The Dynamic Impact of Immigration on Natives' Labor Market Outcomes: Evidence from Israel*

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Abstract

This paper studies the dynamic impact of highly skilled immigrants from the Former Soviet Union to Israel on natives' labor market outcomes. Specifically, we attempt to distinguish between the short-run and long-run effects of immigrants on natives' wages and employment. The transition of immigrants into a new labor market is a gradual process: the dynamics of this process come from immigrants' occupational mobility and from adjustments by local factors of production. Natives may therefore face changing labor market conditions, even years after the arrival of the immigrants.

If immigrants are relatively good substitutes for native workers, we expect that the impact of immigration will be largest immediately upon the immigrants' arrival, and may become smaller as the labor market adjusts to the supply shock. Conversely, if immigrants upon arrival are poor substitutes for natives due to of their lack of local human capital, the initial effect of immigration is small, and increases over time as immigrants acquire local labor market skills and compete with native workers. We empirically examine these alternative hypotheses using data from Israel's Labor Force and Income Surveys from 1989 to 1999.

We find that wages of both men and women are negatively correlated with the fraction of immigrants with little local experience in a given labor market segment. A 10 percent increase in the share of immigrants lowers natives' wages in the short run by 1 to 3 percent, but this effect dissolves after 4 to 7 years. This result is robust to a variety of different segmentations of the labor market, to the inclusion of cohort effects, and to different dynamic structures in the residual term of the wage equation. On the other hand, we do not find any effect of immigration on employment, neither in the short nor in the medium run.

Keywords: Immigration, wages, employment, labor demand.
JEL Codes: J31, J61, J21, J23, F22.

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1. Introduction

As immigration continues to rise throughout the Western world, the question of the economic impact of immigration on the host country labor market is moving to the center of the public debate. The concern that immigrants may compete with low skilled workers and adversely affect their employment and wages is among the factors that drive negative attitudes toward immigrants in Europe and the USA.[1] Despite this widespread sentiment, the economic literature has failed to find conclusive evidence for such an adverse effect of immigration on natives' labor market outcomes. In this paper, we try to shed additional light on this issue by introducing a dynamic dimension to the measurement of the impact of immigration on natives' outcomes.

During the 1990s about 1 million Jews migrated from the Former Soviet-Union (FSU) to Israel. Most of the immigrants had college education and worked in the FSU in high skill occupation. Using repeated cross section national data on these immigrants and natives, we attempt to distinguish between the short and medium run effects of immigration on the labor market. This is in contrast to most previous studies on the impact of immigration, which implicitly assumed that the effect of immigration is homogeneous over time (regardless of whether the time frame of analysis is two or ten years after the arrival of immigrants). This distinction is of a great importance since, in the context of immigration, short and medium-run effects may differ substantially because of the parallel processes of adjustment of the capital stock and immigrants' investment in local human capital.

To illustrate our distinction, consider the following two scenarios. In the first scenario immigrants are relatively close substitutes to natives upon arrival and therefore there is an immediate negative impact on natives' wages and employment, as the stock of capital and other factors of production are fixed in the short run. However, as time goes by, capital and labor adjust, so that the medium and long run response will be smaller, and potentially even zero. Alternatively, upon arrival,

[1] Bauer, Lofstrom and Zimmermann (2000).

immigrants are poor substitutes for native workers, since their imported human capital is not transferable to the host economy. Therefore, the immediate impact of immigration on natives' labor market outcomes is close to zero; nevertheless, as immigrants acquire local labor market skills, they compete with native workers, so that the medium and long run effects on natives' outcomes might be substantial.

To tease out these alternative hypotheses, we set up a simple theoretical model and an econometric framework that allows immigrants with different levels of local labor market experience to have different effects on natives' labor market outcomes. We implement our econometric approach using micro data from Israel's Labor Force and Income Surveys from 1989 to 1999. Specifically, we estimate the impact of the percentage of immigrants with different tenure in Israel in a well-defined labor market segment on natives' wages and employment. The analysis is feasible given the availability of detailed information on dates of immigration in the Israeli data, and the sheer size of the immigration wave, that allows us to observe a sufficiently large amount of immigrants with different amounts of tenure in each labor market segment. We consider different segmentations of the labor market such that moving across labor market segments always involves substantial adjustment costs for natives (education, retraining, moving, commuting, etc.).

Recognizing that immigrants do not allocate themselves randomly across different labor market segments, we use a number of different specifications to control for the potential correlation between immigrants' concentration and unobserved labor market conditions. Specifically, we experiment with different dynamic structures of the error term, including segment-specific fixed effects, a segment-specific linear time trend, and higher-level fixed effects interacted with a full set of time dummies. Thus, identification of the key parameter in the model comes from deviations in wages, employment, and the proportion of immigrants from segment specific means, segment specific trends, or deviations from period-specific means in broad groupings of segments.

Our results indicate that immigration has an adverse short run impact on the wages of native men and women that are close substitutes to the immigrants, though this effect dissolves in the medium and long run. Importantly, the estimated impact of immigration is small and insignificant if one fails to take into account its dynamic nature. Our main result is robust to a variety of different segmentations of the labor market and to alternative structures of the error term. Our preferred estimates suggest that a 10 percent increase in the share of immigrants lowers natives' wages in the short run by 1 to 3 percent. On the other hand, we do not find any effect of immigration on employment, neither in the short nor in the medium run.

Finally, we find that the short-run effect of immigration on native wages is concentrated primarily in blue-collar occupations, suggesting that either in the short run it is easier for immigrants to compete with low-skill natives, or that there may be more scope for complementarities between natives and immigrants within high-skill occupations.

Our paper is related to the large literature on immigrants' impact on natives' outcomes. Simple supply-demand models of the labor market predict that a large migration wave would have an adverse effect on employment rates and wages of native workers. However, much of the evidence from Israel and elsewhere concludes that immigration has had little or no adverse impact on host country wages and employment, independent of the methodological approach that was implemented. In an influential study (to which we will return later), Friedberg (2001) argues that the concentration of FSU immigrants in two-digit occupation cells had no adverse impact on native Israeli wages, once the selectivity of immigrants across occupations is accounted for.

Several papers have used the spatial correlation approach, which exploits geographic variation in immigrant rates over time, and have generally found at most small negative impacts of immigration on native wages and employment.[2] Others have used natural experiments of immigration episodes generated by political factors in the origin country (Card, 1990; Hunt, 1992; Carrington and de Lima, 1996), and also found surprisingly little effects of migration. Our work is more closely related to the analysis by LaLonde and Topel (1991), who exploit variation in the timing of immigration across localities to analyze the dynamic substitution patterns between new and older cohorts of immigrants. They find that older immigrants' wages are negatively affected by immigration, whereas natives' wages are not.

More recent studies have moved away from the spatial correlation approach, which suffers from the problem that the increase in labor supply due to immigration can be diffused across the economy by intercity trade, movements of capital or by outflows of natives (Borjas, Freeman and Katz, 1996). .These studies have tended to find slightly larger adverse impacts of immigration. Card (2001) finds that occupation-specific wages and employment rates are systematically lower in cities with higher relative supplies of workers in a given occupation. Similarly, Borjas (2003) uses only variation in the human capital mix (determined by schooling and experience) of immigrants to study the effect of immigration on different groups of natives; he finds that, within groups, immigrants did have an adverse effect on wages and employment opportunities of natives.

Our results support these recent findings and suggest that the effects of immigration in the short run may be larger than what previously believed, while in the long-run, the effect is indeed negligible. The lack of distinction between the short and the medium run may lead to the mixed results reported in the literature.

The remainder of the paper is organized as follows: the next section presents a brief theoretical framework that illustrates the various forces that affect the short and medium run elasticities of factor prices with respect to immigration. Section 3 gives a

[2] For example, see Altonji and Card (1991) and Goldin (1994) for the US; Pischke and Velling (1997) for Germany; and Dustmann et al. (2005) for the UK.

brief account of the absorption of FSU immigrants in the Israeli labor market, and presents some preliminary evidence on the short and long run responses of wages and employment of natives. In Section 4 we describe and motivate the different labor market segmentations, and present our methodology for estimating the dynamic impact of immigration on native wages and employment. In this context, we also discuss the various structures of error terms that enable to identify the parameters of interest. In section 5 we present the basic estimation results, and perform a series of robustness tests. Section 6 concludes.

2. Theoretical Framework

To illustrate the short and long-run effects of immigration, we present a simple model that builds on Borjas (1999). Consider an economy that produces aggregate output using capital (K) and J different types of workers: [3]

$$Y = f\left(K, L_1, L_2, ..., L_J\right).$$

The production function f is linearly homogenous and satisfies the usual assumptions, $f_i > 0$, $f_{ii} < 0$. Each labor input L_j is a linearly homogeneous aggregate of native (N_j) and immigrant (M_j) workers:

$$L_j = g^j\left(N_j, M_j\right).$$

Importantly, as in Ottaviano and Peri (2005 and 2006), we do not necessarily assume that natives and immigrants are perfectly substitutable within a skill group. We assume that the labor supply of natives in each skill group is perfectly inelastic (i.e., there is also no movement of natives across skill groups) while the supply of capital can be written as:

$$K = a + br, \qquad b \geq 0,$$

[3] The assumption that the economy produces a single aggregate good implicitly assumes that immigration does not induce reallocation of production across sectors. The available empirical evidence seems to support this view, both for Israel (Gandal, Hanson and Slaughter, 2004) and for the United States (Lewis, 2003).

where r is the rental rate of capital. Setting $r = f_k$ and totally differentiating yields:

$$dK = b\left\{ f_{KK} dK + \sum_j f_{Kj}\left(g_M^j dM_j + g_N^j dN_j\right)\right\}.$$

Setting $dN_j = 0,$ and rearranging, we have:

$$dK = \frac{b}{1 - bf_{KK}}\left\{\sum_j f_{Kj} g_M^j dM_j\right\}. \tag{1}$$

The wage of native workers in skill group *j* is equal to their marginal productivity:

$w_{N_j} = \frac{\partial f}{\partial N_j} = f_j g_N^j$. Differentiating this equation gives us:

$$dw_{N_j} = g_N^j\left\{ f_{jK} dK + \sum_{j'} f_{jj'} g_M^{j'} dM_{j'}\right\} + f_j\left(g_{NM}^j dM_j\right).$$

Therefore the elasticity of native wages in skill group *j* with respect to the number of immigrants in skill group *j* can be shown to be:

$$\frac{d\ln w_{N_j}}{d\ln M_j} = \frac{dw_{N_j}}{dM_j}\frac{M_j}{w_{N_j}} = \alpha_{M_j} c_{jj}^f - \alpha_{M_j}\left(\frac{bf_{KK}}{bf_{KK} - 1}\right)\frac{c_{Kj}^{f\ 2}}{c_{KK}^f} + \frac{\alpha_{M_j}}{\alpha_{M_j} + \alpha_{N_j}} c_{NM}^{g_j}\ , \tag{2}$$

where $\alpha_{M_j} = \frac{w_{M_j} M_j}{f}$ is the share of M_j in total output, $c_{ij}^f = \frac{f_{ij} f}{f_i f_j}$ are the elasticities of complementarity (see Hamermesh, 1992) between factors *i* and *j* according to production function *f*, and $c_{NM}^{g_j} = \frac{g_{NM}^j g^j}{g_N^j g_M^j}$ is the elasticity of complementarity between natives and immigrants according to function g^j ($c_{NM}^{g_j} \geq 0$, since g^j is a function of only two inputs).[4]

[4] In the empirical implementation, we only look at the effect of a supply shock in a particular skill group on wags in that skill group. However, to proxy for general equilibrium effects, we always control for time dummies and for an index of labor demand for that skill group. This simple framework could also be used to analyze the general equilibrium effects of a migration shock in a particular skill group on the wages of workers in different skill groups. However, identification of these effects would be achieved only from the time series variation in wages, or from imposing more structure on the nature of the production function.

Analyzing expression (2), we note that the first term is negative; the second term is zero if $b=0$ (the supply of capital is perfectly inelastic), and positive otherwise; the third term is zero if N_j and M_j are perfect substitutes, and positive otherwise (e.g., if g^j is a CES aggregate, $g^j(N_j, M_j) = (\beta_j N_j^{\rho_j} + (1-\beta_j) M_j^{\rho_j})^{1/\rho_j}$, then $c_{NM}^{g_j} = 1-\rho_j$; Hamermesh, 1992).

In other words, equation (2) yields the straightforward prediction that native wages in skill group *j* will fall more if the supply of capital is inelastic, and if natives and immigrants in the skill group are close substitutes. We expect the medium run elasticity of capital to be higher than the short-run elasticity: hence, native wages are expected to fall more in the short-run than in the medium run. However, the extent to which wages will fall in the short run depends also on the degree of substitutability between immigrants and natives. If newly arrived immigrants and natives within a skill group have complementary skills, but progressively become closer substitutes as immigrants acquire local human capital, we would observe a small (or even positive) effect on native wages in the short run, and a larger negative effect in the medium run.

This degree of substitutability between immigrants and natives is particularly important in the Israeli case, because most of the FSU immigrants were college educated and had worked in the FSU in white collar occupations. An extensive literature has analyzed the integration of FSU immigrants in the Israeli labor market. Two main findings emerge from this literature: first, immigrants experienced substantial occupational downgrading and consequently the return in the Israeli labor market to their imported education was quite low (Eckstein and Weiss, 2004); second, immigrants continuously invested in local skills in the form of vocational training, experience and language (Cohen-Goldner and Eckstein, 2008, 2010). The implications of these studies are that native Israelis may face changing labor market conditions even years after the arrival of the immigrants.

This simple model yields two auxiliary predictions. First, it is likely that the short-run impact of immigration on wages will be relative large when skill groups are defined in such a way that the degree of substitutability between immigrants and natives is high (e.g., narrowly defined occupation groups), and relatively small when the degree of substitutability is low (e.g., skill groups defined by age and education cells – since imported human capital is not immediately transferable to the host economy and immigrants with a given level of education and labor market experience are not employed in the same jobs as equivalent native workers). Secondly, if highly-skilled native workers are less easily substitutable by immigrants, then we should observe a larger effect of immigration on the wages of low-skilled workers rather than highly-skilled workers. In the empirical section below we will test these predictions.

3. Background

Migration to Israel

Starting in October 1989, with the collapse of the former Soviet Union (FSU) and the change in emigration restrictions on Russian Jewish citizens, Israel experienced one of its largest immigration inflows, which continued throughout all of the 1990s. From late 1989 until 2001, over a million of immigrants from the FSU arrived in Israel, increasing its population and labor force by extraordinary rates. At the peak of this wave during 1990 and 1991, over 330 thousand FSU Jews immigrated to Israel, increasing Israel's potential labor force by 8 percent and its population by 15 percent.

The most notable characteristic of the FSU immigrants is their high level of education. Over 69 percent of all FSU male and female immigrants had at least some college education and over 40 percent were college graduates. The share of college-educated natives during the same period, on the other hand, is only about 35 percent, and only 22 percent of natives are college graduates.[5] Moreover, immigrants who arrived in the early wave were, on average, more educated than those who arrived in the later wave.[6]

In Table 1 we present the one-digit occupational distribution of natives and immigrants in two sub-periods, 1989-1993 and 1994-1999. The table shows that male immigrants are more concentrated than natives at both ends of the occupational ladder, while female immigrants are especially concentrated at the bottom. The distribution of natives has almost not changed between the two periods. At a first glance, there is no evidence that immigrants substantially affected the occupational distribution of natives. This is important for our empirical analysis because it lends credibility to our assumption that natives' ability to move between segments of the labor market defined by occupation is limited.[7] Therefore, our results are not likely to be contaminated by native flows across skill groups.

As for the distribution of immigrants, it is worthwhile to note that in the early period (1989-1993) they were more likely to be employed in unskilled occupations, probably reflecting that (a) the size of the initial wave was so large that for many immigrants it was difficult to find a job suitable to their imported high skills; and (b) Israeli employers were uncertain about the quality of imported human capital (i.e., education), and it took them some time to learn it.

This last observation is reinforced by Table 2, which presents the occupational distribution of immigrants and natives, by schooling and time since migration. The table shows that the occupational distribution of recently arrived immigrants resembles the occupational distribution of relatively uneducated natives, regardless of

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[5] Throughout the paper, we use the term "natives" to describe the population resident in Israel prior to January 1989 and "immigrants" to describe FSU immigrants who arrived after 1989. The native population includes both Israeli-born and foreign-born individuals. The share of foreign-born among natives is more than 40 percent. Since immigration to Israel was at its lowest during the 1980s, more than 90 percent of these foreign-born individuals have been in Israel for more than 10 years. In all of our analysis, we always control for foreign-born status and years since immigration.

[6] See Cohen-Goldner and Paserman (2006).

[7] In previous work (Cohen-Goldner and Paserman, 2006), we did not find any evidence that higher immigrant concentration in a given occupation affected the occupational choices of young native workers.

the actual level of education attained by the immigrants. For example, 41 percent of recently arrived male immigrants with some college were employed as skilled industry workers, and 11 percent were employed as unskilled workers; the corresponding numbers for native males with some college are 11 percent and 0.5 percent.

As immigrants spend more time in Israel, their occupational distribution begins to match their educational attainment, though it does not converge fully to that of natives.[8] The table highlights the important distinction between the true level of imported education and its effective value in the Israeli labor market. It also illustrates that recent immigrant and native workers with the same levels of formal schooling are not necessarily close substitutes. This should be kept in mind when interpreting the empirical results based on education-based segmentations of the labor market.

Natives' Labor Market Outcomes

We now turn to the analysis of Israeli natives' labor market outcomes during the 1990s. Figure 1 shows the evolution of native male and female real hourly wages between 1987 and 1999, where the scale is 100 in 1987 for each gender. We see that for both native males and females real wages fell substantially at the time the migration wave began. Female real wages returned to their 1989 level only in 1994, and after dipping in 1995, they continued to grow more or less steadily throughout the second part of the decade. On the other hand, male wages were slower to recover, and only in 1996 did they return to their 1989 level for more than two consecutive years.

In Figure 2 we present the evolution of native male and female employment rates (again the scale is 100 for each gender in 1987). Here it seems more difficult to disentangle any potential effect due to immigration from cyclical and secular trends. The employment rate among males was relatively stable throughout the first half of the decade, apart from cyclical movements, and has been falling steadily since 1995.

[8] This finding is consistent with the results of Weiss, Sauer and Gotlibowski (2001), who found that immigrants' wages do not converge fully to those of natives in the long run.

On the other hand, the employment rate among females is characterized by a secular upward trend. These time-series give some preliminary evidence that wages did initially react to the migration wave, and recovered later in the decade, while the picture for employment is less clear.

We now turn to analyze whether there is a cross-sectional correlation between the concentration of immigrants in a sector and the change in wages or employment in the short and medium run. For each two-digit occupation cell, we calculate the average log hourly wage of natives in every year, and the ratio of immigrants who arrived between 1989-1991 in the cell to the size of the cell in 1989. Holding constant the size of the cell in 1989 ensures that we pick up only the variation in the number of immigrants in a cell (the numerator), not contaminated by native flows across labor market segments.

Figures 3 and 4 plot the change in log hourly wages against the fraction of 1989-1991 immigrants in two-digit occupation cells for males and females, respectively. The left-hand panel in the figures presents changes between 1989 and 1994 (the short-run change), while the right-hand panel presents changes between 1989 and 1999 (the long-run change). The overlaid regression line is obtained by weighted least squares, where each cell’s weight is its average size. Note that the regression coefficient represents the percentage change in wages associated with a 100 percent change (i.e., a doubling) in the fraction of immigrants, and can therefore be interpreted as elasticity.

For both males and females, we find that the short run change in log hourly wages exhibits a strongly negative and statistically significant correlation with immigrant penetration at the two-digit occupation level. The regression coefficient places the unadjusted short-run factor price elasticity at around -0.55, a substantially larger number than what had been previously found in the literature. On the other hand, the medium run elasticity is between 0.18 and -0.44, and insignificantly different from zero for both males and females.[9]

Figures 5 and 6 plot the change in employment rates, in the short run and the medium run, against the fraction of 1989-1991 immigrants in two-digit occupation cells. For males, there seems to be a very tenuous relationship between the two variables, independently of the time horizon. For females, the pattern is more similar to that found for wages: employment is negatively correlated with immigrant concentration in the short run, but the medium run correlation is essentially zero.

While these are very raw estimates, they illustrate clearly the importance of distinguishing between the short and medium run effects of immigration, and they provide some preliminary support for the hypothesis that any adverse effects of immigration are more likely to manifest themselves in the short run, before the labor market has had time to adjust.

In the next sections, we investigate further whether the contrast between the short and medium run effects of immigration is robust to the use of individual level data, to the inclusion of additional controls for macroeconomic conditions and individual characteristics, to different segmentations of the labor market, and to alternative structures of the error term.

4. Methodology

We begin by specifying a conventional model for the impact of immigration on native labor market outcomes. Our estimating equation is

$$y_{ijt} = \beta_0 + \beta_1 IMM_{jt} + \beta_2 Z_{jt} + \beta_3 X_{ijt} + \alpha_j + \delta_t + \eta_{jt} + u_{ijt}, \qquad (1)$$

where y_{ijt} is the outcome variable of interest for individual i in labor market segment (or "cell") j observed in calendar quarter t. In the wage regressions y_{ijt} is the log hourly wage, while in the employment regressions it is a dummy indicator for whether

[9] The actual value of the elasticity should be taken with some caution. If immigrants who arrived after 1992 tend to concentrate in the same occupations as immigrants who arrived between 1989 and 1991, and they have a short run negative impact on wages, this may lead to finding a stronger negative correlation between native wages and the fraction of 1989-1991 immigrants.

the individual is employed. IMM_{jt} is the ratio of immigrants (both men and women) in segment j at time t to the size of cell j in 1989, Z_{jt} and X_{ijt} are vectors of observable macro and individual characteristics,[10] α_j is a segment specific fixed effect, δ_t is a calendar quarter fixed effect, η_{jt} is a segment-calendar quarter specific effect, whose exact specification will be presented later, and u_{ijt} is the error term. All regressions adjust standard errors for clustering at the cell-calendar quarter level. The underlying assumption in equation (1) is that all immigrants have the same effect on the dependent variable, regardless of their time of arrival in Israel. Note that the all the time-series variation in the immigrant ratio in a given cell comes from the number of immigrants, since the denominator (the number of natives) is fixed.

Definition of the Labor Market Cells

The variable IMM_{jt} is a key variable in our analysis. Using the LFS, we calculate the share of immigrants in a given labor market cell in each calendar quarter, from the third quarter of 1989 to the fourth quarter of 1999. Following the recent criticisms of the local labor market approach (Borjas, Freeman and Katz, 1996; Borjas 2003), we take particular care to define the segments in such a way that they can be viewed as isolated markets with limited possibilities for native workers to move between them. We adopt four different segmentations of the labor market. In each of these segmentations, moving across labor market segments involves substantial adjustment costs (education, retraining, moving, commuting, etc.).

[10] The vector Z_{jt} is a set of controls for labor demand shocks for workers in segment j at time t. It includes the total number of workers in cell j at time t, and an index for labor demand for workers in the cell. See Cohen-Goldner and Paserman (2004) for details on the construction of this index. The vector X_{ijt} represents a set of individual demographic characteristics of worker i in cell j at time t, and it includes years of schooling, potential experience, and potential experience squared; a marital status dummy (1 if married, zero otherwise) and the number of children aged 0-4, 5-14, and 15-17; a dummy for whether the individual is foreign born (1 for Israeli born) and the number of years since immigration; a set of ethnic origin variables – Jews of European/American origin (Ashkenazi), Jews of Asian/African origin (Sephardi), and non-Jews; and a dummy for whether the individual is employed in the public sector. In all regressions we include a full set of calendar quarter dummies, to capture unobserved macroeconomic conditions.

As in Friedberg (2001), we start by defining a closed labor market segment as a two-digit occupation cell. We next construct cells defined by one-digit occupation interacted with district of residence. The third segmentation is based on one digit occupation interacted with one digit industry, and, following Borjas (2003), the fourth segmentation is defined by the interaction of schooling and experience. In constructing the schooling-experience cells, however, it is important that we take into account the fact that human capital acquired abroad is not immediately transferable to the host economy, especially since the education system in the FSU significantly differs from the Israeli one. In addition, as highlighted in Table 2, many of the highly educated immigrants have difficulties in quickly finding employment that is suitable to their skills. Therefore, we construct two alternative segmentations: one based on the actual level of schooling and experience, and one based on the *effective* schooling and experience embodied in immigrant workers.

To calculate effective experience, we follow Borjas (2003) and estimate a conventional wage regression for immigrants and natives, where for immigrants we separate between years of experience acquired abroad and years of experience in Israel. The effective value of experience in Israel (abroad) is then simply calculated as the ratio of the marginal value of an additional year of experience in Israel (abroad) to the marginal value of a year of experience for natives. See Appendix B for details.

To calculate effective schooling, we follow a different approach, which we briefly summarize here (for the full details, see Appendix C). We first construct a matrix of the one-digit occupational distribution of immigrants (with different levels of experience in Israel) and natives by schooling category (we consider four schooling categories: less than high school, high school, some college, and college or more). We then look for a set of weights $\pi_{jj'}$ $(0 \le \pi_{jj'} \le 1, \sum_{j'} \pi_{jj'} = 1, \quad j, j' = 1,2,3,4)$ that minimize the distance between the occupational distribution of immigrants and natives. These weights then represent the effective schooling of immigrants: an immigrant in actual schooling category j is equivalent to π_{j1} natives in schooling category 1, π_{j2} natives in schooling category 2, and so on. This approach captures the slow transferability of human capital acquired abroad, and reflects more accurately the schooling of natives with which immigrants are effectively competing.

Table 3 presents the number of distinct cells in each segmentation, the average number of observations used to calculate the immigrant share, and the overall average in the fraction of immigrants according to the five different labor market segmentations.[11]

Dynamic Model

We extend now equation (1) to allow for immigrants with different levels of tenure in Israel to have a different impact on native outcomes. Specifically, let IMM_{jst} be the ratio of immigrants with s years of tenure Israel in cell j at time t to the size of cell j in 1989. Then the estimating equation becomes

$$\begin{aligned} y_{ijt} = \beta_0 + \gamma_0 IMM_{j0t} + \gamma_1 IMM_{j1t} + ... + \gamma_{10} IMM_{j,10,t} \\ + \beta_2 Z_{jt} + \beta_3 X_{ijt} + \alpha_j + \delta_t + \eta_{jt} + u_{ijt}. \end{aligned} \quad (2)$$

We are particularly interested in the pattern of the γ coefficients. As shown in Section 2, this pattern depends on the degree of substitutability between immigrants and natives in the short run, and on the speed of adjustment of local factors of production to the migration wave. If immigrants and natives in cell j are close substitutes, and the capital stock adjusts slowly in the short run, we expect the short-run γ's to be significant and negative, while the medium run γ's to be smaller.

Conversely, if the capital stock is quick to adjust, and immigrants are relatively poor substitutes for natives, with the degree of substitutability increasing over time as immigrants gradually acquire local labor market skills, we could have a scenario in which the initial impact of immigration is negligible (or maybe even positive if immigrants and natives are complements, and immigration pushes up the

[11] The proportion of immigrants in each labor market segmentation is calculated using the sampling weights in the LFS.

marginal productivity of Israeli workers), but the effect becomes more negative over time. In this case, the short run γ's are zero or maybe even positive, while the adverse impact of immigration manifests itself in the long-run γ's.

Since we have only eleven years of data, it might be difficult to estimate precisely the coefficients on the long-run γ's. For example, γ_{10} is identified only from the 1999 wave of the LFS, and there might not be enough observations in each cell to obtain a satisfactory estimate of this parameter. Therefore, we adopt a linear functional form for the dynamic pattern of the γ's. Specifically, we assume that

$$\gamma_s = \lambda_0 + \lambda_1 s.$$

Substituting for γ_s in equation (2), we obtain:

$$\begin{aligned} y_{ijt} &= \beta_0 + \lambda_0 \sum_s IMM_{jst} + \lambda_1 \sum_s s \times IMM_{jst} + \beta_1 Z_{jt} + \beta_2 X_{ijt} + \alpha_j + \delta_t + \varepsilon_{ijt} \\ &= \beta_0 + \lambda_0 IMM_{jt} + \lambda_1 I\tilde{M}M_{jt} + \beta_1 Z_{jt} + \beta_2 X_{ijt} + \alpha_j + \delta_t + \eta_{jt} + u_{ijt}, \end{aligned} \qquad (3)$$

where IMM_{jt} is the ratio of total stock of immigrants in cell j at time t to the size of the cell in 1989 (defined exactly as in equation (1) in the static model), and $I\tilde{M}M_{jt}$ is the weighted sum of ratios of immigrant-years in cell j at time t to the size of the cell in 1989. In this specification, the parameters λ_0 and λ_1 have a very straightforward interpretation: λ_0 , which is equivalent to γ_0 in (2), measures the immediate impact of immigration on labor market outcomes.

If immigrants upon arrival are close substitutes to natives, we expect λ_0 to be negative, while it should be zero or even positive if the degree of substitutability is low. The second coefficient, λ_1, measures how the impact of immigration changes over time. We expect λ_1 to be positive if the adverse impact of immigration becomes smaller over time, whereas it should be negative if the native labor market is negatively affected only some years after the initial arrival of immigrants. A simple hypothesis test for the null of λ_1 equal to zero essentially tests whether the impact of immigration is homogeneous over time.

Identification Issues

If all the segment specific effects (the α_j's and the η_{jt}'s) were uncorrelated with the proportion of immigrants in a segment, we could exploit the variation in the fraction of immigrants both across cells and over time, and estimate equations (1) and (3) by simple OLS, adjusting the standard errors for within segment correlations in the error term.

It is important, however, to make sure that the effect we identify in the dynamic model is not simply due to the selection of immigrants across labor market segments. To illustrate the problem, consider the following simple two period example: the labor market consists of two segments, a low wage and a high wage segment. The wage in each segment is fixed and is not affected by immigration. In each period, a wave of immigrants arrives and is employed in the low wage segment. After one period in the host country, all immigrants move to the high wage segment of the labor market. Therefore, all recent immigrants are concentrated in the low wage segment, and all veteran immigrants are concentrated in the high wage segment. As a result, wages are negatively correlated with the concentration of recent immigrants, and positively correlated with the concentration of veteran immigrants. Despite the fact that immigration has no effect on wages, we could erroneously conclude that the initial effect is negative, and then disappears in the medium run. In this simplified example, controlling for segment specific effects would prevent us from reaching the wrong conclusion. The key identifying assumption here is that the fraction of immigrants is potentially correlated with the unobserved overall level of wages or employment in a segment, but we rule out the possibility that it is correlated with unobserved *changes* in wages or employment.

Controlling for segment specific fixed effects is not enough if the segment specific wages are not fixed. Assume for example that wage growth in the high wage segment is faster than in the low wage segment. Then the deviation in wages from the segment mean is positively correlated with deviation in the fraction of veteran immigrants from the segment mean, while it is negatively correlated with the deviation in the fraction of recent immigrants from the segment mean. Hence, even controlling for fixed effects would yield a spurious conclusion that the impact of immigration changes over time. To alleviate this concern, we test the robustness of the estimates to the inclusion of more complex dynamic structures of the segment specific effect. In particular, we control for segment specific time trends, and higher level fixed effects (e.g., one-digit occupation fixed effects when segments are defined by two-digit occupation cells) interacted with a full set of time dummies. In these two specifications, identification is achieved from the deviation in wages and immigrant concentration from their segment specific trends, or from the higher-level mean in a specific year.[12]

5. Results

From this point on we will focus exclusively on native outcomes. The sample of "native" workers, (which includes both Israeli born and veteran immigrants), is taken from the 1989-1999 Labor Force Surveys and Income Surveys. Summary statistics for this sample are presented in Table 4.[13]

Wages

The first two columns of Table 5 present the estimation results for the effect of immigration on natives' log hourly wage, assuming that the effect of immigration is homogeneous over time. We present results for both males and females, with and

[12] Our specification constrains the selectivity patterns of immigrants across labor market cells to at most follow a linear trend, but the process of allocation of immigrants to cells may be more complex and time-varying. This could happen if, for example, the recognition of immigrant qualifications and diplomas responds to shortages or political lobbying, in which case there would be discrete jumps in the fraction of immigrants within a cell. This phenomenon, however, is unlikely to matter for more than a few selected occupations (physicians, lawyers, etc.). Physicians, for example, were required to obtain a license to practice in Israel, and in some cases the requirements for acquiring such a license changed during the early 1990s (Kugler and Sauer, 2005). Removing these occupations from the sample did not substantively affect our results. Moreover, while our identification strategy using 2-digit occupations ignores such changing selections rules, the variety of segmentations we present serves as robustness checks for our results.

[13] The Income Survey excludes households in small localities, hence sample sizes for the income variables are smaller.

without cell fixed effects, and for all the possible segmentations of the labor market.[14] We first examine the specification without fixed effects in the first column of the table. The results here are sensitive to the choice of labor market segmentation. When the segmentation is based on occupational category, we generally find a strong negative correlation between immigrant concentration and native wages. On the other hand, the fraction of immigrants in a schooling-experience cell is *positively* correlated with native wages. The correlation is very strong in the segmentation based on actual schooling and experience, and substantially weaker when we use adjusted schooling and experience. There is a simple explanation for this finding. Immigrants from the FSU are substantially more educated than natives ; however, upon arrival, they cluster in low skill jobs that pay low wages (Eckstein and Weiss, 2002; Weiss, Sauer and Gotlibovski, 2003). Therefore, at the cross-sectional level, we expect to find a strong negative correlation between the fraction of immigrants and natives' wages at the occupational level, but a positive correlation between immigrants and natives' wages when we segment the labor market by schooling and experience.

Part of the correlation that we estimate may rise from the selectivity of immigrants across labor market cells. Hence, we should not attach any causal interpretation to the estimates in the no-fixed effects specification; however, we believe that it is important to report them in order to better understand the nature of the selection of immigrants across labor market segments.

The fixed effects estimates in the second column of the table reinforce the above interpretation. In nearly all specifications, we find that the coefficient estimate in the fixed effect specification is substantially smaller (in absolute value) than the coefficient estimate when fixed effects are not included. For males, the coefficient is negative and statistically significant when we segment the labor market by district of residence and occupation, it is essentially zero in the other occupation-based

[14] The regressions are run separately for men and women, but the key explanatory variable is calculated as the ratio of *total* immigrants (both men and women) to native employment in a labor market cell in 1989.

segmentations and in the adjusted schooling-experience segmentation, and it is still positive and significant in the actual schooling-experience segmentation. For females, the coefficient is negative and statistically significant in all the occupation-based segmentations and in the adjusted schooling-experience segmentation, and it is positive and statistically significant in the actual schooling-experience segmentation. The results based on the actual schooling-experience segmentation are not entirely unexpected: they reinforce the belief that human capital accumulated abroad is not entirely transferable to the host economy (Friedberg, 1999; Eckstein and Weiss, 2004; Kugler and Sauer, 2005), especially in the short run, and hence complementarities between immigrant and native workers are likely to arise in the segmentation based on actual schooling and experience.

The estimates of the dynamic model are presented in specifications 3 and 4. Once again, to illustrate the nature of the selection process, we present results from specifications without segment fixed effects (specification 3) and with segment fixed effects (specification 4). When fixed effects are omitted, we find a pattern similar to that of the static model: in the occupation-based segmentations and in the segmentation based on adjusted schooling and experience there is a very strong short run negative correlation between immigration and native wages, with the sign of the effect reverting in the medium run.

The pattern of signs is reversed in the actual schooling-experience segmentation. As discussed above, this is likely to be due to the selection of immigrants upon arrival in low wage segments, and their subsequent move up the occupational ladder. In fact, when segment fixed effects are included, the estimate for both λ_0 and λ_1 fall substantially. However, with the exception of the segmentation based on actual schooling and experience, we find that that λ_0, the estimate for the immediate effect of immigration on wages, is negative and nearly always statistically significant. The estimate of λ_1 is always positive and is statistically significant in five of the eight segmentations: in the two-digit occupation segmentation it is statistically significant for both males and females, and of similar magnitude.

The pattern of signs in the actual schooling-experience segmentation is reversed, even though the estimates are not statistically different from zero at conventional significance levels. Again, this suggests that immigrants with a given level of schooling and experience are not necessarily substitutes to natives with the same objective attributes. In fact, the positive short-run and negative long-run coefficients are not entirely surprising in this specification, since it is exactly when we segment the labor market by schooling and experience that we expect the degree of substitutability between immigrants and natives to increase over time.

It is worthwhile to compare these results to those of the static model: assuming that the effect of immigration is constant over time and using the two-digit occupation segmentation, we would have concluded that the elasticity of native male wages with respect to immigration is zero, and that of females is –0.12. However, when we allow the effect to differ depending on immigrants' tenure in Israel, our conclusion is dramatically altered. The short run elasticity of wages is –0.20 for males and –0.28 for females, and it takes between 5 and 7 years for occupation-level wages to return to their pre-immigration level.

Employment

The first two columns of Table 6 present the estimates of the static model for employment rates. For males, the pattern is similar to that found for wages. There is a negative cross-sectional correlation between employment and immigrant penetration, but this relationship disappears once we control for segment specific effects. Interestingly, we do not find any evidence of a positive correlation in the actual schooling-experience segmentation. For females, we observe a negative cross-sectional correlation in the occupation-based segmentations, and a positive correlation in the actual schooling-experience segmentation, while the correlation is zero in the adjusted schooling-experience segmentation. All of the correlations switch signs when we include fixed effects, although only the coefficient in the 2-digit occupation segmentation is statistically significant.

In the remaining columns of Table 6 we present the estimates of the dynamic model for employment rates. In the specification without fixed effects, we find the familiar pattern of coefficients, driven by selection. The fixed effects estimates, on the other hand, yield mixed results: we find a short-run negative correlation for males, which diminishes over time, in the actual schooling-experience segmentation; and a positive short-run correlation for females in the two-digit occupation segmentation. All the other coefficients are statistically insignificant, and it is difficult to detect any consistent pattern in the signs of the estimates. Overall, it seems difficult to draw any definite conclusions on the effect of immigration on natives' employment rates. This could be due to several factors. First, our sample is based only on workers in the labor force: it is possible that immigration operates mainly on the labor force status margin.[15] Second, there seem to be important secular trends in both male and female labor supply (see Figure 2), which may make it difficult to identify any effects due to immigration. Finally, if the labor supply curve is inelastic, we would indeed not expect immigration to have any effect on natives' employment.

Robustness Checks

Since there appears to be essentially no effect of immigration on employment, neither in short nor in the medium run, we report robustness checks for the effect of immigration on native wages alone.[16] Moreover, we exclude from the analysis the actual schooling-experience segmentation: it is clear that the dynamics based on this segmentation are different, because of the low-transferability of imported human capital, and the consequent low degree of substitutability between native and immigrants with the same level of formal education and experience. The results are presented in Table 7.

[15] For the schooling-experience segmentation, we also tried to expand the sample to all individuals, and use the employment-population ratio as the left hand side variable. The results did not differ substantively from those reported in the table. We choose to report this specification to facilitate comparisons with the other segmentations.
[16] Similar robustness checks for employment regressions yielded essentially the same results as in Table 6.

The first two columns of the table adjust standard errors for potential serial correlation in the error term. The standard errors reported in Table 5 are correct if there is no serial correlation between the residuals in a particular labor market cell (formally, u_{ijt} and u_{ijs} in equation (3) must be uncorrelated for any two periods s and t). As shown by Bertrand, Duflo and Mullainathan (2004), serial correlation within clusters in differences in differences analysis can lead to serious biases in estimated standard errors, especially so if the explanatory variable of interest is highly persistent. To address this concern, Bertrand et al. suggest estimating the equation with clustering at the cell level, rather than at the cell-time unit level. Column (1) in Table 7 replicates column (4) in Table 5, but presents autocorrelation-robust standard errors. For males, the precision of the estimates is slightly lower in the district of residence-occupation and in the industry-occupation segmentations, but the coefficient for the immediate impact remains significant. By contrast, the estimate of λ_0 in the adjusted schooling-experience segmentation becomes statistically significant at the 10 percent level. For females, the largest increase in standard errors occurs in the two-digit occupation segmentation, but the coefficients remain significant.

We next test whether our results are driven by the fact that different cohorts of immigrants affect the labor market differently. If the impact of immigration is indeed the same, regardless of immigrant tenure in Israel, but different cohorts of immigrants affect native outcomes differently, we would face an identification problem similar to the one that arises in the estimation of the immigrant wage-tenure profile (Borjas, 1985). In a single cross-section, it is impossible to identify separately tenure effects from cohort effects.

With repeated cross-sections, as in our data, identification becomes possible, but one must impose additional restrictions.[17] Therefore, we distinguish between two

[17] To see this, let IMM_{jst} be the fraction of immigrants with s years of tenure in Israel, and let IMM_{jct} be the fraction of immigrants who arrived in cohort (year) c. A general model would allow a different effect for immigrants of any possible combination of cohort and tenure. However, this model is clearly not identified since in a given cell, the sum of the number immigrants with different tenure in Israel is identical to the sum of the number of immigrants from different cohorts. Mathematically,

cohorts of immigrants: those who arrived in the initial wave between 1989 and 1992 (these immigrants essentially fled the Soviet Union in haste, fearing that the country would fall into chaos, and can be described as "refugees"); and those that arrived in 1993 and later, which share more of the features of economic migrants. The identifying assumption is that the impact of the first cohort is zero, and we test whether adding cohort effects changes our estimates from Table 5. The results of this exercise are presented in column (2) of Table 7. It turns out that the inclusion of a cohort of immigration dummy has little effect on either the magnitude or the significance level of the coefficients.

In specification (3) we test for robustness of our estimates to a more flexible specification of the error term structure. Specifically, we allow the segment effects to be time-varying, but we restrict the dynamics to follow a linear trend.[18] The regression equation is estimated with a full set of segment-specific fixed effects, and a full set of segment-specific effects interacted with a linear time trend. In other words, we attempt to identify any effects of immigration from the deviations in wages and immigrant concentration from their segment specific trends.

For both males and females we find that the short-run effect of immigration is smaller in absolute value once we control for a segment specific trend. For males, two of the three significant coefficients in the benchmark case remain statistically significant. The short-run coefficient in the two-digit occupation segmentation is halved in size and becomes insignificant. For females, all the coefficients become insignificant at the 5 percent level, although the pattern of signs is preserved, and all the t-statistics are above one.

In specification (4) we go one step further, and relax the linear trend assumption for the dynamics of the unobserved effect. Instead, we assume that the segment specific effect can vary freely over time, but the dynamics are constant

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$\sum_s IMM_{jst} = \sum_c IMM_{jct}$. Therefore, we have a perfect multicollinearity problem. The same identification problem arises even if we impose a linear structure on the pattern of coefficients.

[18] Formally, in equation (3) we substitute η_{jt} with $\zeta_j \times t$.

within broad groupings of segments. Specifically, suppose that the index $j_{g_1 g_2}$ denotes that segment j belongs to broad groupings g_1 and g_2. For example, if the segmentation is based on district of residence and one-digit occupational category, then the labor market segment for professional workers in the Tel Aviv district belongs to the aggregate grouping of all professional workers (g_1), and to the aggregate grouping of all Tel Aviv residents (g_2). Then, the individual effect for segment $j_{g_1 g_2}$ at time t is $\eta_{j_{g_1 g_2} t} = \theta_{g_1}\delta_t + \theta_{g_2}\delta_t$. In our example, this means adding to regression (4) in Table 4 a full set of district of residence dummies interacted with a full set of year dummies, and a full set of one-digit occupation dummies interacted with a full set of year dummies. In this specification, identification is achieved off the deviations in segment-specific immigrant concentration and wages from their overall mean in the sample period (because of the inclusion of the cell fixed effects) and from the period t mean in broad groupings of segments.

For males we find that the short-run effect of immigration disappears in the residence-occupation segmentation, but is unaffected in the other three segmentations. For females, the effect maintains its sign and significance level only in the two-digit occupation segmentation. It is difficult to interpret these results: on one hand, it's possible that part of the estimated coefficient in the fixed effects specification was capturing the concentration of newly arrived immigrants in sectors with temporarily low wages; on the other hand, it could be that the more complex dynamic structure of the unobserved component swamps out much of the useful variation that is necessary to estimate the effect precisely.[19]

In Table 8 we check whether the results are robust to a more flexible specification of the dynamic impact of immigration on native wages. In particular, we specify a piecewise-constant function for the γ's in equation (2):

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[19] We have also estimated all the models on the data grouped at the segment-calendar quarter level, using weighted least squares. All the results are essentially unchanged, and can be obtained by the authors upon request.

$$\gamma_s = \mu_0 \cdot 1(s=0) + \mu_1 \cdot 1(1 \le s \le 3) + \mu_2 \cdot 1(4 \le s \le 6) + \mu_3 \cdot 1(7 \le s \le 10).$$

The estimating equation then becomes

$$y_{ijt} = \beta_0 + \mu_0 IMM_{j0t} + \mu_1 \sum_{s=1}^{3} IMM_{jst} + \mu_2 \sum_{s=4}^{6} IMM_{jst} + \mu_3 \sum_{s=7}^{10} IMM_{jst} + \\ + \beta_1 Z_{jt} + \beta_2 X_{ijt} + \alpha_j + \delta_t + \varepsilon_{ijt}. \qquad (4)$$

This specification allows us to identify more accurately the dynamic structure of the immigration impact. We estimate equation (4) both with only cell fixed effects, and with a full set of cell effects interacted with a linear trend. In both specifications, we find that the adverse impact of immigration on native wages is concentrated one to three years after the immigrants' arrival. This effect is present for both males and females, and is statistically significant in nearly all segmentations.

Controlling for a more complex dynamic structure of the error term has little effect on the estimates for males, while it reduces by about half those for females. The estimates imply that a 10 percent increase in the fraction of immigrants with one to three years of tenure in Israel reduces native wages by 0.9 to 3.2 percent for males, and by 0.4 to 5.1 percent for females. At all other time spans, the effect is essentially zero in all specifications. The fact that the effect is concentrated in the short run (though not in the very short run, at zero years of tenure), is consistent with the hypothesis that immigrants are substitutes for native workers, and that other factors of production adjust within one to three years after the immigrants' arrival, so that in the medium and long run the effect of immigration on native wages is essentially zero.

Differences between low-skill and high skill sectors

The empirical analysis so far may have been too restrictive, as it imposed the same immediate and long-term effects on workers in different skill groups. However, it is possible that in low skill jobs that require little training and local skills, immigrants and natives are more likely to be substitutes with one another, while in the high-skill sectors there may be more scope for complementarities. Therefore, we allow the effect to differ between two broad categories of workers: white-collar (highly-skilled) and blue-collar (low skilled) workers.[20]

Table 9 presents the results from the dynamic model with fixed effects and with fixed effects interacted with a linear trend, separately for blue-collar and white-collar workers. There is a noticeable difference in the sign of the initial effect and in its significance between blue-collar and white-collar workers. Specifically, in blue-collar occupations we find the same broad pattern that was present in the overall sample: the initial effect tends to be negative and significant, and as immigrants spend time in Israel this effect diminishes. However, the initial effect for white-collar workers has an inconsistent sign and is insignificant.

Based on the results from the separate regressions for different skill groups we conclude that the source of our previous findings (Tables 5, 7, and 8) comes from the dynamic effect of immigrants on low-skilled blue-collar native workers and that white-collar native workers are not significantly affected by immigration neither in the short run nor in the medium run.

Last, one may argue that the impact of immigrants' share on wages of natives diminishes over time due to the response of natives who gradually move out from labor market segments which attracted high shares of immigrants. In order to rule out such a scenario, we exploit the panel structure of the Israeli LFS and follow mobility of natives between segments over time.

Specifically, the LFS is a rotating panel, where each household is interviewed for two consecutive quarters, followed by a break of two quarters, and is interviewed again for two consecutive quarters (see appendix A). Although potentially, we can follow the individual up to 18 months, there is non negligible attrition between the four interviews. Thus, we check for natives' segment mobility in 3 months interval and 12 months interval. The 3 months interval is based on mobility which occurred

[20] White-collar workers are defined as those in occupations 0-299 in the CBS 1994 occupational classification (scientific and academic professionals, other free professionals and technicians, managers); blue-collar workers are defined as those in occupations 300-999 (clerical workers, sales workers, service workers, farm workers, skilled workers in industry, and unskilled workers).

between the 1st and 2nd interview and between the 3rd to 4th interview, while the 12 months interval is based on mobility which occurred between the 1st and 3rd interview and between the 2nd and 4th interview.

As before, we consider a segmentation based on the two-digit occupational classification, as well as one based on 1-digit industry interacted with 1-digit occupation. Overall, the fraction of workers who switch between cells is rather high (about 30 percent at a three months interval, and 35 percent at a twelve month interval), and is somewhat higher for immigrants than for natives. This is not necessarily a concern for our estimates, as long as mobility is not correlated with the fraction of immigrants in one's cell.

In Table 10 we present results from a linear probability model for the probability of natives to move out from his/her original labor market segment as a function of immigrants' share in this particular segment. The first two columns are based on mobility across segments over the three months interval, while the last two columns are based on mobility over the 12 months interval. In the 2-digit occupation segmentation for males (Table 10, first two rows), we find that immigrants' share in a specific occupation did not significantly affect the mobility of natives out of this occupation, neither in the static model nor in the dynamic model. This conclusion holds both in the three months interval and in the 12 months interval. In the second segmentation, the share of immigrants *lowers* the probability of natives to move out of the segment in the constant model based on the three months interval, but has no impact on natives' mobility in the other specifications.

For females we also find that the share of immigrants in the original 2-digit occupation segment does not significantly affect the probability to move out of this occupation. In the second segmentation, based on the 12 month interval, immigrants' share lowers the probability of native females to move away from her original cell, rather than increasing it, both in the static model and in the dynamic model.

Overall, the results in this section support our assumption that our chosen labor market segments can be treated as isolated and that natives did not move out of labor market segments with high share of immigrants. Therefore, our main estimates are unlikely to suffer from significant bias. Our general conclusion that the impact of immigration is largest upon the immigrants' arrival, and then diminishes over time remains intact.

Comparison with Friedberg (2001)

Our results suggest that FSU immigrants had an immediate adverse effect on natives' wages. However, Friedberg (2001) found that the concentration of FSU immigrants in two-digit occupation cells had no adverse impact on native Israeli wages, once the selectivity of immigrants across occupations is accounted for. Thus, it is natural to ask why our conclusions differ from Friedberg's. The main differences between the two studies are *a)* the approach used to address the potential selectivity of immigrants across labor market cells in Israel and; *b)* the data used in the two studies.

In our study we use repeated cross sectional data and our identification strategy is based on the assumption that, after controlling for cell specific fixed effects, cell specific trends, or higher-level fixed effects interacted with a full set of time dummies, the distribution of immigrants across labor market cells is likely to be exogenous with respect to the residual error term. In other words, our strategy allows for potential correlation between immigrant concentration and both wage *levels* and wage *growth* within cells.

By contrast, Friedberg, use only data from the Income Surveys of 1989 and 1994, and her ingenious approach to the selectivity problem used the occupational distribution of immigrants in the FSU as an instrument for their occupational distribution in Israel. While Friedberg's OLS estimates (which controlled for two-digit occupation fixed effects) are broadly consistent with our findings, her IV estimates imply that immigrants had no effect on native labor market outcomes. It should be noted, though, that Friedberg's results were based on comparing natives' wages between 1989 and 1994, i.e., about 4 years after the bulk of the immigration wave. Our own results (Table 8) indicate that after 4 years the effect is close to zero and mostly insignificant. Nonetheless, in this section we further explore the differences between the two approaches to selectivity.

The IV approach used by Friedberg relies on the assumption that the instrumental variable is correlated with the original variable in interest and not correlated with the error term. Several papers suggest that FSU immigrants were not able to find a job in Israel that matched their skills and that they, by and large, accepted jobs for which they were clearly over-qualified (Weiss, Sauer and Gotlibovski, 2003).

For example, the mass immigration wave from the FSU contained an unusually large number of physicians and engineers. About 7 percent of the immigrants who arrived in the last quarter of 1990 worked as physicians in the FSU, and more than 15 percent worked as engineers. On the other hand, in 1994 only 2 percent of the immigrants were employed as physicians, and only 4 percent as engineers. The implication of these findings is that the correlation of Friedberg's instrument and the actual occupational distribution of immigrants in Israel may actually be quite low.

Friedberg used individual-level data to estimate the following equation:

$$\ln w_{ijt} = X_{it}\beta_t + \alpha_t + \sum_{k=1}^{J}\delta_k occ_k + \gamma r_{jt} + \varepsilon_{ijt}, \qquad t = 1989, 1994;$$

where w_{ijt} is the earnings of individual i in occupation j at time t, X_{it} is a vector of control variables, α_t is a year dummy, occ_{jk} are a set of J two-digit occupation dummy variables, and r_{jt} is the ratio of immigrant to native workers in the individual's occupation. As an instrument for r_{jt}, Friedberg calculated p_{jt}, the number of immigrants arrived in the last quarter of 1990 who were employed in occupation j in the FSU divided by the number of natives who were employed in occupation j in 1989, prior to the immigration wave.[21]

The validity of this IV strategy rests on the assumption that the instrument is correlated with the endogenous explanatory variable, r_{jt}, and is uncorrelated with the error term. Here we highlight that the correlation between r_{jt} and p_{jt} is in fact much weaker than that reported by Friedberg, casting doubt on the reliability of her IV estimates.

We were able to obtain the exact same data used by Friedberg in her original article. In Table 11, panel A we present the estimates from Friedberg's Table III: She found that native wages in a 2-digit occupation cell are significantly negatively correlated with the number of immigrants employed in that occupation in Israel, but significantly positively related with the number of immigrants employed in that occupation in the FSU. As a result, the 2SLS coefficient of immigrant concentration on wages is positive and significant. Importantly, the coefficient on p in the first stage regression is positive and highly significant.

Note that while the regression is estimated at the individual level (with more than 8300 observations), the key explanatory variable in the regression and the instrument both vary only at the occupation×year level. For this reason, Friedberg followed standard practice and reported standard errors clustered at this level of aggregation. However, we suspect that the standard clustering adjustment can be misleading in this setting. The number of clusters in the regression is 166 (83 two-digit occupations and two years of data), and the number of explanatory variables is 134.[22]

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[21] The data on the occupational distribution in the FSU was drawn from the Israeli Immigrant Employment Survey (IES), a random sample of 3300 FSU immigrants who immigrated to Israel between October and December 1990, and were re-interviewed annually between 1992 and 1994. The information on occupation in the FSU is drawn from the 1992 and 1993 waves of the survey.

[22] The explanatory variables are: 5 variables in the education spline, a quartic in experience, a full-time dummy, a gender dummy, two ethnic origin dummies (Arabs and Jews of Asia-Africa origin), a dummy for immigrant status, years since migration, 9 economic branch dummies, a year dummy and its interaction with all of the above controls, 83 occupation dummies, the key regressor of interest, and a constant.

Donald and Lang (2007, henceforth DL) examine the case of a simpler data structure, where there are J clusters, N_j observations per cluster, and the key regressor of interest varies only across clusters. They show in this case that when the number of clusters is small, there can be substantial small sample bias in the clustered standard errors and over-rejection of null hypotheses even when these are true. (DL, page 229).

It would appear that the number of clusters in our case is sufficiently large (DL use an example where the number of clusters is 4 to illustrate the pitfalls of the standard clustering adjustment). However, the results in DL do not carry over directly in our setting, because our data structure is more complex: there are JT occupation-year cells (the level at which the key regressor of interest varies), but the model also includes J occupation fixed effects. The behavior of the standard cluster-adjusted estimator in this case is not well understood. In fact, we have run some Monte Carlo simulations that show that, when the number of time periods is small, the conventional cluster-adjusted t statistic rejects the null hypothesis about 20 percent of the time even when the null is true.[23]

Moreover, p_{jt} and r_{jt} are identically set equal to zero for 1989, meaning that there is even less true variation in the data than that implied by the equation with individual level data. These factors are of particular concern in the first stage equation, where we use individual level data even though both the dependent variable and the key explanatory variables are grouped.

To address these concerns, we re-estimated Friedberg's key equation following the two-step procedure suggested by DL. We first run an OLS regression of log wages on all the individual-level regressors, and a full set of occupation-year dummies. We then use the 166 estimated occupation-year dummies and treat them as the dependent variable in the second step, which includes only regressors that do not vary within clusters. That is, in the second step we regress the occupation-year dummies on the percentage of immigrants in each occupation-year cell, a full set of

[23] Details available from the authors upon request.

occupation dummies, and a year dummy, using both OLS and 2SLS. The results, along with the first stage and reduced form estimates, are reported in Table 11, panel B.[24]

All the coefficients in this specification have the same sign as in the regressions estimated using individual level data. The standard errors, however, increase substantially. Remarkably, though, the first stage relationship between *r* and *p* becomes substantially weaker. The first stage F-statistic drops from 42 to 4.5. This is well below the rule-of-thumb value of 10 that is generally considered necessary to make 2SLS estimates reliable.[25] The 2SLS estimate is still positive and even larger than the one estimated using individual level data, but the standard error is now so large that the 95 percent confidence interval includes all values between -0.5 and 3.1. The 2SLS coefficient is essentially uninformative.

As a further robustness check, we also estimated the model using group averages, and weighting all estimates by cell size. This method has the advantage of providing conceptual clarification, even though it has the disadvantage of aggregating individual-level regressors, which can lead to a loss of information and noisier estimates. The results are presented in Table 11 Panel C. The first stage relationship between *r* and *p* becomes *negative* and insignificant, with a first stage F-statistics below 1. With such a weak first stage, the 2SLS coefficient is again essentially uninformative.

In panel D of Table 11 we estimate the regression in first differences using the grouped data. We believe that this is the most reasonable and intuitive specification for the data at hand, since we are not creating an artificial first stage correlation between the instrument and the endogenous regressor by setting $r_{jt} = p_{jt} = 0$ for half the observations. The results are similar to those in Panel C. Again, the 2SLS

[24] The variance covariance matrix of the error term in the second stage equation is non-spherical, because of potential intra-group correlation, and because we are using estimates of the occupation-year effects rather than the true values. Therefore, we calculate standard errors using the standard "sandwich" formula (White, 1980). We also tried using FGLS following the suggestion of DL, and the results are qualitatively identical.

[25] Staiger and Stock (1997).

coefficient is insignificant, and the first stage F-statistic may be too small for 2SLS inference to be reliable.[26]

Summing up, it appears that the IV strategy used by Friedberg suffers from a “weak instruments” problem, and the appropriate standard errors are too large to make any meaningful inference. In addition, even a small degree of correlation between the instrument and the error term (for example, skill-biased technical change favoring the occupations in which the immigrants were employed in the FSU – a possibility acknowledged also by Friedberg) could result in substantially biased 2SLS estimates because of the weak instrument. Overall, we believe that our estimates, based on OLS with a flexible structure of the error term that can account for the potential correlation between immigrant concentration and both the *level* and *growth* rate of wages across labor market cells, yield a more credible picture of the true effect of immigration on native labor market outcomes.

6. Conclusion

This paper studies the dynamic impact of the mass migration from the former Soviet Union on native Israelis’ labor market outcomes. The key feature of our paper is allowing the impact of immigration to vary over time. Our results indicate that immigration did have a short-run adverse impact on wages, with the effect dying out after 5 to 7 years. However, we do not find any immediate nor delayed impact on employment.

Though most of the immigrants had high level of imported human capital, the effect on natives’ wage comes from the effect of immigrants on low-skilled blue-collar natives, while the wage of white-collar native workers is not affected from immigration neither in the short-run nor in the long-run. Interestingly, we find that

[26] We also ran a small simulation (500 replications), in which we took the original data, but substituted $r_{j,94}$ with a simulated random variable that is completely uncorrelated with $p_{j,94}$ (while keeping $r_{j,89} = 0$). Using individual level data, one falsely rejects the null of no relationship between r and p 45% of the time, and the first-stage F statistic is above 10 25% of the replications.

the effect of immigration is quite similar for both men and women. This suggests that there is no reason to neglect the impact of immigration on native females.

Our preferred estimates suggest that a 10 percent increase in the share of immigrants lowers natives' wages in the short run by 1.2 to 5.7 percent. These estimates are higher than those reported in the Friedberg and Hunt's survey (1995) who concluded that a 10 percent increase in the fraction of immigrants reduces natives' wages by at most 1 percent. Nonetheless, our estimates are in line with the estimates in Borjas (2003), who found that native wages are reduced by 3-4 percent.

Our findings are consistent with the notion that within occupation-oriented segments, immigrants are close substitutes to natives in the short run and depress natives' wages; however, as the labor market adjusts to the migration wave through offsetting flows of capital and other factors of production, the adverse effect is diffused in the medium and medium run. These results are robust to the inclusion of cohort effects and to the selection of immigrants into low wage or low wage growth segments in the labor market.

The idiosyncratic characteristics of the Israeli case study may make our results difficult to extend to other countries. The Soviet migration wave represented a sudden large deviation of the immigration rate from its long-run steady-state level. In this setting, it is not surprising that the short-run impact of immigration was substantial, as other factors of production did not have time to adjust due to the unexpected nature of the shock. The dynamic response of the labor market to small fluctuations in the immigration rate from its steady-state, or to gradual increases in the immigration rate, should not necessarily resemble that found in our paper. Nevertheless, we view our methodological contribution as potentially important for understanding the economic impact of immigration in other contexts as well. We leave the investigation of this matter for future research.

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Appendix A: Data

We use micro data from the Israeli *Labor Force Survey* (LFS) and the *Income Survey* (IS) of 1989-1999. The LFS is a rotating panel, where each household is interviewed for two consecutive quarters, followed by a break of two quarters, and is interviewed again for two consecutive quarters. In the fourth interview, a sub-sample of the respondents is asked questions about their income, and this information makes up the Income Survey. The Income survey excludes households in kibbutzim, collective moshavim, and other localities with a population below 10,000. The native male sample includes men between 25 and 65, the native female sample includes women between 25 and 60.[27] We do include non-Jews in our sample, but we exclude ultra-orthodox Jews since most of them dedicate their time to Torah study and are permanently out of the labor force. We also exclude people who reported more than 30 years of schooling. Since most of our segmentations are based on workers' occupational category, we also drop from the sample workers who did not report a previous occupation. This excludes from the sample workers who are unemployed for more than a year and individuals out of the labor force who did not work in the calendar year prior to the survey date.

Appendix B: Calculation of Effective Experience

To calculate effective experience, we follow Borjas (2003). We estimate the following wage equation, jointly for natives and immigrants, using all the available Income Surveys between 1989 and 1999:

$$\begin{aligned}\log w = {} & \alpha^{N}{}' N \cdot Ethnic + \beta^{N} N \cdot SCH + \gamma_1^{N} N \cdot EXP + \gamma_2^{N} N_i EXP^2 + \\ & \alpha^{I} I + \beta^{I} I \cdot SCH + \gamma_1^{F} I \cdot EXPFOR + \gamma_2^{F} I \cdot EXPFOR^2 + \\ & \gamma_1^{I} I \cdot EXPISR + \gamma_2^{F} I \cdot EXPISR^2 + \delta Y + \lambda C + v,\end{aligned}$$

[27] We omit younger workers because in some of the years the CBS masks the actual age of individuals between 18 and 24. Moreover, all young Jews serve in the military for a compulsory period of between 20 months (women) and 36 months (men), hence they are not members of the labor force. The upper limit represents the mandatory retirement ages for men and women during the sample period.

where N and I are dummies for "natives" (Israeli born and veteran immigrants) and post 1989 immigrants respectively, *SCH* denotes years of schooling, *EXP* is years of potential experience for natives, *EXPFOR* is years of potential experience abroad for immigrants, *EXPISR* is years of experience in Israel, Y are year dummies, and C are cohort of immigration dummies. The effective value of a year of experience abroad is the ratio of the returns to experience abroad to the returns to experience for natives (evaluated at the average value of experience abroad for immigrants), and the effective value of experience in Israel is calculated analogously. Appendix Table B1 presents the details of the calculation of the effective value of experience for immigrants

Appendix C: Calculation of Effective Schooling

Let M be a 10×4 matrix whose representative element m_{ij} is the proportion of immigrants in schooling category j (less than high school, high school, some college, and college or more) who are employed in one-digit occupation i (occupation category 10 represents workers who are not employed or have a missing occupation). Let N be the analogous matrix for natives. Let M_j be the j^{th} column of the matrix M. Our goal is to find, for every schooling category j, the vector $\pi_j = \left(\pi_{j1}, \pi_{j2}, \pi_{j3}, \pi_{j4}\right)'$ that minimizes the distance between N and M_j. Formally,

$$\hat{\pi}_j = \arg\min_{\pi} \left(N\pi - M_j\right)' W^{-1} \left(N\pi - M_j\right)$$
$$s.t.: \sum_{j'=1}^{4} \pi_{jj'} = 1,$$
$$0 \le \pi_{jj'} \le 1, \qquad j' = 1, 2, 3, 4.$$

For simplicity, we use the identity matrix as the weighting matrix. The resulting coefficients $\pi_{jj'}$ represent the probability of an immigrant with actual schooling j to be equivalent to a native with schooling j'. We estimate a different set of π's for men and women, and for immigrants with 0-2, 3-5, and 6-10 years of experience in Israel. The resulting matrices are presented in Appendix Table C1.

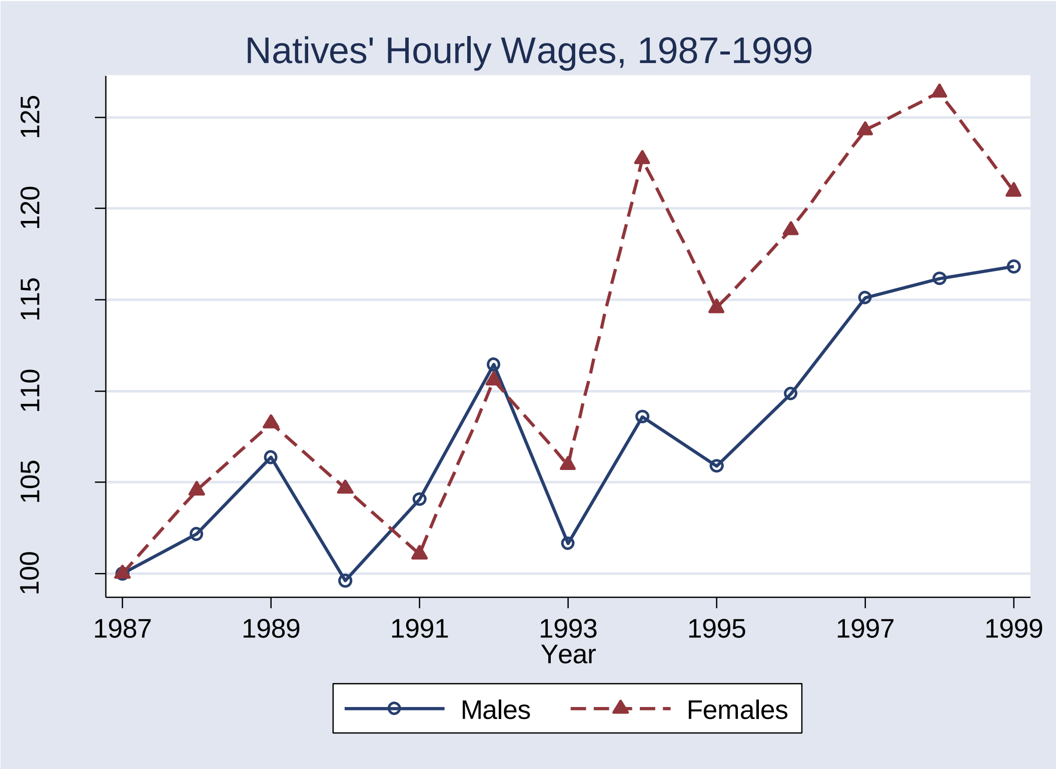

Figure 1: Natives' Hourly Wages, 1989-1999

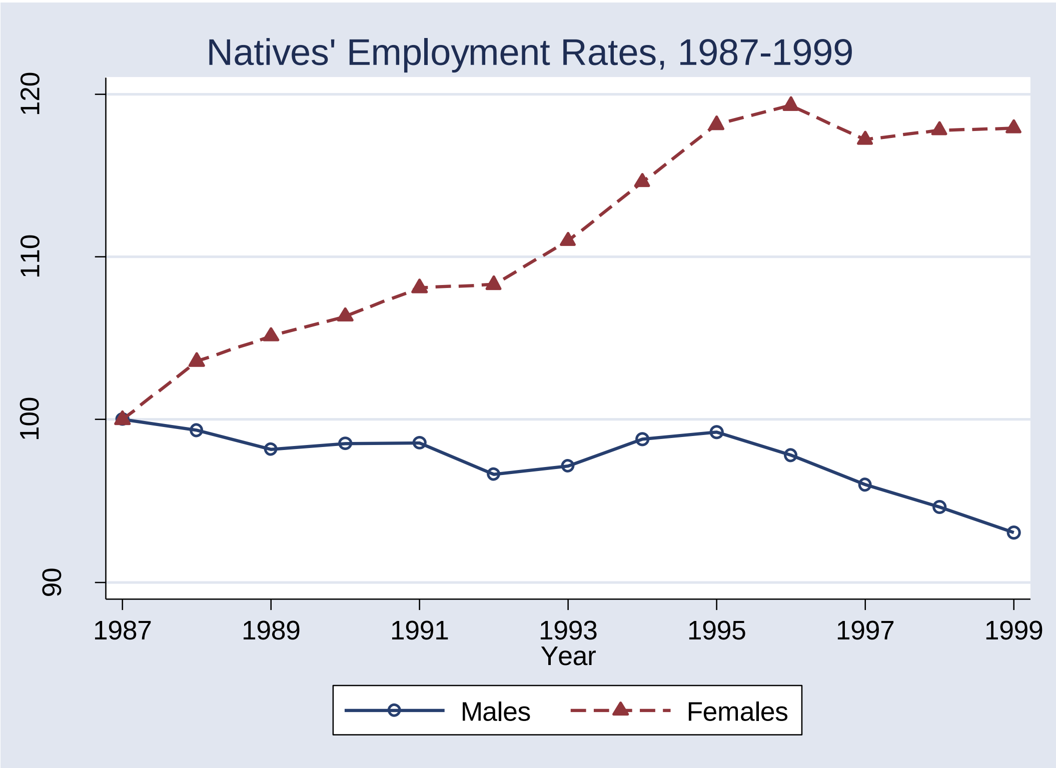

Figure 2: Natives' Employment Rate, 1989-1999

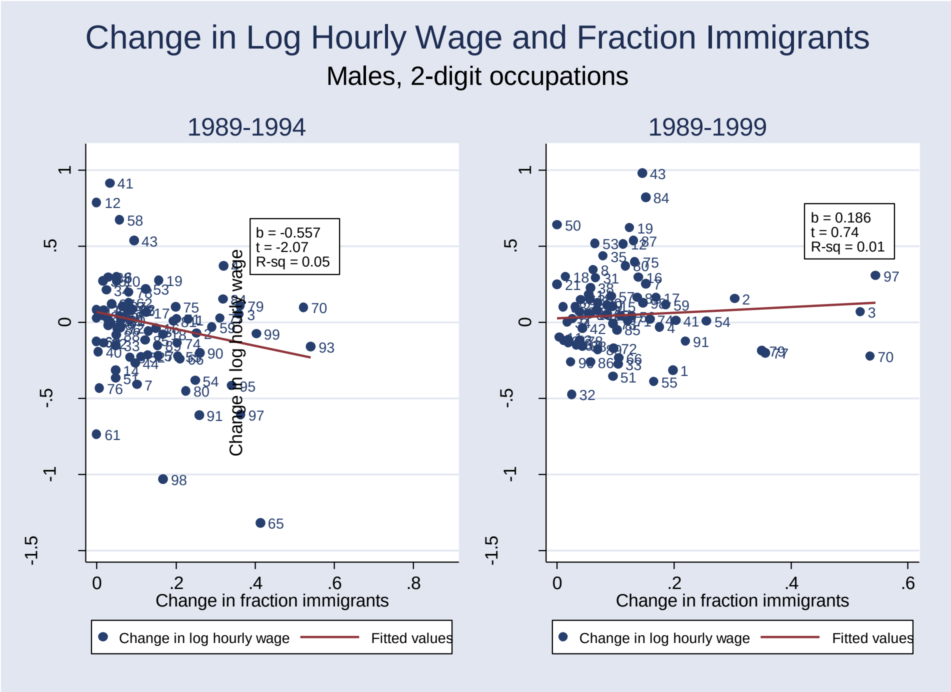

Figure 3: Change in Log Hourly Wages and Fraction of 1989-1991 Immigrants Males, 2-digit occupations

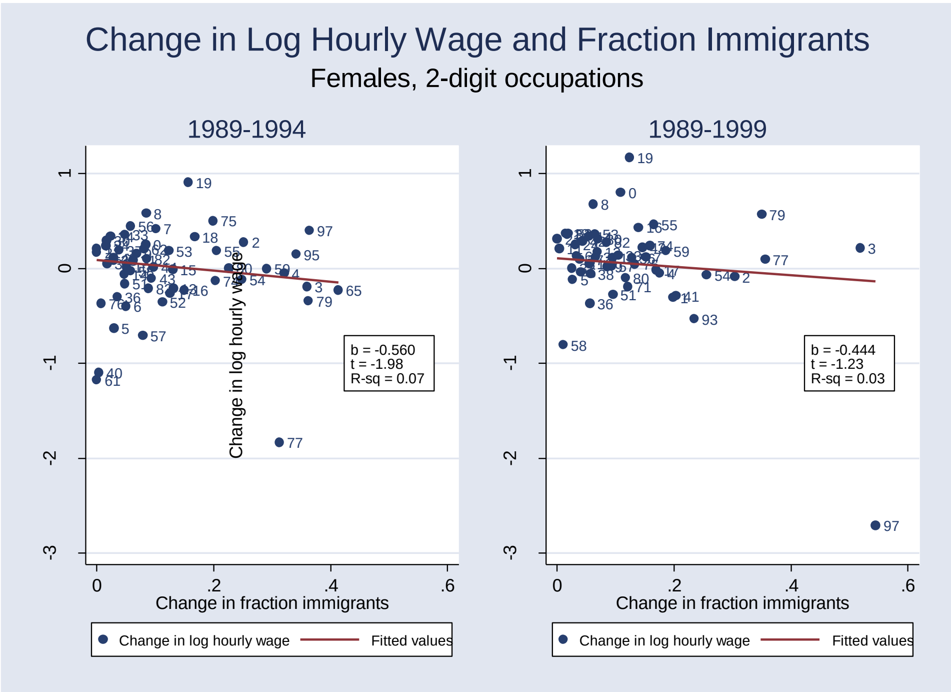

Figure 4: Change in Log Hourly Wages and Fraction of 1989-1991 Immigrants Females, 2-digit occupations

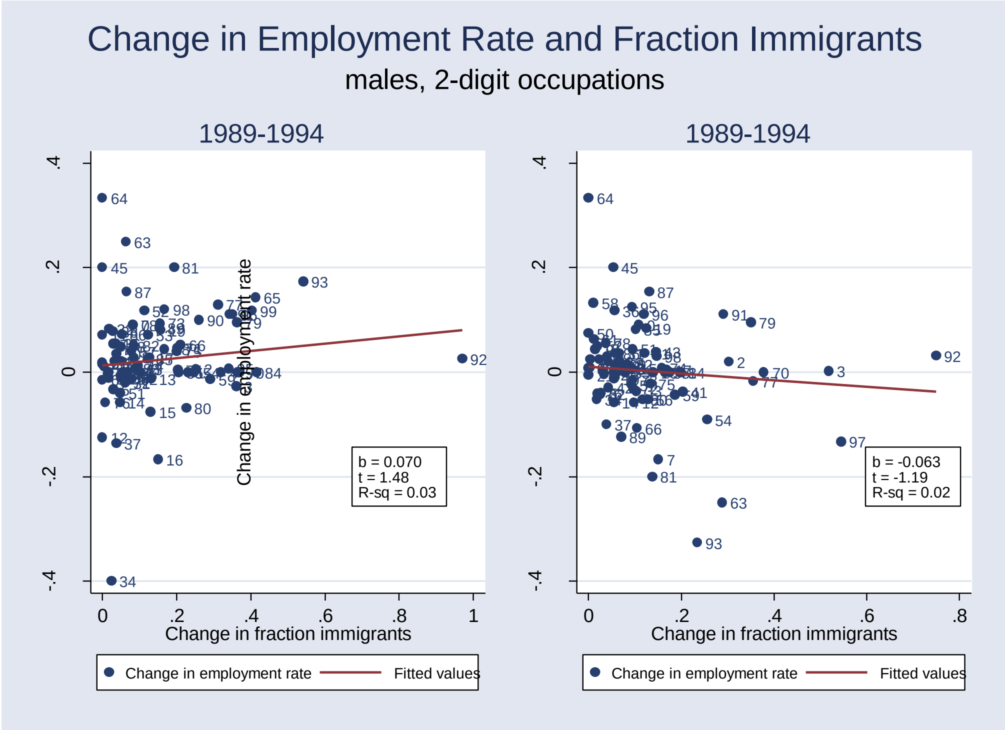

Figure 5: Change in Employment Rates and Fraction of 1989-1991 Immigrants Males, 2-digit occupations

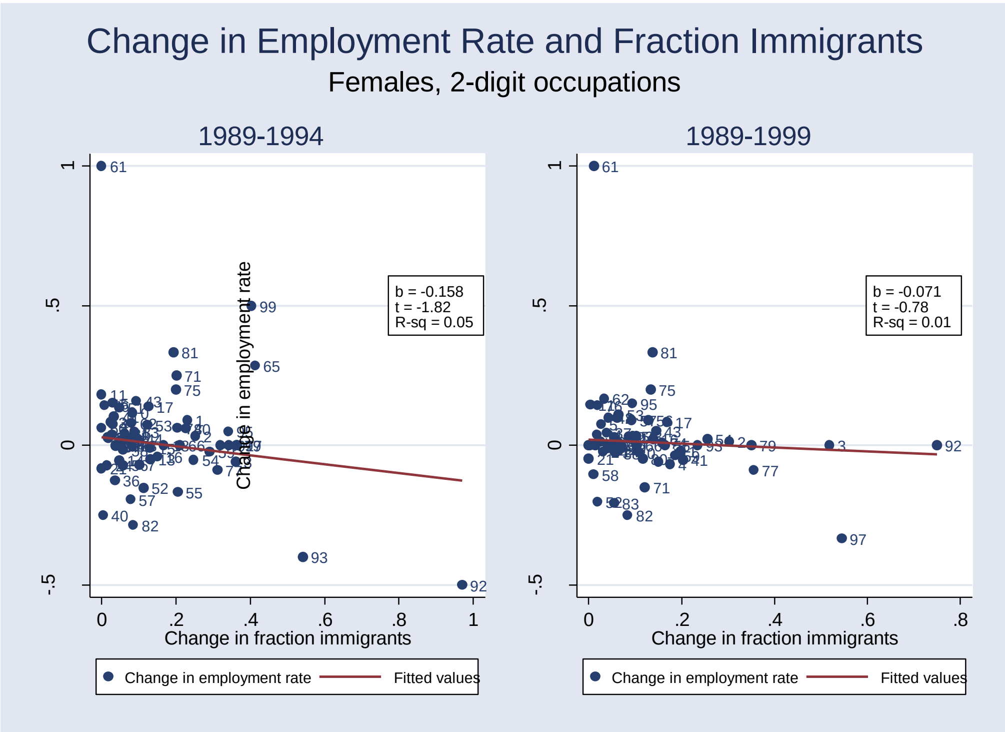

Figure 6: Change in Employment Rates and Fraction of 1989-1991 Immigrants Females, 2-digit occupations

Table 1: Occupational Distribution of Immigrants and Natives

Panel A: Males

	1989-1993		1994-1999	
	All immigrants	Natives	All immigrants	Natives
Academic and Scient. Professionals	12.02	9.41	12.94	10.16
Other Professionals and Technicians	8.26	10.92	10.02	11.87
Managers	0.40	8.61	1.74	9.60
Clerical Workers	1.56	9.14	3.34	9.19
Sales Workers	2.34	9.75	3.45	9.68
Service Workers	14.17	8.16	13.52	9.48
Skilled Agricultural Workers	3.47	4.60	5.03	4.20
Skilled Industry Workers	44.12	36.03	41.88	33.46
Unskilled Workers	13.66	3.39	8.09	2.34

Panel B: Females

	1989-1993		1994-1999	
	All immigrants	Natives	All immigrants	Natives
Academic and Scient. Professionals	9.03	9.49	10.39	10.04
Other Professionals and Technicians	14.40	26.51	13.79	25.68
Managers	0.42	2.49	0.70	3.59
Clerical Workers	7.82	27.62	9.90	30.09
Sales Workers	4.99	7.71	6.37	7.47
Service Workers	36.75	18.17	36.30	17.48
Skilled Agricultural Workers	2.24	1.27	3.79	1.21
Skilled Industry Workers	15.37	5.42	13.83	3.74
Unskilled Workers	8.97	1.32	4.94	0.70

Source: Authors’ calculations from the Israeli Labor Force Survey

Table 2: Occupational Distribution of Immigrants and Natives, by Level of Schooling and Years in Israel

	Immigrants, less than 2 years in Israel		Immigrants, 3-5 years in Israel		Immigrants, 6-10 years in Israel		Natives	
	High school or less	More than high school	High school or less	More than high school	High school or less	More than high school	High school or less	More than high school
Panel A: Males								
Academic and Scient. Professionals	0.0	13.5	0.5	18.5	0.5	22.8	0.3	27.2
Other Professionals and Technicians	3.2	9.7	1.8	14.1	3.0	16.7	4.8	23.1
Managers	0.5	1.0	0.4	1.7	0.8	3.3	6.2	15.6
Clerical Workers	0.9	2.4	2.2	3.3	3.3	4.3	9.6	8.7
Sales Workers	2.3	2.5	4.3	3.2	5.0	3.9	10.9	7.2
Service Workers	17.3	14.2	16.5	12.7	13.4	10.2	11.0	4.3
Skilled Agric. Workers	5.9	4.3	7.0	2.9	8.1	2.4	5.9	2.4
Skilled Industry Workers	53.5	41.3	55.0	37.2	55.9	31.5	47.1	11.1
Unskilled Workers	16.4	11.1	12.4	6.6	10.1	4.9	4.2	0.5
Panel A: Females								
Academic and Scient. Professionals	0.0	8.7	0.3	12.6	0.0	15.6	0.3	19.8
Other Professionals and Technicians	2.2	13.3	7.0	17.5	5.9	23.1	7.5	45.4
Managers	0.0	0.6	0.0	0.4	0.7	0.8	1.6	4.5
Clerical Workers	4.1	7.4	5.2	11.4	9.2	16.9	38.3	18.5
Sales Workers	2.5	5.0	7.0	6.8	9.5	6.8	10.2	4.4
Service Workers	59.5	42.5	44.1	30.1	42.8	21.8	30.2	5.1
Skilled Agric. Workers	5.1	2.8	6.2	3.1	5.9	2.5	2.0	0.7
Skilled Industry Workers	17.7	12.7	21.4	13.3	19.7	10.0	8.1	1.5
Unskilled Workers	8.9	7.0	9.0	4.8	6.3	2.6	1.8	0.3

Source: Authors' calculations from the Israeli Labor Force Survey

Table 3: Immigrant Ratio by Labor Market Segmentation

Segmentation	Number of distinct cells	Average number of observations per cell	Mean	Std.dev.	Minimum value	Maximum value
Two Digit Occupation	85	113	0.1439	0.2715	0	4.4117
District of Residence × Occupation	63	151	0.1247	0.1440	0	1.0997
Industry × Occupation	80	122	0.1413	0.4136	0	10.7266
Schooling × Experience	38	342	0.1593	0.2201	0	1.9092
Adjusted Schooling × Experience	40	321	0.0888	0.2670	0	34.6249

Immigrants' share in cell j at time t is defined as the number of immigrants in cell j at time t divided by total employment in cell j in 1989. The number of immigrants and total employment in the cell are calculated using sampling weights.
The summary statistics in all segmentations are calculated across all cells and all periods, and are weighted by the number of natives employed in the segment.

Table 4: Summary Statistics of the Natives Sample

	Males	Females
Employed (% of the population)	80.24	57.30
Education	11.87	11.59
Experience	24.51	23.03
Percentage married	82.45	80.30
Number of children aged 0-4	0.437	0.447
Number of children aged 5-14	0.896	0.995
Number of children aged 15-17	0.275	0.296
Percentage of Non-Jews	13.89	13.73
Origin Asia-Africa*	44.57	44.14
Origin Europe-America*	32.85	33.11
Percentage foreign born	37.82	36.08
Years in Israel (foreign born)	33.67	31.72
Employed in public sector (%)	17.83	46.95
Total number of natives in LFS sample	58,485	59,263
Hourly wage (2000 NIS)**	44.99	36.44
Log hourly wage (2000 NIS)**	3.55	3.36
Total Number of natives in Income Survey sample	40,372	42,437
Total Number of natives in Income Survey sample with non missing wage data	25,190	20,007

Source: Authors' calculations from the Israeli Labor Force and Income surveys, 1989-1999.
* Origin of respondent or respondent's father.
** In 2000, 1 US$ = 4.07 NIS.

Table 5: The Effect of Immigration on Native Wages: Constant and Dynamic Effects

	Constant Effect		Dynamic Effect			
	(1)	(2)	(3)		(4)	
			λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel A: Males						
Two Digit Occupation	-0.0103 [-0.69]	0.0959 [-0.10]	-0.3088 [-4.98]	0.0679 [5.05]	-0.2031 [-2.90]	0.0393 [3.89]
District of Residence × Occupation	-0.4426 [-11.21]	-0.1844 [-3.46]	-0.8766 [-9.13]	0.1087 [4.56]	-0.2741 [-2.56]	0.0205 [0.95]
Industry × Occupation	-0.0542 [-4.73]	-0.0151 [-0.67]	-0.3236 [-4.75]	0.0631 [4.41]	-0.1616 [-3.47]	0.0309 [3.34]
Schooling × Experience	0.4081 [13.39]	0.0843 [2.05]	0.6282 [8.35]	-0.0442 [-3.10]	0.1226 [1.28]	-0.0065 [-0.43]
Adjusted Schooling × Experience	0.0646 [2.70]	-0.0313 [-1.04]	-0.1574 [-3.72]	0.0543 [5.56]	-0.0930 [-1.39]	0.0127 [1.09]
Cell Fixed Effects	No	Yes	No		Yes	

	Constant Effect		Dynamic Effect			
	(1)	(2)	(3)		(4)	
			λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel B: Females						
Two Digit Occupation	-0.2334 [-7.61]	-0.1152 [-2.61]	-0.7152 [-5.80]	0.1131 [4.69]	-0.2836 [-3.61]	0.0352 [2.40]
District of Residence × Occupation	-0.6074 [-10.72]	-0.2771 [-4.10]	-1.5501 [-11.78]	0.2347 [7.49]	-0.5669 [-3.94]	0.0651 [2.45]
Industry × Occupation	-0.0742 [-5.14]	-0.0676 [-2.68]	-0.3223 [-3.93]	0.0594 [3.33]	-0.1249 [-2.06]	0.0118 [1.07]
Schooling × Experience	0.5046 [14.74]	0.1152 [2.61]	0.9198 [10.84]	-0.0792 [-5.35]	0.1598 [1.52]	-0.0068 [-0.44]
Adjusted Schooling × Experience	0.0416 [1.36]	-0.0948 [-2.68]	-0.3292 [-6.21]	0.0870 [7.98]	-0.2344 [-3.04]	0.0266 [1.98]
Cell Fixed Effects	No	Yes	No		Yes	

Dependent variable: log hourly wages. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate regressions. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level. Sample sizes: around 24,200 for males; around 19,300 for females.

The sample is an extract from the 1989-1999 Israeli Income Survey, and includes all natives and pre-1989 immigrants for whom data on occupation is non-missing. The male sample is restricted to ages 25 to 65, the female sample is restricted to ages 25 to 60. All regressions include the following variables: total employment in the segment, an index of labor demand for workers in the segment (see text for details, education, experience, experience squared; a dummy for married; dummies for the number of children between 0 and 4, between 5 and 14, and between 15 and 17; a dummy for non-Jews; dummies for ethnic origin Asia-Africa and ethnic origin Europe-America-Oceania (third generation Israelis are the omitted category); a dummy for foreign born status and years since immigration (zero for natives); a full set of calendar quarter dummies. Observations with missing data were deleted.

Table 6: The Effect of Immigration on Native Employment: Constant and Dynamic Effects

	Constant Effect		Dynamic Effect			
	(1)	(2)	(3)		(4)	
			λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel A: Males						
Two Digit Occupation	-0.0114 [-2.50]	0.0098 [0.95]	-0.0602 [-3.83]	0.0112 [3.42]	0.0230 [1.11]	-0.0027 [-0.83]
District of Residence × Occupation	-0.0514 [-5.67]	-0.0056 [-0.37]	-0.1070 [-4.22]	0.0138 [2.56]	-0.0366 [-1.20]	0.0069 [1.19]
Industry Occupation	-0.0053 [-1.78]	0.0093 [0.90]	-0.0418 [-2.48]	0.0084 [2.28]	0.0169 [0.88]	-0.0016 [-0.52]
Schooling × Experience	-0.0072 [-1.07]	-0.0037 [-0.36]	-0.0561 [-3.27]	0.0097 [3.21]	-0.0628 [-2.91]	0.0100 [3.09]
Adjusted Schooling × Experience	0.0037 [0.74]	0.0073 [1.09]	0.0143 [1.19]	-0.0024 [-1.09]	0.0168 [0.99]	-0.0020 [-0.68]
Cell fixed effects	No	Yes	No		Yes	

	Constant Effect		Dynamic Effect			
	(1)	(2)	(3)		(4)	
			λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel B: Females						
Two Digit Occupation	-0.0382 [-4.25]	0.0666 [3.76]	-0.1123 [-3.55]	0.0174 [2.49]	0.0869 [2.07]	-0.0044 [-0.56]
District of Residence × Occupation	-0.0736 [-4.60]	0.0331 [1.24]	-0.2042 [-5.34]	0.0318 [3.90]	0.0739 [1.43]	-0.0090 [-0.97]
Industry × Occupation	-0.0053 [-1.11]	0.0268 [1.79]	-0.0649 [-2.53]	0.0136 [2.52]	0.0255 [1.05]	0.0003 [0.06]
Schooling × Experience	0.0185 [1.93]	-0.0159 [-1.11]	0.0455 [1.72]	-0.0051 [-1.11]	-0.0452 [-1.29]	0.0047 [0.93]
Adjusted Schooling × Experience	-0.0041 [-0.38]	0.0144 [0.95]	-0.0218 [-0.99]	0.0040 [1.15]	0.0369 [0.94]	-0.0043 [-0.75]
Cell fixed effects	No	Yes	No		Yes	

Dependent variable: 1 if employed, 0 otherwise. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate linear probability models. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level. Sample sizes: around 47,000 for males; around 35,500 for females.

The sample is an extract from the 1989-1999 Israeli Labor Force Survey, and includes all natives and pre-1989 immigrants in their first LFS interview for whom data on occupation is non-missing. The male sample is restricted to ages 25 to 65, the female sample is restricted to ages 25 to 60. All regressions include the following variables: total employment in the segment, an index of labor demand for workers in the segment (see text for details, education, experience, experience squared; a dummy for married; dummies for the number of children between 0 and 4, between 5 and 14, and between 15 and 17; a dummy for non-Jews; dummies for ethnic origin Asia-Africa and ethnic origin Europe-America-Oceania (third generation Israelis are the omitted category); a dummy for foreign born status and years since immigration (zero for natives); a full set of calendar quarter dummies. Observations with missing data were deleted.

Table 7: Immigrants' Short-Run and Long-Run Effects on Natives' Wage: Robustness Checks

	(1) Autocorrelation Robust Standard Errors		(2) Adding Cohort Dummies		(3) Cell Fixed Effects Interacted with Linear Time Trend		(4) Cell Fixed Effects and One-digit Interactions with a Full Set of Year Dummies	
	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel A: Males								
Two Digit Occupation	-0.2031 [-2.92]	0.0393 [4.25]	-0.1910 [-2.69]	0.0407 [3.88]	-0.1068 [-1.14]	0.0138 [0.64]	-0.1024 [-1.39]	0.0301 [2.86]
District of Residence × Occupation	-0.2741 [-1.90]	0.0205 [0.89]	-0.3217 [-2.88]	0.0152 [0.69]	-0.2555 [-2.30]	0.0230 [0.80]	0.0243 [0.16]	-0.0134 [-0.40]
Industry × Occupation	-0.1616 [-1.95]	0.0309 [2.31]	-0.1438 [-2.84]	0.0354 [3.58]	-0.1234 [-2.59]	0.0265 [2.25]	-0.1579 [-3.14]	0.0391 [3.64]
Adjusted Schooling × Experience	-0.0930 [-1.75]	0.0127 [1.39]	-0.1078 [-1.62]	0.0147 [1.26]	-0.1063 [-1.39]	0.0141 [0.96]	-0.1216 [-1.55]	0.0195 [1.34]
Panel B: Females								
Two Digit Occupation	-0.2836 [-2.22]	0.0352 [1.49]	-0.2146 [-2.42]	0.0426 [2.79]	-0.1234 [-1.56]	0.0303 [1.44]	-0.2515 [-2.48]	0.0528 [2.88]
District of Residence × Occupation	-0.5669 [-3.76]	0.0651 [2.12]	-0.4832 [-3.20]	0.0690 [2.55]	-0.3060 [-1.84]	0.0453 [1.37]	-0.1539 [-0.73]	0.0055 [0.13]
Industry × Occupation	-0.1249 [-2.23]	0.0118 [1.63]	-0.0898 [-1.37]	0.0191 [1.40]	-0.0680 [-1.25]	0.0075 [0.46]	0.1029 [1.14]	-0.0208 [-1.12]
Adjusted Schooling × Experience	-0.2344 [-4.10]	0.0266 [2.88]	-0.2363 [-2.86]	0.0268 [1.91]	-0.1413 [-1.45]	-0.0034 [-0.19]	-0.0313 [-0.31]	0.0088 [0.47]

Dependent variable: log hourly wages. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate regressions. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level. Sample sizes: around 24,200 for males; around 19,300 for females. For sample selection rules and the full set of explanatory variables, see notes to Table 5.

Table 8: Piecewise Constant Effects of Immigration on Native Wages

	(1) Cell Fixed Effects				(2) Cell Fixed Effects Interacted with Linear Trend			
	Effect at 0 years	Effect at 1-3 years	Effect at 4-6 years	Effect at 7-10 years	Effect at 0 years	Effect at 1-3 years	Effect at 4-6 years	Effects at 7-10 years
Panel A: Males								
Two Digit Occupation	-0.2861 [-1.33]	-0.1791 [-2.67]	0.0471 [0.82]	0.1086 [2.12]	-0.2612 [-1.10]	-0.1296 [-2.00]	0.0099 [0.17]	-0.0221 [-0.19]
District of Residence × Occupation	0.1477 [0.44]	-0.3157 [-3.32]	-0.1257 [-1.29]	-0.1015 [-0.87]	0.0218 [0.06]	-0.3153 [-3.08]	-0.0140 [-0.11]	-0.0417 [-0.22]
Industry × Occupation	-0.7284 [-2.987	-0.1400 [-2.94]	0.0479 [1.49]	0.0976 [2.31]	-0.6876 [-2.87]	-0.1077 [-2.21]	0.0428 [1.28]	0.0270 [0.41]
Adjusted Schooling × Experience	-0.0929 [-0.32]	-0.0875 [-1.62]	0.0163 [0.30]	0.0022 [0.04]	0.2640 [0.73]	-0.0904 [-1.70]	0.0085 [0.14]	0.0097 [0.01]
Panel B: Females								
Two Digit Occupation	-0.2322 [-0.71]	-0.3105 [-3.49]	-0.0046 [-0.06]	-0.0164 [-0.21]	-0.1627 [-0.48]	-0.1712 [-1.78]	0.1374 [1.18]	0.0947 [0.59]
District of Residence × Occupation	-0.0316 [-0.05]	-0.5051 [-4.22]	-0.2382 [-2.02]	-0.0066 [-0.05]	-0.1611 [-0.28]	-0.2896 [-2.01]	0.0041 [0.02]	0.0562 [0.20]
Industry × Occupation	0.6562 [1.41]	-0.1022 [-1.35]	-0.0935 [-1.42]	-0.0189 [-0.46]	0.5756 [1.19]	-0.0398 [-0.43]	-0.0358 [-0.42]	0.0476 [0.40]
Adjusted Schooling × Experience	0.1242 [0.48]	-0.1659 [-2.43]	-0.1488 [-1.96]	0.0052 0.90]	0.1623 [0.59]	-0.1747 [-2.26]	-0.1479 [-1.65]	-0.1545 [-1.85]

Dependent variable: log hourly wages. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate regressions. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level. Sample sizes: around 24,200 for males; around 19,300 for females. For sample selection rules and the full set of explanatory variables, see notes to Table 5.

Table 9: The Dynamic Effect of Immigration on Native Wages: Blue-Collar versus White-Collar Occupations

	(1) Cell Fixed Effects				(2) Cell Fixed Effects Interacted with Linear Trend			
	Blue-collar		White Collar		Blue collar		White Collar	
	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time	λ_0: Initial Effect	λ_1: Change over time
Panel A: Males								
Two Digit Occupation	-0.1689 [-2.28]	0.0332 [3.22]	0.0180 [0.08]	0.0193 [0.55]	-0.1242 [-1.23]	0.0166 [0.71]	0.1337 [0.54]	0.0105 [0.24]
Residence × Occupation	-0.2036 [-1.70]	0.0246 [0.95]	0.1086 [0.30]	-0.0190 [-0.37]	-0.3324 [-2.74]	0.0400 [1.16]	0.0548 [0.15]	-0.0153 [-0.26]
Industry × Occupation	-0.1217 [-2.70]	0.0245 [2.61]	-0.1858 [-1.07]	0.0603 [2.54]	-0.1273 [-2.39]	0.0276 [2.13]	-0.0869 [-0.51]	0.0293 [0.78]
Adjusted Schooling × Experience	-0.0084 [-0.09]	-0.0142 [-0.74]	0.0801 [0.43]	-0.0018 [-0.07]	-0.1011 [-1.07]	0.0094 [0.42]	0.1499 [0.75]	-0.0114 [-0.44]
Panel B: Females								
Two Digit Occupation	-0.1982 [-2.51]	0.0191 [1.24]	-0.1504 [-0.45]	0.0503 [1.03]	-0.1246 [-1.55]	0.0194 [0.84]	-0.0986 [-0.29]	0.0395 [0.66]
Residence × Occupation	-0.3940 [-2.45]	0.0338 [1.07]	0.0152 [0.04]	0.0018 [0.03]	-0.4095 [-2.26]	0.0555 [1.39]	0.0236 [0.06]	-0.0122 [-0.19]
Industry × Occupation	-0.0611 [-1.04]	0.0018 [0.16]	-0.4753 [-1.30]	0.0564 [0.89]	-0.0516 [-0.93]	0.0089 [0.51]	-0.4672 [-1.18]	0.0288 [0.35]
Adjusted Schooling × Experience	-0.2295 [-1.91]	0.0239 [0.85]	-0.2681 [-1.85]	0.0249 [1.14]	-0.0626 [-0.38]	-0.0139 [-0.38]	-0.2822 [-1.97]	0.0072 [0.32]

Dependent variable: log hourly wage. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate linear regressions. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level. Sample sizes: blue-collar males, 16300; white-collar males, 7900; blue-collar females, 12100; white-collar females, 7200. For sample selection rules and the full set of explanatory variables, see notes to Table 5.

Table 10: The effect of Immigration on Occupational Mobility: Constant and Dynamic Effects

		3 months change		12 months change	
		Constant effect	Dynamic effect	Constant effect	Dynamic Effect
Panel A: Males					
Two Digit Occupation	Initial effect	-0.0005 [-0.03]	-0.0275 [-0.77]	0.0220 [0.91]	0.0372 [0.78]
	Change over time	-	0.0053 [0.89]	-	-0.0035 [-0.40]
Industry × Occupation	Initial effect	-0.0282 [-2.13]	-0.0483 [-1.46]	-0.0139 [-0.79]	-0.0312 [-0.73]
	Change over time	-	0.0039 [0.60]	-	0.0036 [0.53]
Cell fixed effects		Yes	Yes	Yes	Yes
Number of observations		53645		32183	

		3 months change		12 months change	
		Constant effect	Dynamic effect	Constant effect	Dynamic effect
Panel B: Females					
Two Digit Occupation	Initial effect	-0.0006 [-0.03]	-0.0008 [-0.02]	-0.0375 [-1.33]	-0.0699 [-1.00]
	Change over time	-	0.00004 [0.00]	-	0.0075 [0.57]
Industry × Occupation	Initial effect	0.0009 [0.04]	-0.0070 [-0.21]	-0.090 [-2.76]	-0.1204 [-2.13]
	Change over time	-	0.0016 [0.25]	-	0.0072 [0.77]
Cell fixed effects		Yes	Yes	Yes	Yes
Number of observations		41009		23480	

Dependent variable: 1 if individual switched between labor market cells between interview 1 and 2, 0 otherwise.. Entries in the table represent the parameter estimate and t-statistics (in brackets) for the coefficient on the fraction immigrants in a labor market segment from separate regressions. Standard errors are robust to general heteroskedasticity and clustering at the segment-calendar quarter level.

Table 11: Replication of Friedberg (2001), Table III

A: Friedberg's results, individual level data. Standard errors clustered at the occupation× year level.

Dependent variable	Estimation method	Independent variable *p*	Independent variable *r*	R^2	N
ln w	OLS		-0.324 (0.086)	.53	8353
r	OLS	0.188 (0.029)		.76	8353
ln w	OLS	0.135 (0.057)		.53	8353
ln w	2SLS		0.718 (0.343)		8353
	First- Stage F statistic		42.25		

B: Two-step procedure (Donald-Lang)

Dependent variable	Estimation method	Independent variable *p*	Independent variable *r*	R^2	N
ln w	OLS		-0.342 (0.221)	0.997	166
r	OLS	0.216 (0.102)		0.682	166
ln w	OLS	0.290 (0.197)		0.996	166
ln w	2SLS		1.339 (0.912)	0.725	166
	First- Stage F statistic		4.54		

C: Grouped data (weighted by cell size)

Dependent variable	Estimation method	Independent variable *p*	Independent variable *r*	R^2	N
ln w	OLS		-0.266 (0.281)	0.997	166
r	OLS	-0.066 (0.088)		0.964	166
ln w	OLS	0.037 (0.164)		0.996	166
ln w	2SLS		-0.571 (1.084)	0.997	166
	First- Stage F statistic		0.56		

Note: Each row in the table presents estimates from separate regressions. See text and Friedberg (2001) for the definition of *r* and *p*. All regressions control for a time dummy, a piecewise linear function of years of schooling, a quadratic in experience, full time status, gender, ethnicity, nativity, years since migration (0 for Israeli born), and 9 one-digit industry dummies. The regressions in panels A-C also include an interaction between the time dummy and all the control variables. In panels B-C, the unit of observation is an occupation×year cell. In panel D the unit of observation is an occupation cell. In panels C and D, regressions are weighted by cell size. Individual-level data are from pooled IS 1989 and 1994; occupation level data are from LFS 1994; occupation in FSU data are from IES 1992 and 1993.

Table 11, continued:

D: Replication, grouped data, first differenced equation

Dependent variable	Estimation method	Independent variable *p*	*r*	R^2	N
ln w	OLS		-0.271 (0.150)	0.446	83
r	OLS	0.174 (0.060)		0.494	83
ln w	OLS	0.038 (0.069)		0.405	83
ln w	2SLS		0.219 (0.343)	0.308	83
	First- Stage F statistic		8.45		

Note: Each row in the table presents estimates from separate regressions. See text and Friedberg (2001) for the definition of *r* and *p*. All regressions control for a time dummy, a piecewise linear function of years of schooling, a quadratic in experience, full time status, gender, ethnicity, nativity, years since migration (0 for Israeli born), and 9 one-digit industry dummies. The regressions in panels A-C also include an interaction between the time dummy and all the control variables. In panels B-C, the unit of observation is an occupation×year cell. In panel D the unit of observation is an occupation cell. In panels C and D, regressions are weighted by cell size. Individual-level data are from pooled IS 1989 and 1994; occupation level data are from LFS 1994; occupation in FSU data are from IES 1992 and 1993.

Appendix Table B1: Calculating Effective Years of Experience for Immigrants

	Males		Females	
	Natives	Immigrants	Natives	Immigrants
Regression coefficients:				
FSU experience	-	0.00732	-	0.00511
FSU experience squared	-	-0.00038	-	-0.00053
Experience in Israel	0.0633	0.1129	0.03343	0.09888
Experience in Israel squared	-0.00093	-0.00352	-0.00049	-0.00245
Mean value of :				
FSU experience	-	17.91	-	15.79
Israel experience	23.02	3.94	20.83	4.29
Marginal value of an additional year of experience for immigrants				
FSU experience	-	-0.00615	-	-0.01153
Israel experience	-	0.08519	-	0.07789
Marginal value of an additional year of experience for natives (evaluated at mean of immigrant experience)	0.02270	-	0.01385	-
Effective value of experience for immigrants				
FSU experience	-	-0.27115	-	-0.83280
Israel experience	-	3.75382	-	5.62363

Authors' calculations based on Israeli Income Survey data. See Appendix B for details of the calculations.

Appendix Table C1: Calculating the Effective Schooling of immigrants

Panel A: Males

		Immigrants with 0-2 years in Israel				Immigrants with 3-5 years in Israel				Immigrants with 6-10 years in Israel			
		Immigrants in schooling category:				Immigrants in schooling category:				Immigrants in schooling category:			
		High school dropout	High school	Some college	College or more	High school dropout	High school	Some college	College or more	High school dropout	High school	Some college	College or more
Probability of being equivalent to native in schooling category:	Less than high school	0.9655	1	0.9702	0.6972	0.9870	0.9999	0.8295	0.5429	0.9056	0.9999	0.7711	0.3428
	High school	0	0	0	0	0	0.0001	0.0003	0	0	0.0001	0.1276	0
	Some college	0	0	0.0001	0	0	0	0.1702	0	0	0	0.1014	0
	College or more	0.0345	0	0.0298	0.3027	0.0130	0	0	0.457	0.0944	0	0	0.6571

Panel B: Females

		Immigrants with 0-2 years in Israel				Immigrants with 3-5 years in Israel				Immigrants with 6-10 years in Israel			
		Immigrants in schooling category:				Immigrants in schooling category:				Immigrants in schooling category:			
		High school dropout	High school	Some college	College or more	High school dropout	High school	Some college	College or more	High school dropout	High school	Some college	College or more
Probability of being equivalent to native in schooling category:	Less than high school	1	1	0.8945	0.8472	1	0.7310	0.4754	0.2908	1	0.4353	0.3276	0.0453
	High school	0	0	0	0	0	0.0231	0.1160	0.1704	0	0.2931	0.1042	0.2860
	Some college	0	0	0.0125	0	0	0.0275	0.1912	0	0	0.0185	0.3768	0
	College or more	0	0	0.0929	0.1528	0	0.2184	0.2174	0.5388	0	0.2531	0.1814	0.6687

Authors’ calculation from Israeli Income Survey and Labor Force Survey data. See Appendix C for details.