304	67,960	57,862
R <sup>2</sup>	.023	.017	.015	.028	.011	.021
B. Exits from the UAE						
Postreform × post-contract expire	-4.476*** (.426)	-3.862*** (.351)	-3.662*** (.578)	-5.311*** (.397)	-4.951*** (.359)	-5.895*** (.435)
Postreform × period contract expire	-2.535*** (.282)	-.919*** (.243)	-1.119*** (.353)	-2.315*** (.271)	-2.181*** (.236)	-1.844*** (.282)
Observations	254,513	290,595	185,956	233,322	342,021	283,944
Number of clusters	51,867	58,266	38,904	45,406	68,143	58,022
R <sup>2</sup>	.102	.067	.082	.094	.093	.088

TABLE A1 (Continued)

	Indian (1)	Non-Indian (2)	Educated (3)	Construction (4)	Dubai-Sharjah (5)	Age <35 (6)
C. Employment Transitions						
Postreform $\times$ post-contract expire	.931*** (.134)	.370** (.175)	1.102*** (.252)	.224** (.111)	1.174*** (.106)	.845*** (.159)
Postreform $\times$ period contract expire	.738*** (.095)	.242** (.112)	.631*** (.162)	.444*** (.083)	.710*** (.076)	.712*** (.106)
Observations	236,339	272,882	173,046	219,192	321,830	263,445
Number of clusters	50,916	57,304	38,230	45,035	67,582	56,823
$R^2$	.007	.006	.012	.003	.008	.007

NOTE.—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 are in parentheses. The sample includes all months from January 2009 to October 2012. Postreform  $\times$  post-contract expire corresponds to  $\sum_{h=1}^9 \gamma_h^{Post2011} - \gamma_k^{Pre2011}$ , and postreform  $\times$  period contract expire corresponds to  $\gamma_0^{Post2011} - \gamma_0^{Pre2011}$ . Postreform 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 in which the individual's contract expires.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

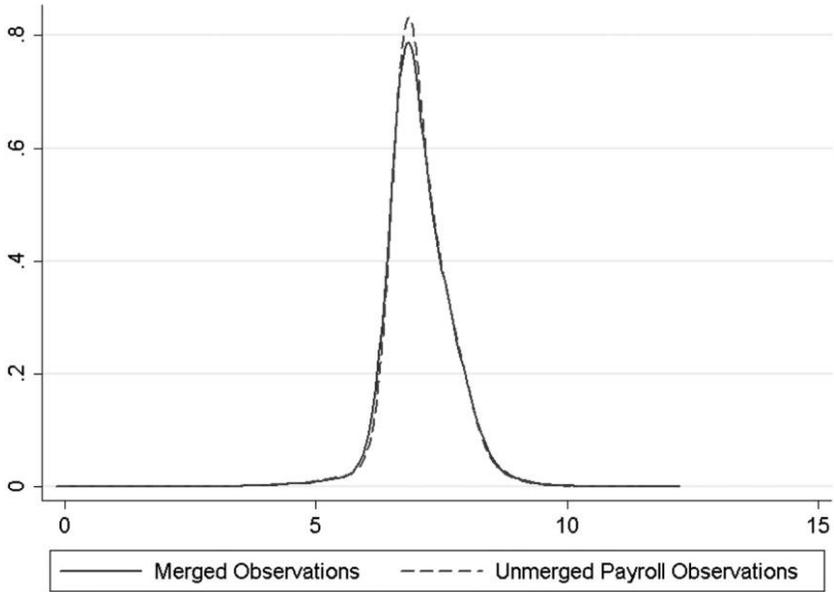


FIG. A1.—Density of matched payroll-MOL log earnings and unmatched payroll log earnings. The figure shows the kernel density of the distribution of log earnings in the payroll data by whether the observation matches into the MOL data or not. Color version available as an online enhancement.

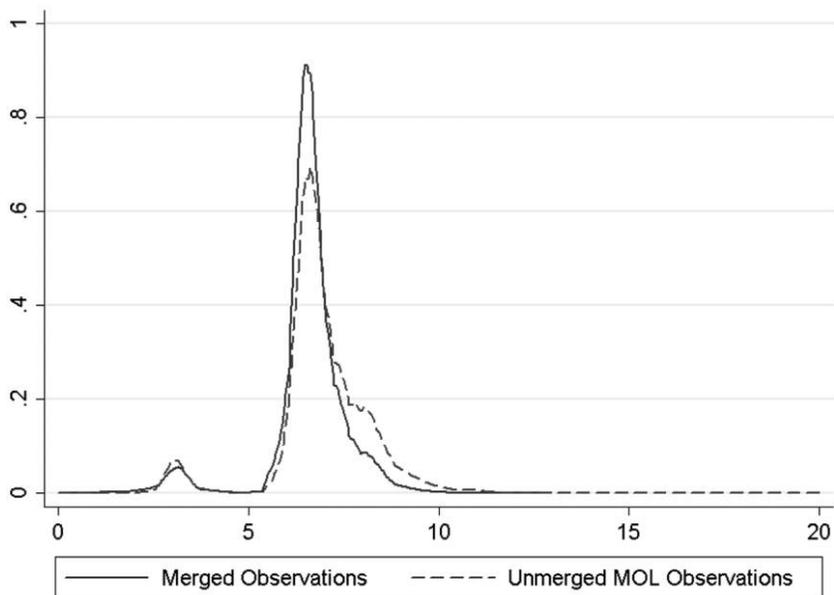


FIG. A2.—Density of matched payroll-MOL log contract salary and unmatched MOL log contract salary. The figure shows the kernel density of the distribution of log contract earnings in the MOL data by whether the observation matches into the payroll data or not. Color version available as an online enhancement.

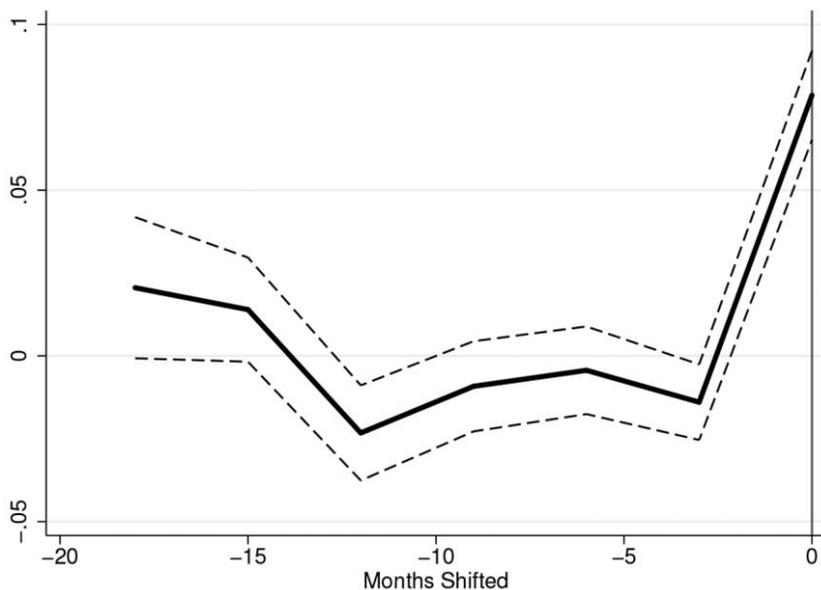


FIG. A3.—Effects of time-shifted placebos on log earnings. The figure displays the average 3-month post-contract expiration effect of the reform on log earnings in which the timing of the contract expiration is shifted by the months given on the x-axis. The regressions include individual fixed effects, year-month fixed effects, and a constant term. The omitted category is the 3 months prior to contract expiration. The dotted lines give the 95 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

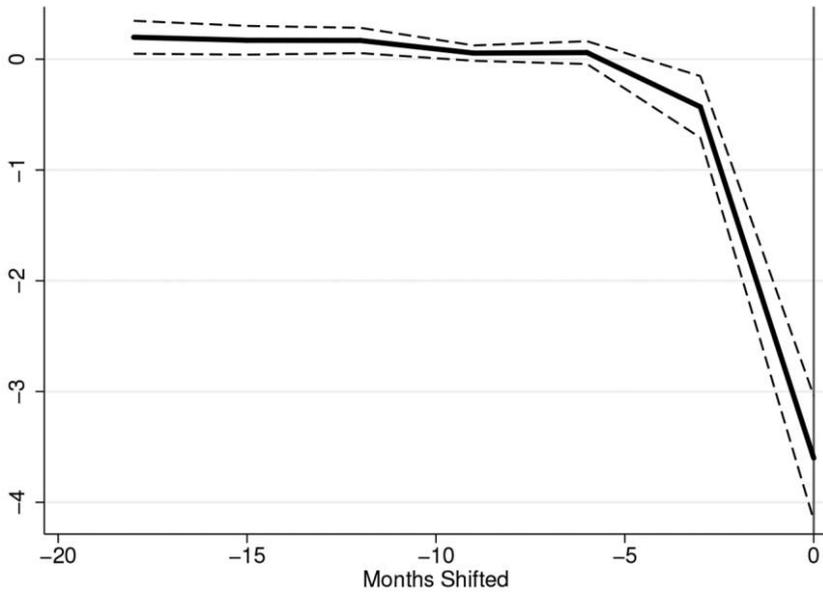


FIG. A4.—Effects of time-shifted placebos on exits from the UAE. The figure displays the average 3-month post-contract expiration effect of the reform on country exits in which the timing of the contract expiration is shifted by the months given on the  $x$ -axis. The regressions include individual fixed effects, year-month fixed effects, and a constant term. The omitted category is the 3 months prior to contract expiration. The dotted lines give the 95 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

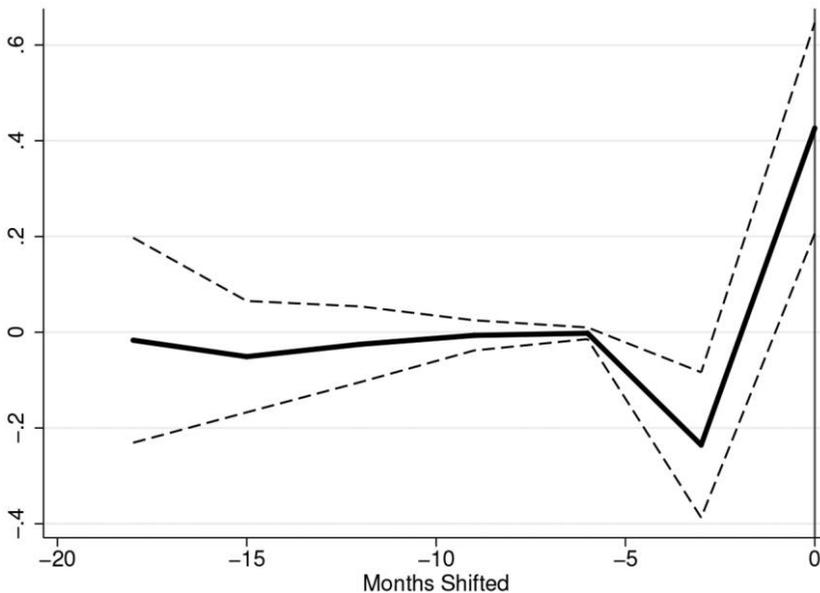


FIG. A5.—Effects of time-shifted placebos on employer changes. The figure displays the average 3-month post-contract expiration effect of the reform on employer changes in which the timing of the contract expiration is shifted by the months given on the  $x$ -axis. The regressions include individual fixed effects, year-month fixed effects, and a constant term. The omitted category is the 3 months prior to contract expiration. The dotted lines give the 95 percent confidence interval. The vertical line indicates the month in which the worker's contract expires. Color version available as an online enhancement.

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