Immigration and Prices

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This paper examines the behavior of prices following the unexpected arrival of a large number of immigrants from the former Soviet Union (FSU) to Israel during 1990. I use store-level price data on 915 consumer price index products to show that the increase in aggregate demand prompted by the arrival of the FSU immigration significantly reduced prices during 1990. When one controls for native population size and city and month effects, a one-percentage-point increase in the ratio of immigrants to natives in a city decreases prices by 0.5 percentage point on average. It is argued that this negative immigration effect is consistent with FSU immigrants—the new consumers—having higher price elasticities and lower search costs than the native population. Thus immigration can have a moderating effect on inflation through its direct effect on product markets, and not only by increasing the supply of labor.

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I. Introduction

Since the end of 1989, and until 1995, a large inflow of immigrants from the former Soviet Union (FSU) arrived in Israel, with monthly immigration flows increasing exponentially during 1990 (see fig. 1 below). At the end of 1990, FSU immigrants represented 4 percent of the population. Immigrants are not only workers but also consumers of goods and services, and their arrival must have increased aggregate demand. This paper examines the effect of the arrival of FSU immigrants to Israel on the level of prices in 1990.

In general, one would not expect this effect to be large because immigration flows are usually small relative to the size of the native population and are also predictable. This study therefore focuses on 1990, the first year of the FSU immigration, in hope that the initially unexpected nature of the FSU immigration wave, its large size, and the fast pace at which it occurred combined in such a way as to leave measurable traces in the price data.

I use monthly store-level price data for 915 products sold in over 1,800 retail stores located in 52 cities in Israel during 1990 to estimate the relationship between prices and immigrants. More precisely, I relate price changes in a city to changes in its population composition—the ratio of FSU immigrants to natives—controlling for native population size, product-specific inflation rates, and month effects. The immigrants/natives ratio was essentially zero in all cities in October 1989—before the start of the immigration wave—but by December 31, 1990, it ranged between 0 and 17 percent across cities (col. 6 of table 1 below). This significant variation in the spatial distribution of FSU immigrants is used to identify the effect of immigration on prices.[1] I show that the estimated immigration effect combines a *size effect* due to the unexpected increase in the number of consumers and a *composition effect* due to the arrival of consumers with different demand characteristics.

The main empirical finding is that, contrary to the predictions of the standard perfectly competitive model, the arrival of immigrants into a city chosen at random had a significant negative effect on prices. A one-percentage-point increase in the ratio of immigrants to natives decreases prices by 0.5 percentage point. This estimate implies that, when the size of the native population and all other factors are held constant, prices in a city with the average immigrants/natives ratio should be lower by 2.6 percent in December 1990 compared to the case in which no immigrants settled in the city. This is a large and significant effect that is

[1] A before-after comparison is not feasible because disaggregated price data are not available before January 1990. Comparing prices within 1990 is problematic, not only because immigration was already in full swing, but mainly because there are no monthly immigration data by city to match the monthly price data.

robust to changes in the assumptions underlying the empirical work.[2] The results also show that the size effect of immigration is not significantly different from zero so that the negative immigration effect is due to a negative composition effect.

A plausible explanation of the negative immigration effect is that the newly arrived immigrants were more price sensitive than the native population, possibly because of their lower income and lack of brand and store loyalties. Retailers then have an incentive to lower their markups in order to attract these new high-elasticity consumers, especially when brand and store attachments are initially weak and develop over time (Bils 1989). A complementary explanation is that the FSU immigrants searched more intensively than the native population for stores offering better bargains. This is a reasonable hypothesis because these immigrants were not familiar with features of modern market economies that affect shopping behavior, such as the sale of the same product at different prices in different stores—price dispersion—and the existence of a large variety of brands to choose from. Moreover, the FSU immigrants also faced relatively low costs of searching for information about stores and products because they were initially unemployed or did not immediately participate in the labor force. This should have induced them to check prices in additional stores, and, indeed, the available data confirm that FSU immigrants spent considerably more time shopping than their Israeli counterparts (even after one controls for demographics and income). This increase in search activity strengthens the competitive pressure on firms to lower their prices (Stahl 1989).

The short-run nature of the empirical analysis limits the extent to which the negative immigration effect can be explained by declines in retail costs. There is no strong empirical evidence of a negative effect of the FSU immigration on natives' wages and employment (Friedberg 2001; Cohen-Goldner and Paserman 2004), and, as mentioned above, the labor force participation of the recently arrived immigrants was very low. Moreover, separate analysis of the price effects of immigration by 40 product categories reveals that the immigration effect is significantly stronger in products for which FSU immigrants' share of the expenditure was larger, such as pork products and vodka. This result accords more with a demand-based explanation than with one based on depressed wages of retail workers.

The primary contribution of this paper is to show that the increase in aggregate demand prompted by the arrival of the FSU immigration was accompanied by a change in its composition in such a way that the FSU immigration had a significant negative effect on prices during 1990.

[2] As a reference point, the monthly price change averaged over all months, products, and stores in the sample was 0.74 percent.

It is the arrival of a mass of new consumers with high price elasticities and low search costs that can discipline stores to lower their prices. The relative importance of the high price elasticity and low search cost explanations, however, cannot be assessed with the current data because it is the same group of consumers who search more intensively for lower prices and are more price sensitive at the same time.

The results of this paper contribute to two empirical literatures. First, the evidence presented in this paper helps to explain recent empirical findings showing that, contrary to the standard perfectly competitive prediction, retail prices fall during periods of high demand (Warner and Barsky 1995; MacDonald 2000; Chevalier, Kashyap, and Rossi 2003). This empirical literature, however, is concerned with price fluctuations due to temporary shocks or to cycles in demand during weekends and holidays rather than with permanent shifts in demand. The message from the present paper is that when the increase in demand is accompanied by a change in the composition of consumers—consumers with high price elasticity or lower search costs during weekends and holidays—the overall effect on prices can indeed be negative (see also Nevo and Hatzitaskos 2005).

Second, empirical studies on the effect of search on the distribution of prices did not focus, until recently, on price-level effects.[3] This is somewhat surprising given that changes in the price level can have first-order impacts on consumer welfare. Recently, Brown and Goolsbee (2002) showed that the rapid diffusion of the Internet—which facilitates cheap and fast price comparisons across stores—leads to lower prices, as predicted by search models, whereas Sorensen (2000) showed that prices of drugs that are repeatedly purchased—and therefore more likely to benefit from search—exhibit significant reductions in both dispersion and price-cost margins relative to occasionally purchased drugs. If we interpret the ratio of immigrants to natives in a city as a proxy for the amount of search in the city, then the present paper could be seen as presenting additional evidence, based on a large number of products, that more search does indeed reduce prices.

The paper is organized as follows: Section II presents the price and immigration data and some preliminary, nonparametric evidence on the price-immigration relationship. Section III develops an econometric model whose results are described in Section IV. Also in Section IV, possible explanations of the estimated effects are discussed and evidence in support of a high-elasticity/search-based interpretation is presented. Conclusions in Section V close the paper.

[3] Most of the empirical search literature focused on describing the patterns of price dispersion and the suitability of search models in explaining the observed dispersion.

II. Description of the Data

A. *A Short-Run Analysis*

The empirical analysis focuses on the year 1990, the first year of the FSU immigration. As mentioned in the introduction, the 1990 wave of immigrants was unexpected and relatively large. Moreover, these immigrants were not very familiar with competitive markets nor with the peculiarities of the Israeli economy.[4] Subsequent waves of FSU immigrants—after the final dissolution of the Soviet Union in 1991—were more "westernized" and more informed about the Israeli economy. This suggests that immigrants' consumption behavior may have differed across cohorts in ways that cannot easily be controlled by cohort dummies. Moreover, the assimilation of immigrants into the local economy implies that their effect is weakened over time.[5] Focusing on the first year of the FSU immigration sharpens the distinction between the immigrants and the native population and therefore helps in identifying the effect caused by the arrival of new consumers on prices.

Another reason for the focus on 1990 is that we can safely ignore changes in natives' demand as a result of changes in income prompted by the FSU immigration.[6] This means that the estimated effects are more likely to reflect the change in the immigrant population than changes in natives' demand.

Finally, the focus on the short run implies that production/retail capacity could not have changed much during the period of analysis, nor could the number of stores. This limits the extent to which prices could have declined as a result of economies of scale, entry, or lower retail costs (see Sec. V for further analysis). Although these long-run effects may be substantial, the goal of this paper is to isolate the demand effects of immigration on prices, holding production technology and market structure fixed. Focusing on 1990 achieves this.

[4] The *glasnost* and *perestroika* reforms were introduced just a few years before (1985–87). Two characteristics of modern market economies—presumably absent in the Soviet economy—are particularly relevant to our analysis, namely, the large extent of product differentiation (in terms of manufacturers, brands, packaging, etc.) and the existence of price dispersion across stores.

[5] Indeed, Weiss, Sauer, and Gotlibovski (2003) show that FSU immigrants' wages rise sharply with time in Israel even though they do not fully catch up with the wages of comparable natives.

[6] Friedberg (2001) reports that natives' wages were not affected by the FSU immigration. Cohen-Goldner and Paserman (2004) show that natives' employment was not affected either, but they find a small negative effect on their wages in the short run. In 1990, no new taxes were imposed to finance the absorption of immigrants.

B. The Price Data

The price data consist of monthly price quotations obtained from retail stores and service providers by the Central Bureau of Statistics (CBS). These prices are used to compute the monthly consumer price index.[7] The CPI in 1990 included prices on 1,332 goods and services, but I exclude from the empirical analysis the prices of fruits and vegetables, which vary a lot across stores and, within a store, over the year, as well as the prices of services (including housing) because these are closely tied to their quality, which is usually unobserved. Moreover, prices of services are obtained telephonically, and this increases the uncertainty regarding what exactly the service is providing. Goods, on the other hand, are sampled through actual visits to retail stores. Most of the goods belong to the following broad categories: food, clothing, furniture, and appliances.

The ideal experiment would be to compare prices in 1990 to prices in 1989, before the FSU immigration started. Unfortunately, this is not possible because prices for the period before January 1990 are not available at the CBS in a way amenable to academic research. The empirical analysis is therefore based on 199,425 monthly price quotations on 915 products for the year 1990 obtained from 1,837 retail stores located in 52 cities in Israel. Table 1 presents the cities (sorted by population size) and the number of sampled stores in each of them (col. 1). In the large cities, over 200 stores are visited, and, as shown in column 2, most products are sampled (e.g., Tel Aviv has prices for 892 of the 915 products); in the small towns, only a few stores are visited, and just a fraction of the products are sampled (e.g., less than 10 percent of the products in Sederot). As a result, the number of total price observations by city in column 3 ranges from 363 in Sederot to 33,716 in Tel Aviv.[8]

Each product file has a store and city identifier, a nominal price quotation observed during the month, information on the manufacturer, brand, size, weight, and other relevant attributes of the product. Thus a "single" product file includes data on different varieties of the

[7] Importantly, the price data are not "scanner" data. The prices are therefore "asking" prices. For many products, asking and actual transaction prices are identical. The price data used by Lach and Tsiddon (1992, 1996) and Lach (2002) came from the same source—the CPI raw data collected by the CBS—but were limited to a restricted set of products and to different time periods.

[8] The number of price quotations (i.e., stores) per product in a given month also varies considerably. In general, the size of the sample for each product depends on its popularity. For example, tee shirts were sampled in about 110 stores per month on average, but electric drills were sampled in only four stores. About half the products have sample sizes of 17 stores or less, and just 10 percent of the products have price quotations in 37 or more stores. Another statistic of interest is the number of products sampled in each visit to a store. On average, 17 products are sampled in each monthly visit. Half the stores have fewer than 12 products sampled, whereas 10 percent of the stores have over 34 products with monthly price quotations.

TABLE 1
Summary Statistics by City in 1990

City	Stores[a] (1)	Products[a] (2)	Observations (3)	Total Population[b] (4)	Native Population Growth (%)[b] (5)	Immigrants/ Natives Ratio[b] (6)	Average Price Change (%)[c] (7)
Jerusalem	207	871	22,045	504.1	1.9	.021	.964
Tel Aviv	372	892	33,716	321.7	.5	.049	.824
Haifa	197	820	19,824	223.6	−.6	.106	.767
Holon	68	681	8,147	148.4	.7	.048	.886
Petah Tiqwa	63	615	8,352	135.4	1.0	.053	.715
Bat Yam	61	576	5,605	133.2	−.2	.062	.903
Rishon LeZiyyon	50	565	5,645	129.4	1.8	.058	.908
Netanya	65	652	7,601	120.3	.9	.089	.817
Ramat Gan	70	661	7,028	116.1	−.4	.034	.670
Beer Sheva	65	686	8,336	113.8	−.3	.075	.727
Bene Beraq	36	359	3,259	111.8	2.3	.020	1.140
Ashdod	26	346	2,799	76.6	2.1	.073	.612
Rehovot	38	480	4,172	73.8	.4	.084	1.079
Herzliyya	37	502	5,080	73.2	2.6	.028	.753
Ashqelon	29	445	3,845	56.8	1.2	.038	.463
Kefar Sava	28	379	3,985	56.5	3.3	.046	1.253
Ra'anana	21	373	2,769	50.9	2.1	.031	.260
Nazareth[d]	39	320	3,495	48.1	3.1	.000	1.083
Giv'atayim	37	398	2,928	45.6	−.4	.026	.779
Ramla	15	205	1,644	45.0	1.3	.050	.842
Hadera	18	316	2,047	42.2	2.1	.058	.675
Lod	13	233	1,835	41.6	1.0	.031	1.017
Akko	14	249	2,166	37.4	1.3	.063	−.125
Ramat Hasharon	19	297	1,973	36.3	1.1	.005	1.064
Qiryat Atta	12	235	1,962	36.0	1.7	.063	1.245
Qiryat Bialik	5	210	1,715	32.8	.0	.064	.024

Qiryat Yam	12	234	1,148	32.6	−.9	.111	.765
Tiberias	7	143	949	31.7	1.6	.037	.511
Nahariyya	19	371	2,139	30.6	1.6	.093	.390
Qiryat Motzkin	18	296	1,933	30.4	−.3	.069	.589
Qiryat Gat	16	288	2,068	27.7	1.8	.064	.907
Nazareth Illit	4	78	563	25.2	.4	.170	−.253
Afula	6	120	745	25.0	2.8	.086	1.016
Dimona	6	130	882	24.8	1.2	.036	.155
Hod HaSharon	5	104	635	24.8	2.0	.028	.908
Umm Alfahm[d]	5	79	509	24.6	3.2	.000	1.128
Qiryat Ono	7	111	674	22.2	.4	.036	.374
Karmiel	5	150	1,109	21.0	.9	.142	.501
Shefaram[d]	10	152	1,074	20.4	2.4	.000	1.208
Or Yehuda	8	141	789	20.2	.0	.084	.662
Nes Ziyyona	11	225	1,495	19.3	2.6	.051	.952
Zefat	7	175	1,189	16.6	7.5	.078	1.121
Pardes Hanna-Karkur	11	230	1,278	16.3	1.2	.024	1.208
Tamra[d]	10	169	1,102	15.9	3.1	.000	.380
Sakhnin[d]	7	100	727	15.8	3.1	.000	.283
Yehud	5	75	486	15.5	1.3	.032	.454
Migdal HaEmeq	7	169	856	15.3	2.6	.096	.671
Qiryat Shemona	16	243	1,725	15.3	1.9	.064	.206
Arad	12	197	1,616	13.8	2.2	.092	.652
Arrabe[d]	10	113	790	11.7	3.4	.000	1.151
Nesher	5	79	608	10.5	1.9	.065	.931
Sederot	3	68	363	9.7	1.0	.020	1.382
Total	1,837	n.r.	199,425	3,221	1.53[e]	.053[e]	.74[e]

Note.—n.r. = not relevant.

[a] Count of stores (products) appearing at least once in the sample during 1990.

[b] Population in thousands on December 31, 1989. Native population growth between December 31, 1989, and December 31, 1990. Immigrants/natives ratio on December 31, 1990.

[c] Price changes based on the longest time difference available for each store-product observation.

[d] Cities with Arab population only.

[e] Simple average.

product in terms of the product's attributes (manufacturer, brand, weight, packaging, etc.). This information was coded both numerically and in Hebrew. For example, hummus, a popular food product, exhibits up to nine variations in terms of the manufacturer, the weight of the product, and whether it includes tahini or not. Any attempt to "dummify" this information and use it to control for differences in price levels proved to be futile: there are too many attributes, different products have different sets of attributes, and the importance of a particular attribute varies with the product. Given the large number of products (and the even larger number of varieties), I essentially ignore this information and treat the products' attributes as unobserved. Because product attributes are important determinants of differences in price *levels*, I will focus the empirical analysis on price *changes* over time.

Analyzing prices changes presumes that the attributes of products do not change over the sample period. We can be assured that this is indeed the case because the CBS takes good care to base the computation of the monthly price changes on exactly the same variety of the product. For this reason, the CBS samples only one specific variety in each store. Once the variety is chosen, the CBS keeps sampling it irrespective of its volume of sales. If the same variety is not available in a given month, the price is assigned a missing value by the CBS surveyor, and a price change cannot be computed for that variety in that month. This ensures that observed monthly price changes refer to the same variety of the product. After observing three consecutive missing values, the CBS stops sampling that particular variety. In the fourth month, the CBS declares the product to be not available at the store and switches to the "closest" substitute available in subsequent months. In the sample, the incidence of store-product observations with three or more consecutive missing prices (not occurring at the beginning or at the end of the observed data spell) is less than 1 percent. This means that the variety observed at the end of the data spell is the same as the one at the beginning in at least 99 percent of the store-product observations. This is important because the empirical analysis will be based on the price difference between the last and first months the product is observed in the sample (for the same store). In short, the way the data are collected ensures that the quality (e.g., variety) of the product is fixed over the sample period and precludes, in particular, recording a price decrease if a store switches to a cheaper variety, perhaps because of the arrival of immigrants.

Table 2 shows the distribution of stores' durations during 1990. Duration is computed for each store-product observation in the sample.[9]

[9] In a store in which two (or more) products are sampled, one product may be sampled from January to May and the other from July to December. This store generates two (or

TABLE 2
DISTRIBUTION OF STORE-PRODUCT DURATIONS

Duration (Months)	Frequency	Percentage	Cumulative
1	795	2.4	2.4
2	985	2.9	5.3
3	1,103	3.3	8.6
4	2,838	8.5	17.0
5	9,805	29.2	46.2
6	4,975	14.8	61.1
7	3,065	9.1	70.2
8	8,369	24.9	95.1
9	1,644	4.9	100.0
Total	33,579		

NOTE.—Durations are computed for each store-product observation.

There are 33,579 store-product observations, but only 32,784 can be used to compute price changes. Durations are short: half the store-product observations appear during less than five months and no store-product observation has price quotations for more than nine months.[10]

C. The Immigration Data

The first FSU immigrants started to arrive in Israel during the last months of 1989 as a direct consequence of the political developments in the Soviet Union. Even though the process leading to the collapse of the Soviet Union in 1991 was set in motion years before, the massive immigration caught Israel completely unawares. The monthly inflow of FSU immigrants grew from about 1,500 in October 1989 to about 35,000 in December 1990. Figure 1 shows the monthly flow of immigrants during 1989–95. During the period October 1989–December 1990, about 192,000 FSU immigrants arrived in Israel, and their share in the total population reached 4 percent by the end of 1990. The immigration process continued during the first half of the 1990s but at a decreasing rate. During 1991, 145,000 immigrants arrived in Israel, but during 1992–95, the yearly inflow was around 65,000.

Immigrants did not settle uniformly over the country. As seen in table

more) durations of five and six months, even though the store is in the sample during 11 months.

[10] The reason for the short durations is a technical one: the CBS moved gradually from using large mainframes to using personal computers during the year 1990. As a result, not all the data in the mainframe were transferred to the PCs. This is particularly true for the first half of 1990, the initial phase of the transition. In the second half of 1990, when the transition was almost complete, 97.5 percent of the durations in the stores appearing in the sample for the first time in July 1990 are at their maximum (six months). As long as the reasons for the missing data are random, this should not affect the estimation results.

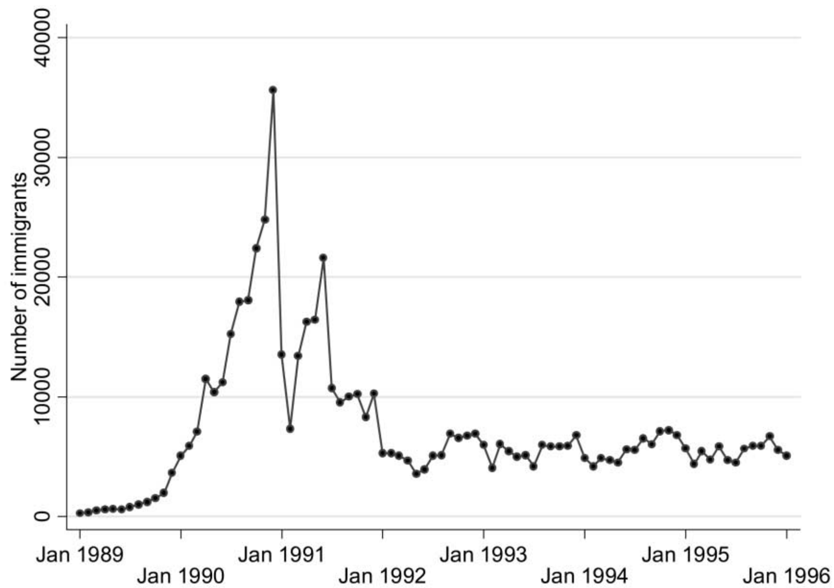

FIG. 1.—Monthly flow of FSU immigrants to Israel

1 (col. 6), there is a large variation in the ratio of immigrants to natives across cities (on December 31, 1990). There are many locations (e.g., Arab towns) in which there are no immigrants at all, whereas in other cities (e.g., Karmiel, Nazareth Illit, Qiriat Yam), the immigrants/natives ratio is over 11 percent at the end of 1990. The simple average of the immigration ratios across cities is 0.053 and the standard deviation is 0.036. This variation will be used to identify the relationship between prices and immigration.

An important limitation of the immigration and population data is that we do not have city-level data for every month of the year. Population data by city are available only for December 31, 1989 and 1990, whereas data on FSU immigrants are available only for December 31, 1990. We will return to this issue in Section IV.

D. *Preliminary Data Analysis*

The average monthly price change of a product j in store i in city c is computed as the difference in log price between the last (t_{1ij}) and first (t_{0ij}) months the store-product is observed in the sample divided by the duration in the sample,

$$\Delta \log p_{jic} = \frac{\log p_{jict_{1ij}} - \log p_{jict_{0ij}}}{t_{1ij} - t_{0ij}}.$$

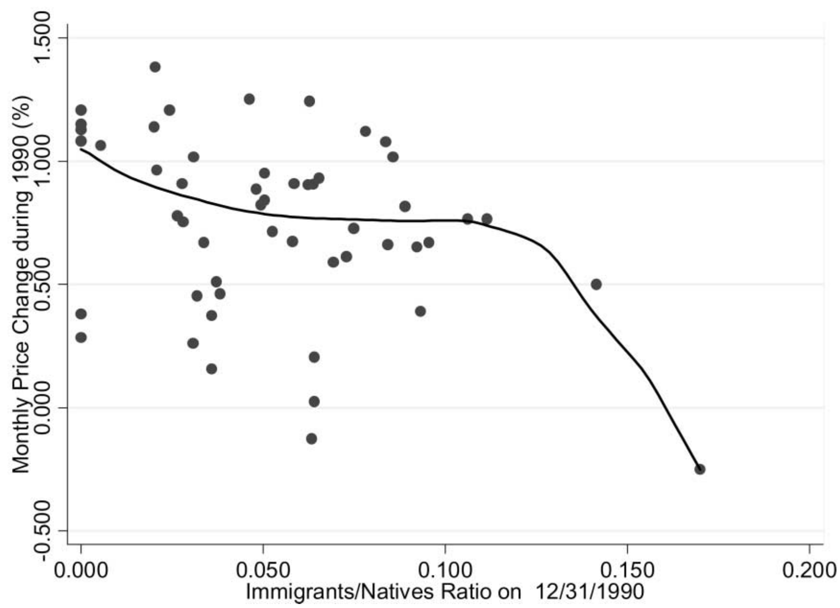

FIG. 2.—Price changes and immigrants/natives ratio

I then average all these price changes over all products and all stores in the city, and this is the number appearing in column 7 of table 1.[11] The simple average of the monthly price changes is 0.74 percent, but there is large variation across cities ranging from 1.4 percent in Sederot to −0.25 percent in Nazareth Illit (the standard deviation across cities is 0.37 percentage point).

The price change and immigration ratios are plotted in figure 2. A fitted regression line has a negative slope (−3.4) that is precisely estimated (robust standard error 1.5). I also use all the data at the store-product level to estimate the expectation of the monthly price change conditional on the immigrants/natives ratio. I estimate this expectation nonparametrically using Fan's (1992) locally weighted regression smoother (the solid line in fig. 2). The conditional expectation also shows an inverse relationship between the average monthly price change and the ratio of immigrants to natives.

These results suggest that prices increase less rapidly in cities in which the ratio of immigrants to natives is larger. But changes in population

[11] Computing the change between the average price in December 1990 and the average price level in January 1990 involves comparing prices from different stores since no store-product observation is in the sample for more than nine months (see table 2). This may introduce a bias because the price averages are based on prices of products that may differ between the two months.

size, differential inflation rates among products, and the potential endogeneity of the ratio of immigrants to natives due to the self-selection of immigrants into cities are ignored. In order to assess how these preliminary results fare when these omissions are corrected and to get a quantitative assessment of the estimated relationship and its precision, I proceed to a regression-based analysis of the data.

III. Econometric Specification

The econometric model aims at estimating the effect of the arrival of immigrants into a city on the prices of products sold in that city. The following specification for the nominal price of product j ($j = 1, \ldots, 915$) sold by store i in city c ($c = 1, \ldots, 52$) during month t ($t = 1, \ldots, 12$) is used:

$$\log p_{jict} = \mu_j + \mu_t + \pi_j t + \mu_i + \mu_c + \delta_j \left(\frac{I_{ct}}{N_{ct}}\right) + \beta_j \log (I_{ct} + N_{ct}) + u_{jict}, \quad (1)$$

where I_{ct} and N_{ct} are, respectively, the number of FSU immigrants and natives in city c in month t, the μ's are fixed effects, and u_{jict} is a shock to price in month t. The parameters of interest are allowed to vary across products j. Total population in city c in month t equals $I_{ct} + N_{ct}$.[12]

This specification emphasizes that immigration has potentially two types of effects on aggregate demand and, therefore, on prices. As immigrants flow into a city, the number of consumers increases, and this may affect prices. This *size effect* is captured by the coefficient on total population size, $I + N$. Immigrants and natives are therefore treated symmetrically, and β_j reflects the effect of an increase in the *count* of consumers. But immigrants' socioeconomic characteristics (tastes, income, etc.) usually differ from those of the native population, and prices may respond differently to an increase in the number of consumers resulting from an increase in the number of immigrants than to one caused by an increase in the number of natives. This *composition effect* is captured by the coefficient on the ratio of immigrants to natives, I/N.

Another way in which immigrants and natives differ is that changes in I during 1990 were unexpected whereas changes in N were likely to be anticipated. The former may increase retail costs and push prices up if stores are already operating at or near full capacity, whereas changes in the native population may have a smaller effect on prices, if at all, in the short run. This suggests that the size effect may differ

[12] The relevant market for each product is implicitly defined as the city in which the store selling the product is located, but in the empirical section, I show that the results are robust to alternative market definitions.

between natives and immigrants because of the unexpected nature of the immigrants' arrival. Modifying (1) to

$$\log p_{jict} = \mu_j + \mu_t + \pi_j t + \mu_i + \mu_c + \delta_j\left(\frac{I_{ct}}{N_{ct}}\right) + \beta_j \log\left[(1+\theta_j)I_{ct} + N_{ct}\right] + u_{jict} \tag{2}$$

allows for this possibility.

In order to isolate the total effect of the FSU immigration on prices, equation (2) is rewritten as

$$\log p_{jict} \approx \mu_j + \mu_t + \pi_j t + \mu_i + \mu_c + \lambda_j R_{ct} + \beta_j \log N_{ct} + u_{jict}, \tag{3}$$

where $R_{ct} \equiv I_{ct}/N_{ct}$ and $\lambda_j \equiv \delta_j + \beta_j(1+\theta_j)$.[13]

Equation (3) is taken to the data in order to estimate λ_j, the coefficient of R_{ct}. This coefficient represents the total effect of the arrival of immigrants into a city on the price of product j, with the size of the native population and the various fixed effects held constant. This effect is composed of a composition and a size effect. The goal of this paper is to estimate this total immigration effect rather than to disentangle its different components.[14]

Note that controlling for native population size is important for identification of the immigration effect because of the possible selection of immigrants into large cities. City size is an important determinant of an immigrant's location decision since larger cities usually offer better job opportunities and other amenities. On the other hand, larger cities may have more competitive markets.[15] Failing to control for native population size in (3) may therefore bias the estimate of λ_j downward.

The terms μ_j and μ_t are product and month dummies that capture, respectively, permanent differences in price levels among different products and common time trends in prices. The price effects of the various religious holidays are also picked up by the month dummies. The term $\pi_j t$ allows for different inflation rates across products but assumes that

[13] I use the approximation

$$\log\left[(1+\theta)I_{ct} + N_{ct}\right] = \log\{N_{ct}[(1+\theta)R_{ct} + 1]\} \approx \log N_{ct} + (1+\theta)R_{ct},$$

which is appropriate for the values of R in the sample and not too large values of θ.

[14] One can always recover $\lambda_j - \beta_j = \delta_j + \beta_j\theta_j$ and redefine this as a composition effect reflecting both the different socioeconomic characteristics of the immigrants and the unexpected nature of their arrival. Separate identification of δ_j, β_j, and θ_j requires nonlinear estimation, which is not warranted here because of the small number of observations over which the main regressors vary, namely, the 52 cities in the sample.

[15] The simple correlation between the number of FSU immigrants and the native population across the 52 cities in the sample is 0.75. About 17,000 immigrants settled on average in each of the three largest cities (Jerusalem, Tel Aviv, and Haifa) compared to an average of 1,400 in the 35 smallest ones. Recent empirical evidence finds that larger markets accommodate a larger number of firms and have lower markups and prices (Barron, Taylor, and Umbeck 2004; Campbell 2005; Campbell and Hopenhayn 2005).

they are constant over time.[16] The term μ_i captures the effect of time-invariant features of the store that bear on prices, such as location within the city, type of store, quality of service, and so forth. The city-specific effect, μ_c, picks up features of the city's demographic and economic characteristics and citywide amenities (presence of a shopping mall, pedestrian district, etc.) that may affect prices and are fixed during the sample period. The various fixed effects allow for a rich pattern of correlations among prices. Equation (3) should be understood as a reduced-form equation showing the equilibrium price determined by the values of N, I, and the various fixed effects.

Estimation of the parameters in the price-level equation (3) is problematic for two reasons. First, as described in Section II.*B*, product j includes many different variations of the product (brand, size, packaging, etc.) that affect prices. It is impractical to dummify all the variations within product j or their different attributes. Because these attributes may be related to characteristics of the city that also correlate with the arrival of immigrants, ordinary least squares (OLS) estimates may suffer from an omitted variable bias. One may suspect that immigrants are inclined to consume cheaper variations of a given product. If locations with cheaper variations attract more immigrants, then the OLS estimates of the λ_j's will reflect this selection effect. In order to control for differences across cities in the variety of products consumed, I examine *changes* in prices over time. As discussed in Section II.*B*, price changes over time are almost always computed for exactly the same variation of the product.[17] The unobserved characteristics of each variation are differenced out, as well as the store and city effects. Note that any systematic difference in price changes (i.e., in inflation) between products is captured by the product-specific dummies π_j. Thus I exploit the panel structure of the data to get rid of time-invariant store, city, and product characteristics.

Time-differencing, however, does not address the second problem, which is the lack of monthly data on R_{ct} and N_{ct}. The only time-differencing for which we have immigrants data is a 12-month difference, that is, the December 1990–December 1989 difference, assuming that the number of immigrants is zero on December 31, 1989, in all cities ($R_{c0} = 0$). This, however, is not feasible because I do not have price

[16] A completely unrestricted specification is theoretically plausible but difficult to implement because it requires the additional estimation of about 10,000 parameters corresponding to the 11 monthly inflation rates for the 915 products.

[17] In fact, the CPI is often criticized for not being able to pick up substitution effects in a timely fashion. Although usually not a desirable property, this "delay" is a good thing for the purposes of this paper. Indeed, during 1990, there was no change in the baskets of goods constituting the CPI nor in the sample of stores. Even if such changes occurred, they could not have been motivated by the FSU immigration because of the lack of timely expenditure data.

data for December 1989. I therefore compute the longest possible difference for each store-product observation. Moving to "long differences" has the additional advantage that it removes part of the month-to-month noise in price changes caused by getting in and out of sales and other promotions.

As in Section II.*D*, let t_{0ij} and t_{1ij} be the first and the last months a store *i*, product *j* observation is observed in the sample. I compute an average monthly percentage price change during this period. For example, a store appearing for the first time in April 1990 and for the last time in November 1990 will have $t_{0ij} = 4$ and $t_{1ij} = 11$. Recall that the median duration is five months (see table 2). "Long-differencing" equation (3) and dividing by $t_{1ij} - t_{0ij}$ gives

$$\frac{\log p_{jict_{1ij}} - \log p_{jict_{0ij}}}{t_{1ij} - t_{0ij}} = \frac{\mu_{t_{1ij}} - \mu_{t_{0ij}}}{t_{1ij} - t_{0ij}} + \pi_j + \lambda_j \frac{R_{ct_{1ij}} - R_{ct_{0ij}}}{t_{1ij} - t_{0ij}}$$
$$+ \beta_j \frac{\log N_{ct_{1ij}} - \log N_{ct_{0ij}}}{t_{1ij} - t_{0ij}} + \Delta u_{jic}, \qquad (4)$$

where

$$\Delta u_{jic} = \frac{u_{jict_{1ij}} - u_{jict_{0ij}}}{t_{1ij} - t_{0ij}}.$$

Notice that each store-product combination has a *single* observation representing its average monthly price change. The dependent variable, averaged over all stores and products in each city, is the average monthly price change appearing in column 7 of table 1.

The key regressors in (4), the average monthly change in R and the average monthly percentage change in the native population, are unobserved, and I measure them as follows. It can be safely assumed that R was zero or very close to it in all cities on December 31, 1989 (see fig. 1). For the remaining months I assume that R grew linearly from zero to the value on December 31, 1990, denoted by R_{c12}. That is,

$$R_{ct} = t\frac{R_{c12}}{12} \qquad (5)$$

for each month $t = 1, 2, \ldots, 12$. Since the monthly increment in R is $R_{c12}/12$, I use $R_{c12}/12$ in place of $(R_{ct_{1ij}} - R_{ct_{0ij}})/(t_{1ij} - t_{0ij})$ in equation (4).

For the native population I assume a constant monthly growth rate,

$$N_{ct} = N_{c0}(1 + g_c)^t, \qquad (6)$$

where N_{c12} and N_{c0} are the native population levels at the end of December 1990 and 1989, respectively. This implies that the growth rate g_c is approximately equal to $(\log N_{c12} - \log N_{c0})/12$, and I use this instead of $(\log N_{ct_{1ij}} - \log N_{ct_{0ij}})/(t_{1ij} - t_{0ij})$ in equation (4).

A few remarks on regression (4) are in order. First, because the key regressors are measured at the city level, we cannot separately identify the effect of immigration on prices from the effect of city-specific price trends. In Israel, however, because of its small geographic size and high degree of economic integration, it is very unlikely that prices exhibit significant city-specific trends over prolonged periods of time. Second, identification of the parameters relies on the cross-city variation in R_{c12} and $\log N_{c12} - \log N_{c0}$. In particular, all cities started with $R_{c0} = 0$ but evolved differently in terms of their absorption of immigrants. Thus the monthly change in R differs across cities. This cross-sectional variation is used to identify the effect of R on prices. Third, $(\mu_{t_{1ij}} - \mu_{t_{0ij}})/(t_{1ij} - t_{0ij})$ is the average monthly inflation rate between t_{0ij} and t_{1ij}. Since this varies across store-products, I dummify the t_1's and t_0's and enter them separately in the regression after multiplying them by the inverse sample duration, $1/(t_{1ij} - t_{0ij})$.[18] Finally, because there are 915 products, I adopt a random coefficients formulation and focus on estimating the mean effects λ and β in

$$\lambda_j = \lambda + \eta_j^\lambda,$$
$$\beta_j = \beta + \eta_j^\beta. \tag{7}$$

I assume

$$E(\eta_j^\lambda | x_{jc}) = E(\eta_j^\beta | x_{jc}) = 0,$$
$$E(\eta_j^\lambda \eta_k^\lambda | x_{jc}, x_{kc}) = E(\eta_j^\beta \eta_k^\beta | x_{jc}, x_{kc}) = 0 \quad \text{for } j \neq k, \tag{8}$$

where $x_{jc} = (R_{c12}, N_{c0}, N_{c12}, t_{1j}, t_{0j},$ product and month dummies).[19]

Assumption (8) implies that there is no relationship between the size of the native population and immigrants in a city and the magnitude of the price response to changes in them. A specific product could be more responsive to immigrants because of product-specific attributes that match the immigrants' tastes, but this matching is unrelated to the native population size and to the ratio of immigrants in the city. I also assume that the random components of the coefficients are uncorre-

[18] Notice that the months affected by long-differencing depend on the store's price availability, i.e., on t_{0ij} and t_{1ij}. Because the panel is very much unbalanced in terms of the months in which each store-product observation appears in the sample, there is a lot of variation in the length of the time difference $t_{1ij} - t_{0ij}$ as well as on the specific months over which the price change is computed.

[19] In terms of the original parameters in (2) we would have $\lambda = \delta + \beta(1 + \theta)$ and $\eta_j^\lambda = \eta_j^\delta + \eta_j^\beta(1 + \theta + \eta_j^\theta) + \beta\eta_j^\theta$, using obvious notation.

lated across products. All the correlation across store-products is captured by the store, city, and month effects.

On the basis of (5), (6), and (7), the estimated equation becomes

$$\frac{\log p_{jict_{1ij}} - \log p_{jict_{0ij}}}{t_{1ij} - t_{0ij}} = \frac{\mu_{t_{1ij}} - \mu_{t_{0ij}}}{t_{1ij} - t_{0ij}} + \pi_j + \lambda \frac{R_{c12}}{12} + \beta \frac{\log N_{c12} - \log N_{c0}}{12} + \Delta u_{jic} + \frac{\eta_j^{\lambda}(R_{c12}/12) + \eta_j^{\beta}[(\log N_{c12} - \log N_{c0})/12]}{t_{1ij} - t_{0ij}} + \text{measurement error.} \qquad (9)$$

Equation (9) is estimated by OLS after all store-product observations are pooled. Assumptions (8) suffice for

$$\frac{\eta_j^{\lambda}(R_{c12}/12) + \eta_j^{\beta}[(\log N_{c12} - \log N_{c0})/12]}{t_{1ij} - t_{0ij}}$$

to be uncorrelated with R_{c12} and $\log N_{c12} - \log N_{c0}$. In order for Δu_{jic} to be uncorrelated with the regressors, it suffices to assume that u_{jict} is mean-independent of $(R_{c12}, N_{c0}, N_{c12})$, conditional on the various fixed effects.

This last identifying assumption implies that store-product-specific price shocks are not related to the inflow of immigrants to the city and to its population size. This is a reasonable assumption if one believes that immigrants do not decide where to settle on the basis of the occurrence of store-specific sales. This does not mean that immigrants do not choose to settle in cities with lower or higher than average prices.[20] In fact, the economic and demographic characteristics of the city, picked up by the city fixed effect and the size of the native population, may affect the decision of immigrants to settle in a particular city. For example, FSU immigrants may be attracted to cities with lots of amenities, and these cities may be associated with higher than average prices. Importantly, no assumptions are made on the correlation between the city effects and the city's native population and ratio of immigrants to natives. In fact, no assumptions are made on the correlation between R or N and any of the "fixed effects" affecting the price level.

The measurement error component of the disturbance in (9) reflects the error-ridden measurement of the growth in R and in N. In general,

[20] The vast majority of FSU immigrants were not directed to settle in specific locations as, e.g., with the Ethiopian immigration wave in 1991. FSU immigrants went through the "direct absorption" method in which they received a subsidy and had to rent an apartment in the private market in a location of their choice.

long-difference equations have the advantage of reducing biases due to measurement errors in the regressors. This is particularly true in our case: when $t_1 = 12$ and $t_0 = 1$, the regressors in (4) would be measured without errors (given $R = 0$ on December 31, 1989). The longer the time difference, the closer the regressors match the available data and measurement errors are less severe.

The remaining issue is the estimator's covariance matrix. First, because of the presence of product, store, and city effects in the price-level equation, the price shock u_{jict}, as a first approximation, could be treated as uncorrelated across products, stores, and cities. Second, because the data used to estimate equation (9) are *single* observations per store-product combination, I make no assumptions on the serial correlation in u_{jict}. Third, notice that the random coefficient assumption (7) induces a correlation among prices of the same product across stores. Thus I allow for arbitrary correlation of the disturbance in equation (9) across prices of the same product (in different stores), but assume zero correlation between prices of different products. Practically, this requires clustering the standard errors at the product level. Finally, if there is a time-varying, city-level component in u_{jict} (e.g., weather), then Δu_{jic} will be correlated within cities. Not accounting for this correlation tends to bias the estimated standard errors downward, particularly when the regressors are highly correlated within cities, which is the case here. Clustering at the city level solves this problem.

IV. Empirical Results and Explanations

A. *Empirical Results*

Table 3 presents the estimates of the parameters in equation (9) at two different levels of aggregation. The most disaggregated sample, in columns 1 and 2, consists of 32,784 store-product observations with durations larger than one month. In column 1, I omit native population from the regression and obtain a significant negative estimate of the immigration effect using either type of standard error.[21] A one-percentage-point increase in the ratio of immigrants in a city decreases prices by 0.39 percent on average. This is a first hint that immigration matters for prices. Adding the native's rate of growth to the regression in column 2 lowers the estimated immigration effect to −0.28, but it

[21] In the regressions estimated with store-product observations the standard error in parentheses is clustered at the product level whereas the one in brackets is clustered at the city level. Throughout the text, the first reported standard error or p-value in parentheses is based on standard errors clustered at the product level, whereas the second one is based on standard errors clustered at the city level.

TABLE 3
IMMIGRATION EFFECTS ON PRICES, EQUATION (9)
DEPENDENT VARIABLE: AVERAGE MONTHLY DIFFERENCE IN LOG PRICE

	AGGREGATION LEVEL: STORE-PRODUCT LEVEL		AGGREGATION LEVEL: CITY LEVEL							
			Weighted				Unweighted			
					Adjusted Price Changes				Adjusted Price Changes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change in immigrants/natives ratio	−.388 (.089)*** [.102]***	−.279 (.108)*** [.121]**	−.333 (.156)**	−.309 (.157)*	−.315 (.159)*	−.286 (.154)*	−.555 (.158)***	−.476 (.162)***	−.578 (.162)***	−.496 (.160)***
Growth rate in native population		.573 (.287)** [.338]*		.151 (.498)		.178 (.488)		.697 (.553)		.719 (.544)
p-value of F-test for zero coefficients		(<.01) [<.01]		.13		.17		<.01		<.01
Product dummies	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
R^2	.08	.08	.34	.34	.54	.54	.46	.48	.54	.56
Observations	32,784	32,784	52	52	52	52	52	52	52	52

NOTE.—In cols. 1 and 2, standard errors in parentheses are clustered at the product level and those in brackets at the city level, using 915 and 52 clusters, respectively. Standard errors in cols. 3–10 are robust to heteroskedasticity. In cols. 3–6, cities are weighted by the number of observations in each city. In cols. 5, 6, 9, and 10, the city average of the product dummies estimated in col. 2 is subtracted from the dependent variable. All regressions include dummies for t_1 and t_0 interacted with $(t_1 - t_0)^{-1}$.

* Significantly different from zero at the 10 percent significance level.

** Significantly different from zero at the 5 percent significance level.

*** Significantly different from zero at the 1 percent significance level.

still remains significantly different from zero (p-values 0.01 and 0.025).[22] The estimated size effect, β, is 0.57 but is borderline significant (p-values 0.046 and 0.096). The third row indicates that both parameter estimates are jointly significantly different from zero.[23]

As mentioned in Section III, the immigration effect is identified by variation in R_{c12} across cities. Because R_{c12} is at the city level, it is appropriate to match its cross-city variation to variations in city-average price changes. We do this by averaging equation (9) to the city level, eliminating the variation in price changes across stores and products within a city. Aggregating up from store-level data ensures that changes in the average price per city are not due to changes in the identity of the stores or the products over time. The number of observations is reduced to the 52 cities. The coefficients are now essentially estimated by regressing a "between long-differenced" regression: the monthly price change averaged across all stores and products in each city is regressed on the immigrants' ratio and native population growth in the city (and on the averages of the interactions between the dummies for the t_1's and the t_0's and $1/[t_{1ij} - t_{0ij}]$).[24]

Columns 3–6 present the estimates from this regression when each city is weighted by the number of observations in each city. At the city level, the product-specific error terms η_j^{λ} and η_j^{β} are averaged out so that standard errors are robust to heteroskedasticity only. The estimated immigration effects in columns 3 and 4 are very close to the estimates in columns 1 and 2 but, as expected, are less precisely estimated. The estimate of λ in column 4 is borderline significant with a p-value of 0.06.[25] The similarity of the parameter estimates between the two aggregation levels emphasizes that identification of the immigration effect is obtained from the cross-city variation in the ratio of immigrants to natives, R, or, in other words, from the spatial differences in the pace at which immigrants settled in Israel.

The set of products sampled in a city varies across cities (table 1). Therefore, averaging the product dummies π_j across stores and products

[22] Although the simple correlation between I and N is positive (see n. 15), the correlation between R and the growth rate in the native population is negative (-0.29) but marginally significant (p-value 0.04).

[23] The difference between the estimated coefficient of R and that of $\log N$ provides an estimate of $\delta + \beta\theta$. In col. 2, this difference is -0.85 (standard errors 0.24 and 0.31) and is significant at the 1 percent level under either type of standard error.

[24] Wooldridge (2003) points out that using the between regression gives the appropriate standard errors and uses the correct t-distribution for inference (assuming normality and homoskedasticity of the errors). When the number of clusters is small, an inference obtained from the between regression can be very conservative if there is no cluster effect in the u_{jict}'s, which is a strong possibility in the present application.

[25] The value of the t-statistic is 1.97, which has a p-value of 0.049 under the standard normal distribution and a p-value of 0.058 under the t-distribution with 29 degrees of freedom.

in a city gives a different intercept for each city, but this was ignored in regressions (3) and (4). Because city dummies cannot be estimated for each city, I average the estimated product dummies, the $\hat{\pi}_j$'s from column 2, across all stores and products in each city and subtract these averages from the dependent variable. I then regress this adjusted average price change on the usual regressors. Results appear in columns 5 and 6. The estimates are quite similar to those in columns 3 and 4, suggesting that a possible correlation between the set of products sampled and the regressors is not a major problem in these data.[26]

The estimates of β in columns 4 and 6 are considerably lower than the estimate in column 2 and are less precisely estimated, becoming not significantly different from zero. This, and the large increase in the standard error of $\hat{\lambda}$, leads to the nonrejection of the null joint hypothesis of zero coefficients ($\lambda = \beta = 0$) at conventional significance levels.

Weighting the city-level observations by the number of observations in the city gives very large weights to the three largest cities in Israel. The share of observations corresponding to these cities—Jerusalem, Tel Aviv, and Haifa—is 38 percent, whereas the smallest 35 cities in the sample—those with a population less than 50,000—constitute only 24 percent of the data. If the parameters differ across cities, then the estimates in columns 1–6 disproportionately reflect the parameters of the largest cities.

To explore this issue further, I added to equation (9) interaction terms between a dummy for the 35 smallest cities and, respectively, $R_{c12}/12$ and $(\log N_{c12} - \log N_{c0})/12$. The results (not reported) indicate that only the interaction with $R_{c12}/12$ is significant. The implied estimate of λ for the smallest 35 cities increases to -0.498 (standard error 0.115) and that of the larger cities is reduced to -0.133 (standard error 0.127). This suggests that the effect of immigration on prices is stronger in the smallest cities.[27] If this is indeed correct, the weighting procedure used in columns 3–6 would underestimate the immigration effect for a city chosen at random.

Given the small number of cities in the sample, it is not practical to arbitrarily group cities into size classes and allow λ to vary across classes. I can, however, give equal weight to each city in order to make the estimates more representative. The unweighted, or equal-weights, estimates can be interpreted as the effect of the arrival of immigrants into a city chosen at random, whereas the weighted estimates represent the effect of the arrival of immigrants chosen at random. Because the goal

[26] An alternative is to add the averaged $\hat{\pi}_j$'s as a regressor. This gives similar estimates of the coefficients of R and native population growth. For example, the estimates for col. 6 are -0.290 (0.160) and 0.173 (0.490), respectively, where the standard errors are not adjusted for the use of the generated regressor.

[27] This would be expected if larger cities have more competitive markets. See n. 15.

of this paper is to find out what happens to prices as immigrants settle in a typical city, the unweighted estimates are more appropriate for this purpose. The unweighted estimates of λ in columns 7–10 are indeed higher (in absolute value) than the weighted ones, reflecting the stronger immigration effect in the smaller cities, and are significant at the 1 percent level. The estimates of β increased relative to the weighted estimates but are very imprecisely estimated; the null hypothesis that there are no size effects cannot be rejected.

To allay concerns about the endogeneity of R in equation (9), table 4 presents two-stage least-squares estimates of λ and β. The instruments are the ratio of Soviet Union immigrants to natives in 1983 and a socioeconomic index for the city in 1983. Local immigrant networks are important factors in the immigrant's location decision, and past immigration patterns are related to the existence and extent of such networks (Munshi 2003). Card and Lewis (2005), for example, used historical immigration rates to instrument for current immigration flows. The city's socioeconomic index is a summary statistic of about 15 variables representing demographic, economic, and other social characteristics and can also be an important consideration in the location decision.[28] These two instruments are strong predictors of the immigration share in 1990. In the first-stage regression, both instruments have a positive and significant effect on the 1990 share of FSU immigrants. The instruments are not weak as evidenced by the high values of the F-statistics in the first-stage regression except, perhaps, in the weighted regressions. The overidentification tests do not reject the null hypothesis of zero correlation between the instruments and the disturbance, albeit somewhat marginally in the unweighted regressions. The instrumental variable estimates of λ are always stronger (more negative) than the OLS estimates, but this difference is not statistically significant as evidenced by the results of the Hausman tests for endogeneity of R. This means that the use of price *changes* is effectively coping with the possible endogenous selection of immigrants into cities.

In the remainder of this section I examine the robustness of the estimated immigration effect to changes in some of the working assumptions.[29] I start by examining the robustness of the results to changes in the definition of the market. I implicitly defined the boundaries of the relevant markets as those given by the city limits. This is unduly restrictive because intercity distances in Israel are not large and immi-

[28] The instruments are taken from the 1983 Census of Population, which is the latest census year prior to 1990. The socioeconomic index increases with the socioeconomic status of the city; its computation is described in Central Bureau of Statistics (1987).

[29] I usually report the store-product level and the unweighted city-level estimates. The weighted city-level regression results are also robust to the same changes in assumptions, and these results are available on request.

TABLE 4

Two-Stage Least-Squares Estimates of Immigration Effects on Prices, Equation (9)
Dependent Variable: Average Monthly Difference in Log Price

	Aggregation Level: Store-Product Level (1)	Aggregation Level: City Level			
		Weighted		Unweighted	
		(2)	Adjusted Price Changes (3)	(4)	Adjusted Price Changes (5)
Change in immigrants/natives ratio	−.484	−.627	−.577	−.561	−.587
	(.199)**	(.292)**	(.272)**	(.162)***	(.167)***
	[.201]**				
Growth rate in native population	.332	−.293	−.230	.593	.608
	(.357)	(.734)	(.713)	(.565)	(.572)
	[.517]				
p-value of *F*-test for zero coefficients	<.01	.04	.04	<.01	<.01
F-test for instruments in first-stage regression	14.3	7.21	7.21	23.7	23.7
p-value of overidentification test	.58	.37	.41	.05	.09
p-value of endogeneity test	.20	.18	.21	.66	.70
Product dummies	Yes	No	Yes	No	Yes
R^2	.08	.29	.51	.48	.55
Observations	32,784	52	52	52	52

Note.—The instrumental variables are the ratio of Soviet immigrants to natives in 1983 and a 1983 socioeconomic index by city. In col. 1, standard errors in parentheses are clustered at the product level and those in brackets at the city level, using 915 and 52 clusters, respectively. Standard errors in cols. 2–5 are robust to heteroskedasticity. In cols. 2 and 3, cities are weighted by the number of observations in each city. In cols. 3 and 5 the city average of the product dummies estimated in col. 1 is subtracted from the dependent variable. In col. 1, diagnostic statistics are based on errors clustered at the city level. All regression include dummies for t_1 and t_0 interacted with $(t_1 - t_0)^{-1}$.

* Significantly different from zero at the 10 percent significance level.

** Significantly different from zero at the 5 percent significance level.

*** Significantly different from zero at the 1 percent significance level.

grants in one city can easily make their weekly purchases in a nearby city. It follows that the change in demand in a city should also take into account the immigrants in the city's surrounding area. As a first approximation, I divided the 52 cities into four metropolitan areas: Jerusalem (one city), the northern area (22 cities), the central area (23 cities), and the southern area (six cities). For a given city, I computed the ratio of immigrants to natives in the metropolitan area in which the city is located (excluding the given city). This new variable (divided by 12) is added to the regressors in equation (9), and the results appear in table 5. The estimates of the metropolitan ratio of immigrants to natives are always negative and large but are not significantly different from zero, maybe because there is not much cross-city variation in this regressor. Notice that the addition of this regressor does not affect the estimates of the own ratio and of the native population, which remain similar to those in table 3.[30] Spatial effects may indeed be important but are not precisely estimated in this sample.

The linear interpolation scheme in (5) is arbitrary, but the estimates are robust to alternative assumptions. One such alternative is to interpolate backward the number of immigrants in each city using the aggregate (national) monthly growth rate in the stock of immigrants during 1990. This would introduce variation over time in the change in R_{ct}, which would contribute to the identification of the parameters. This source of identification, however, is problematic because it is fictitious: there are no actual data on the monthly changes in the ratio of immigrants by city. Thus I prefer to forgo this source of identification and to rely only on cross-city variation in the ratio of immigrants to natives in December 1990. In any case, estimates based on this interpolation assumption are similar to those in table 3, confirming that parameter identification is essentially due to cross-city variation in immigration ratios and population growth.[31]

A last thing to notice is that FSU immigrants did not settle in Arab cities. On the other hand, most of these cities had above-average inflation in 1990 (see table 1). Thus the estimated negative relationship between prices and the immigrants/natives ratio could be driven by

[30] The regressions in table 5 do not include observations for Jerusalem because Jerusalem is a single metropolitan area. Redefining the metropolitan ratio to include the city (and thus including Jerusalem in the regression) generates almost identical estimates of the parameters of the own ratio of immigrants and of the native population but smaller (in absolute value) estimates of the ratio of immigrants in the metropolitan area. The latter are still negative but not significantly different from zero.

[31] For example, the estimates of λ and β corresponding to col. 2 of table 3 are -0.32 (0.10, 0.11) and 0.53 (0.28, 0.35), respectively, when based on the alternative interpolation procedure. At the city level, the unweighted estimates of λ and β corresponding to col. 8 are -0.36 (0.11) and 0.74 (0.56), respectively, and those corresponding to col. 10 are -0.38 (0.12) and 0.75 (0.55). The simple correlation between the two alternative measures of the ratio of immigrants to natives is 0.91.

TABLE 5
IMMIGRATION EFFECTS ON PRICES WITH METROPOLITAN EFFECTS
DEPENDENT VARIABLE: AVERAGE MONTLHLY DIFFERENCE IN LOG PRICE

		Aggregation Level: City Level			
		Weighted		Unweighted	
	Aggregation Level: Store-Product Level (1)	(2)	Adjusted Price Changes (3)	(4)	Adjusted Price Changes (5)
Change in immigrants/natives ratio	−.226	−.202	−.186	−.448	−.481
	(.115)**	(.181)	(.179)	(.177)**	(.172)***
	[.133]*				
Change in immigrants/natives ratio in metropolitan area	−.363	−.706	−.835	−.524	−.558
	(.278)	(.807)	(.803)	(.644)	(.621)
	[.449]				
Growth rate in native population	.676	.096	.152	.698	.714
	(.305)**	(.495)	(.486)	(.518)	(.502)
	[.386]*				
p-value of *F*-test for zero coefficients	<.01 (.037)	.51	.54	.015	<.01
Product dummies	Yes	No	Yes	No	Yes
R^2	.08	.34	.54	.49	.57
Observations	29,174	51	51	51	51

NOTE.—In col. 1, standard errors in parentheses are clustered at the product level and those in brackets at the city level, using 915 and 52 clusters, respectively. Standard errors in cols. 2–5 are robust to heteroskedasticity. In cols. 2 and 3, cities are weighted by the number of observations in each city. The ratio of immigrants to natives in the metropolitan area of a city excludes the city itself. The four metro areas are Jerusalem (one city), northern (22 cities), central (23 cities), and southern (six cities). Regressions exclude observations for Jerusalem. The *F*-test for zero coefficients does not include the coefficient of the ratio in the metropolitan area. In cols. 3 and 5, the city average of the product dummies estimated in col. 1 is subtracted from the dependent variable. All regressions include dummies for t_1 and t_0 interacted with $(t_1 - t_0)^{-1}$.

* Significantly different from zero at the 10 percent significance level.

** Significantly different from zero at the 5 percent significance level.

*** Significantly different from zero at the 1 percent significance level.

these few cities. To rule out this possibility I rerun the regressions in table 3 excluding the observations belonging to the six Arab cities in the sample. The estimated immigration effects are indeed a bit smaller but remain negative and quite significant even though they are less precisely estimated because of the smaller number of observations and the smaller variance in the immigrants/natives ratio.[32]

Other robustness checks were conducted and, overall, the estimates of the effect of the FSU immigration on prices are largely invariant to various modifications and extensions of the baseline regressions in table 3.[33] The estimates point to a large negative effect of immigration on prices. The estimates imply that, when the size of the native population and all other factors are held constant, prices in a city chosen at random should be lower by 2.6 percent in December 1990 compared to the case in which no immigrants settled in the city.[34] This is a strong effect when compared to the average monthly price change of 0.74 percent over all months, products, and stores in the sample. We can also assess the economic plausibility of these estimates by deducing the demand elasticity immigrants would need to have in order to rationalize a 2.6 percent price decrease when stores price according to the usual inverse demand price elasticity rule. Clearly, demand price elasticity would need to increase to justify the price decrease. Since demand elasticity is a weighted average of the natives' and immigrants' elasticities with weights given by their share in total demand, the implied demand elasticity of immigrants should be significantly larger than the natives' demand elasticity. In a typical example, if the markup ratio before immigration is

[32] For example, the estimates of λ and β corresponding to col. 2 of table 3 excluding the six Arab cities are -0.22 (0.11, 0.12) and 0.49 (0.30, 0.34), respectively. At the city level, the unweighted estimates of λ and β corresponding to col. 8 are -0.30 (0.20) and 0.42 (0.47), respectively, and those corresponding to col. 10 are -0.32 (0.19) and 0.43 (0.51). These estimates are based on 46 cities. The variance of R decreases by 19 percent when the six Arab cities are excluded.

[33] The monthly data allow us to estimate a first-differenced version of (3). The number of observations increases to 165,846 store-product-month observations because each store-product combination is observed in several months. The results from the first-differenced version are very close to those from the long-differenced version. The estimates of λ and β corresponding to col. 2 of table 3 are -0.20 (0.11, 0.13) and 0.61 (0.27, 0.35), respectively. At the city level, the unweighted estimates of λ and β corresponding to col. 8 are -0.51 (0.11) and 0.68 (0.40), respectively, and those corresponding to col. 10 are -0.46 (0.11) and 0.68 (0.39). The larger number of observations does not increase the precision of the estimators much because the dependent variable is now much noisier.

One may conjecture that the arrival of immigrants may initially decrease prices, but further increases in R may have smaller effects because prices are already close to their competitive level. In order to capture this nonlinear effect, I added R^2 to the basic specification, but its effect, although positive, is not significantly different from zero. The marginal effect of the immigration ratio on prices is constant in this sample.

[34] This is obtained by multiplying the average ratio of immigrants to natives across all cities, 0.053 in the bottom row of col. 6 in table 1, by 0.496, the estimated immigration effect in col. 10 of table 3.

1.5—which translates into a natives' demand elasticity of 3—the implied immigrants' elasticity is 5.8.[35] This back-of-the-envelope calculation indicates that the implied quantitative relationship between the immigrants' and natives' elasticities is plausible, at least for reasonable markup ratios.

In principle, FSU immigrants can have two opposing effects on prices: they unexpectedly increase aggregate demand, and this can increase prices, at least during the short run, but they also change the composition of demand in ways that may have decreased prices. The size effect is not in general significantly different from zero. On the other hand, the estimate of λ, the total immigration effect on prices, is negative and statistically significant at conventional levels of significance. This suggests that the composition effect of the FSU immigration is also negative. This is important because this negative effect on prices may last as long as immigrants are not completely assimilated into the local economy.[36] Thus gains from the lower prices prompted by the FSU immigration may be long-lasting, and because stores do not price-discriminate, the whole population stands to benefit from this.

[35] Consider a symmetric model of monopolistic competition with differentiated products à la Dixit-Stiglitz. The equilibrium price for any brand before immigration is given by $p = c[1 - (1/\epsilon_N)]^{-1}$, where c is marginal cost and ϵ_N is the price elasticity of natives' demand for a specific brand; for a large number of brands, it can be shown to be approximately equal to the elasticity of substitution. Consider now the arrival of FSU immigrants into a city. Demand for each brand equals the sum of the demands from the N natives and I immigrants. For pricing purposes, the only difference among the two types of consumers that matters is that immigrants have a higher elasticity of substitution and therefore a higher price elasticity than natives, i.e., $\epsilon_I > \epsilon_N$. Let ϵ' be the price elasticity of demand after immigration. This elasticity is a weighted average of the natives' (ϵ_N) and immigrants' (ϵ_I) elasticities, with weights equal to the group's share in total demand. It is assumed that ϵ_N is the same before and after immigration. Assuming that demand shares are equal to consumption expenditure shares, 94 and 6 percent for natives and immigrants respectively, we have $\epsilon' = 0.94\epsilon_N + 0.06\epsilon_I$. Let p' be the new price for a specific brand after immigration. Because the new price satisfies $p' = c[1 - (1/\epsilon')]^{-1}$ and $p' = (1 - \alpha)p$, where p is the price before immigration and $0 < \alpha < 1$ is the reduction in prices, e.g., $\alpha = 0.026$, we get

$$\frac{1}{\epsilon'} = 1 - \frac{1}{1-\alpha}\left(1 - \frac{1}{\epsilon_N}\right).$$

The relationship between the natives' and immigrants' elasticities implied by this equation and the one used in the text is given by

$$\epsilon_I = \frac{1}{1 - .94}\frac{(1-\alpha)\epsilon_N}{1 - \alpha\epsilon_N} - \frac{.94}{1 - .94}\epsilon_N.$$

[36] This can take a long time. Weiss et al. (2003) show that FSU immigrants never reach the income level of comparable Israeli natives. The estimates of their structural model predict that immigrants' lifetime earnings are 57 percent below the lifetime earnings of comparable natives. Even after 30 years in Israel, FSU immigrants are predicted to earn 15 percent less than natives in the same job.

B. Possible Explanations

If immigrants behave in the same way that natives do, then demand composition should not matter and λ would be zero. The data strongly reject this hypothesis. The estimated negative immigration effect is consistent with models in which the new immigrants have higher price elasticities than the native population. For example, in Bils's (1989) model, consumers develop some attachment to products previously purchased and stores are keen to lock in these new buyers by lowering their markups, trading off the objectives of exploiting existing customers and attracting new ones. Immigrants do not have strong brand and store attachments upon arrival in Israel and also have lower incomes, making them potentially more price-sensitive than natives. Other things equal, cities with a higher proportion of immigrants should exhibit lower prices.

Another explanation of the negative immigration effect is that FSU immigrants search more intensively for lower prices than the native population. Stahl (1989) shows that as the proportion of consumers with zero search costs increases, stores will compete more fiercely by lowering their prices.[37] Intuitively, it pays to lower prices because stores that deviate downward will get more of the consumers with zero search costs. This will result in an equilibrium distribution of prices that shifts monotonically from the unique monopoly price when there are no consumers with zero search costs toward the unique competitive price when all consumers have zero search costs. Along the way, the expected price falls monotonically with the share of zero–search cost consumers. If we identify the arrival of immigrants with an increase in the share of zero–search cost consumers, then Stahl's model predicts a negative relationship between expected price and the ratio of immigrants to natives, as found in table 3.[38]

At this stage, it is not possible to identify the independent contribution of each explanation because it is the same group of consumers that both is more price elastic and searches more intensively for lower prices than the native population.

[37] See Brown and Goolsbee (2002) for a recent empirical examination of the model's implications and also Janssen and Moraga-Gonzales (2004) for a recent theoretical extension of Stahl's model.

[38] The important point in Stahl's model is not that search costs are zero but that there is a group of consumers who are fully informed about prices in different stores. Stahl's prediction is robust to the model's assumptions. For example, the earlier models along the "bargains and ripoffs" line generate two-price distributions in which an increase in the proportion of consumers with low search costs induces an increase in the proportion of stores selling at the low price. Thus average price also declines. Also, in Stahl's model, marginal cost of production is constant, and therefore demand size does not affect prices; only its distribution among zero– and positive–search cost consumers does. To allow for a size effect, one would need to introduce increasing marginal costs.

But do immigrants have lower search costs and do they search more intensively than the native population? Theory and the available data suggest an affirmative answer. As mentioned in Section II.*A*, the familiarity of the initial wave of FSU immigrants arriving in Israel during 1990 with a modern market economy was limited. Learning about the different ways in which a market economy operates can be thought of as a process of search. The immigrants visited the stores and learned about the large variety of goods and services offered in the Israeli market relative to their previous experience in the FSU. In particular, they learned that the same products are sold at different prices in different stores. The existence of price dispersion constitutes a powerful incentive for immigrants to engage in search for the stores having the best combination of product characteristics and prices.[39] Processing this new information and matching the new options to their individual preferences take time and effort. But the alternative cost of this time and effort was relatively low for FSU immigrants because most of them were not gainfully employed during the first few months following their arrival in Israel. At the time of the 1990 Labor Force Survey, 81 percent of the immigrants who arrived during 1990 were *not* part of the labor force; among those in the labor force, 53 percent were unemployed.[40] The cost of time for the new immigrants was therefore much lower than for the native population so that, on this account, they faced lower search costs.[41]

The only available data on shopping habits of consumers are taken from the Time Budget Survey conducted in late 1991 and early 1992. FSU immigrants are identified by their country of immigration and by the requirement that they immigrated to Israel after 1989. As seen in table 6, time spent shopping per day averages 26 minutes for immigrants but only 15 minutes for natives. Most individuals did not shop during the day they were sampled, but among those shopping, immigrants spent markedly more time shopping than nonimmigrants. The difference re-

[39] See Lach (2002) for evidence on the existence and persistence of price dispersion in Israel.

[40] The participation rate of Israeli-born persons was around 57 percent, and their unemployment rate was 11 percent. In 1991, 54 percent of the immigrants arriving in 1990 and 1991 were not in the labor force, and among those in the labor force, 38 percent were unemployed.

[41] Another mechanism leading to more search is proposed by Fishman and Simhon (2005). They show that changes in the distribution of income can generate changes in the amount of search because the utility gain from finding a lower price is larger for low-income consumers than for high-income consumers. Thus an inflow of low-income consumers would increase search in the market. See also Frankel and Gould (2001) for empirical evidence on the relationship between a city's income distribution and prices.

TABLE 6
Time Spent Shopping (Minutes per Day)

	Mean	Median	75%	90%	95%	Observations
FSU immigrants	25.8	0	45	90	105	187
Natives	15.2	0	15	60	90	4,586

Note.—Data are taken from "Time Use in Israel—Time Budget Survey 1991/92" (Central Bureau of Statistics 1995). Shopping is defined as everyday shopping and other shopping (not everyday) excluding time spent on personal and medical services.

mains after I control for individual and household characteristics.[42] Although time shopping is not exactly equivalent to time spent searching for low prices, it certainly is among the measurements closer to the ideal concept.

If the immigration effect does indeed reflect changes in demand, we would expect to find a stronger effect in products in which immigrants represent a larger share of the market. In an extreme case, if FSU immigrants do not buy a particular product, then its price should not be responsive to the ratio of immigrants in the city. In order to estimate the immigration effect by product, I grouped the 915 individual products into 40 categories determined by the CPI classification of products. Table 7 presents results obtained by interacting R with the 40 category dummies.[43] I report the estimated coefficient of R for each group (and not just the contrast with the reference group) and its standard error, sorted from the most negative to the most positive estimate. Notice that the immigration effect is negative in 32 out of the 40 product groups, but only six products have significant negative estimates at the 10 percent significance level when standard errors are clustered at the city level. When they are clustered at the product category level, 23 out of the 40 coefficients are significantly negative at the 10 percent significance level.

These results indicate that the negative immigration effect is not spe-

[42] An OLS regression of time shopping on an immigrant dummy gives an estimated coefficient of 10.6 ($= 25.8 - 15.2$) with a robust standard error of 3.18. Adding age, gender, an employment indicator, household size, and household income to this regression reduces the immigrant dummy to 9.8 minutes with a robust standard error of 3.28.

[43] The dependent variable is the average of $(\log p_{jict_{1ij}} - \log p_{jict_{0ij}})/(t_{1ij} - t_{0ij})$ in eq. (9) over stores i and products j in each product category and city. The number of observations should be $40 \times 52 = 2{,}080$, but it actually is 1,002 because most product categories are not sampled in all cities. In fact, only clothing and footwear are sampled in all 52 cities, whereas pork products and pets and accessories, e.g., are sampled in only six and seven cities, respectively. The product category–city average monthly price change is adjusted for variations in the set of products sampled in each city by subtracting the average of individual product dummies estimated from a store-level regression. In addition to the timing variables, the regressors include dummies for each product category and interactions between these dummies and the change in the ratio of immigrants to natives. Except for these dummies, the specification of the regression otherwise is equal to that in col. 10 of table 3. The R^2 of the regression is 0.28.

cific to particular products. They also reveal interesting differences among the various product categories. The specific religious and sociological characteristics of Israel provide us with a natural case in which immigrants constitute a major market force: pork products. Pork products were sold in Israel for a long time, but the market was small because of religious restrictions.[44] The arrival of the largely nonkosher FSU immigrants, who were also familiar with pork products, generated a disproportionate increase in the demand for pork products. If the estimated immigration effect is indeed picking up a demand change, we would expect to find a strong effect in the market for pork products. This appears to be the case: pork products exhibit a very strong negative immigration effect, although it is not measured precisely because pork products are sold in only six cities in the sample. Another case of interest is alcoholic beverages and, particularly, vodka. The estimated immigration effect on prices of alcoholic beverages, excluding vodka, is indeed negative (-0.53), although not particularly strong; but it is more than three times stronger for vodka (-1.81), a product whose share of FSU immigrants is likely to be large.

In order to go beyond the obvious cases (pork products and vodka), we need to match consumption expenditure data to the product categories used in the CPI. This is problematic because the CPI price classification is at a much higher disaggregated level than the available consumption expenditure data. Nevertheless, data from the Household Expenditure Survey for 1992/93—the closest year to 1990—are informative on the share of FSU immigrants in some broad types of expenditures that usually include the product categories defined in the CPI data (Central Bureau of Statistics 1994). The data reveal that FSU immigrants' share in total consumption as well as in food consumption was 6 percent. However, their share in the expenditures on processed meat products (including pork products) was 15 percent, in alcoholic beverages (including vodka) it was 8 percent, and in fish it was 7 percent. These are product categories appearing at the top of the list in table 7. In meat and poultry, the share of FSU immigrants was close to their share in total food expenditures, 5–6 percent, whereas their share in expenditures on linen was only 2 percent. These are product categories in the bottom half of the list in table 7. There are, however, some categories (e.g., toys and games) for which the share of FSU immigrants was relatively small but their effect was very strong, and categories for which the share of FSU immigrants was large and their effects small

[44] Dietary restrictions forbid Jews and Muslims to eat pork. Nevertheless, there was a small market for pork products prior to the FSU immigration. These products are not sold in regular supermarkets or grocery stores. They are sold only in special nonkosher stores.

TABLE 7
Immigration Effects on Prices by Product Category

	Immigration Effect	Standard Error Clustered at: City Level	Standard Error Clustered at: Product Category Level
1. Jam and sweets	−3.50	.87	.14
2. Toys and games	−2.51	1.42	.42
3. Vodka	−1.81	1.05	.10
4. Nonelectric equipment	−1.56	.75	.30
5. Pork, all kinds	−1.54	1.97	.29
6. Canned meat, sausages, frozen meatballs, and hamburgers	−1.14	1.86	.26
7. Soft drinks	−1.03	.57	.16
8. Fish, all kinds, fresh and frozen	−1.00	.85	.22
9. Chocolate, candy, and other sweets	−.93	.70	.13
10. Sugar, spices, instant soups	−.91	1.29	.14
11. Sport and recreation equipment	−.89	.70	.27
12. School supplies	−.89	.66	.26
13. Frozen, pickled, and preserved fruits and vegetables	−.72	.61	.05
14. Washing and cosmetics articles	−.65	1.64	.28
15. Coffee, tea, cocoa	−.61	.83	.16
16. Jewelry and watches	−.61	.80	.43
17. Medicines, glasses, and contact lenses and accessories	−.61	1.08	.34
18. Clothing, footwear, and accessories	−.59	.35	.12
19. Wine, beer, and spirits (excluding vodka)	−.53	.73	.14
20. Cakes, biscuits, and cookies	−.40	.50	.08
21. Durable culture and entertainment products (books, music, etc.)	−.33	.77	.29
22. Furniture	−.26	.59	.13
23. Chicken, turkey, whole and parts, fresh and frozen	−.25	.49	.10
24. Bread, flour, cereals, rice, and dough products (e.g., spaghetti)	−.20	1.00	.09
25. Electrical appliances and equipment	−.20	.64	.14
26. Pets and accessories	−.19	1.48	.31
27. Meals away from home	−.18	.64	.20
28. Hairdressing	−.17	.94	.17
29. Cigarettes	−.17	.72	.07
30. Religious articles	−.09	.30	.22
31. Beef, all kinds, fresh and frozen	−.06	.66	.15
32. Tailoring and fabrics	−.03	.83	.30
33. Baby food	.01	1.34	.16
34. Milk and dairy products	.10	.51	.26
35. Miscellaneous household items (including paint and tools)	.26	.42	.13
36. Oil, eggs, hummus, tahini, and eggplant salad	.27	1.26	.13
37. Fish preserves and fish salads	.36	1.22	.12
38. Cold cuts (kosher)	.69	1.58	.10

TABLE 7
(*Continued*)

	Immigration Effect	Standard Error Clustered at: City Level	Standard Error Clustered at: Product Category Level
39. Linen and home decoration (including plants and flowers)	.85	1.69	.20
40. Car accessories and gas	2.23	1.17	.21

Note.—The estimated regression is as in col. 10 of table 3 with product category dummies interacted with *R*. Entries are the estimated immigration effect for each product (coefficient for reference group plus interaction dummy).

(e.g., electronics).[45] Nevertheless, and despite the problems in matching the product definitions between the two data sources, the "by product category" analysis is suggestive of a demand-based interpretation of the estimated immigration effect.[46]

The empirical findings indicate that the FSU immigrants played a significant moderating role in the pricing of consumer goods in Israel. It is not unreasonable to expect newly arrived immigrants to have higher price elasticities and lower search costs than the native population, and the effect of the arrival of a mass of such consumers is to constrain stores' abilities to raise prices. We can actually observe the correlation between the arrival of immigrants in a city and the extent to which stores lowered their prices during 1990. For each city, we computed the share of "price decreases" during 1990. A price decrease indicator was assigned to each store-product observation when the store's average monthly price change for the product—the dependent variable in (9)—was lower than the product-specific average monthly price change.[47] Thus a price decrease occurs when a store did not increase its price as much as the average product-specific inflation rate. This is a more appropriate indicator of price decreases than simply nominal price decreases because of the upward trend in prices during 1990: aggregate (CPI) inflation was 17.6 percent. Overall, 55 percent of the 32,784 store-

[45] In toys and games, FSU immigrants accounted for 4 percent of all expenditures, and the estimated effect is −2.5. In electrical appliances and equipment, FSU immigrants accounted for 10 percent of the expenditures, and the estimated effect was −0.20.

[46] Assume that the immigration effect for product j is now $\lambda_j = \alpha_j\lambda + \eta_j^\lambda$, where α_j is the share of FSU consumers in product j's market. Allowing for product-specific parameters allows us to estimate $\alpha_j\lambda$. As suggested by a referee, if we had good data on α_j, we could test this specification and show that indeed the strength of the immigration effect depends on the share of FSU consumers. Regretfully, the available data on α_j do not match well with the CPI price classification of products to pursue this issue econometrically further.

[47] The latter is the average of the monthly price changes across all stores (in all cities) weighted by the store's number of monthly observations for the product. The average and median of these product-specific monthly inflation rates among the 915 products was 0.85 percent.

product observations across all cities were assigned a price decrease this way. I then computed the share of store-products having a price decrease during 1990 for each city—this share varies between 0.44 and 0.64 among the 52 cities—and regressed this variable on the ratio of immigrants to natives at the end of December 1990, R, controlling for the number of stores and products in the city and for the size of the native population.

Table 8 reports these results. The effect of R on the share of price decreases is always positive and significant, irrespective of the other control variables. A one-percentage-point increase in the ratio of immigrants to natives increases the share of price decreases by 0.5 percentage point. This is indeed consistent with the previous finding of a negative effect of immigration on prices.

Table 8 also reports the effect of immigration on the incidence of "sales" in a city. For each store-product observation a sale in month t occurs when the price at t is at least x percent *below* the price in the previous month and the price in month $t+1$ is equal to or above the price at $t-1$.[48] It is certainly not surprising that the vast majority of store-product observations do not correspond to sales according to this definition.[49] I computed the share of store-products exhibiting a sales episode for each city and regressed this variable on the same regressors used in the price decrease regressions.[50] The relationship between sales and immigration, although positive, is not significant (although the p-value of the estimated coefficient of R in col. 6 is 0.11). It should be noted, however, that the measure of sales understates the true number of sales in the city, and this can bias downward the estimated coefficient of R.[51] It is therefore difficult to reach a definite conclusion. If, however,

[48] In fact, to avoid rounding errors the price at $t+1$ was required to be at least 99 percent of the price at $t-1$.

[49] When sales are defined as price decreases of at least 2.5 percent ($x = 2.5$), only 11.6 percent of the store-products exhibit at least one sale; when x is set to 10 percent, sales represent only 3.8 percent of all store-product observations. Occasionally stores are observed to have two or three sale episodes per product during 1990, but this is a very rare event: it occurs in 0.68 percent of the cases when sales are defined as price reductions of at least 2.5 percent and in 0.19 percent of the cases when sales are defined as at least 10 percent price reductions.

[50] When sales are defined by the 2.5 percent rule, the average and median share of sales by city is 13.5 percent, ranging from 3.6 percent in Migdal HaEmeq to 44 percent in Nazareth Illit. The latter had the highest ratio of immigrants to natives in 1990, $R = 0.17$. When sales are defined by the 10 percent rule, the average and median share of sales by city is 4 percent, ranging from 0 percent in Hod Hasharon to 10 percent in Dimona.

[51] The definition of sales used here cannot detect "short-lasting" sales, i.e., sales episodes occurring wholly within two consecutive sampling times. Let N^* be the true number of sales and $N = N^* - \epsilon$ be the observed value. Because $N \leq N^*$, we require $\epsilon \geq 0$. Using N instead of N^* as the dependent variable adds $-\epsilon$ to the error. If ϵ is positively correlated with R because, say, cities with higher immigration ratios may have more sales, including short-lasting sales, then the error in the regression and R are negatively correlated, possibly leading to a downward-biased estimator of R's coefficient.

TABLE 8
IMMIGRATION EFFECTS ON PRICE DECREASES AND SALES

	Aggregation Level: City Level (Unweighted)								
				Dependent Variable: Share of Sales					
	Dependent Variable: Share of Price Decreases			At Least 2.5%			At Least 10%		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immigrants/natives ratio	.503***	.509***	.499***	.584	.620	.647	.072	.072	.081
	(.167)	(.163)	(.168)	(.455)	(.432)	(.400)	(.125)	(.129)	(.119)
Number of products in city		−.00001	.00005		−.00012**	−.00030**		−.000002	−.000060
		(.00004)	(.00009)		(.00006)	(.0013)		(.00002)	(.00005)
Number of stores in city		−.00007	−.00006		.0002	.00018*		−.000003	−.000008
		(.0010)	(.00010)		(.00012)	(.00011)		(.00004)	(.00004)
Native population size			−.019			.048			.016
			(.021)			(.027)*			(.010)
R^2	.13	.15	.16	.10	.17	.23	.03	.03	.06
Observations	52	52	52	52	52	52	52	52	52

NOTE.—Standard errors, in parentheses, are robust to heteroskedasticity.
* Significantly different from zero at the 10 percent significance level.
** Significantly different from zero at the 5 percent significance level.
*** Significantly different from zero at the 1 percent significance level.

the incidence of sales is not significantly associated with the arrival of immigrants into a city, this would suggest that immigration does lead to lower prices through permanent reductions in price rather than through sales.

An alternative explanation of the negative immigration effect is that, as production increased to meet demand, firms exploited scale economies and decreased production costs. In a small country such as Israel, where production is usually centralized in one location and distributed nationally, decreasing production costs would be reflected in lower prices in *all* cities, irrespective of their population size and composition. Decreasing production costs would therefore be captured by the aggregate month and product-specific inflation dummies and not by the ratio of immigrants to natives in the city.[52]

A different version of this argument is that the FSU immigration depressed wages in *local* labor markets, especially in low-skilled occupations such as clerks in retail stores. The estimated negative immigration effect would then reflect this supply channel because of the negative correlation between R and retail wages. The key point here is the hypothesized negative correlation between immigration and wages. By restricting the empirical analysis to the first year of the FSU immigration wave, we limit the extent of such correlation because most immigrants did not participate in the labor force during 1990. Moreover, the available empirical evidence shows that natives' wages decreased very little, if at all, as a result of the arrival of the FSU immigrants (Friedberg 2001; Cohen-Goldner and Paserman 2004). In addition, the finding that λ is somewhat stronger in products whose immigrants' market share is larger is not a pattern we would expect to observe if immigration effects reflect wage decreases. In sum, although a supply-side explanation of this type cannot be completely ruled out, there is no compelling evidence supporting it either, at least in the short run.[53]

While production costs may have decreased because of scale economies, the sale of those goods—retailing—may have become more costly. The massive, rapid, and unexpected increase in the number of customers could have prompted stores to increase their workforce, inventories, and so forth on short notice with consequent increases in marginal

[52] The same argument applies to imports since imports are also distributed nationally. Most of the products in the sample are tradable except, possibly, for dairy products.

[53] In the long run, Cortes (2005) finds that low-skilled immigration to the United States reduces prices of low-skilled intensive services such as gardening, housekeeping, babysitting, etc. because low-skilled immigrants reduce wages in these occupations. Her analysis, however, is based on effects operating for a long period of time, from 1980 to 2000.

costs, if they were already operating at or near full capacity.[54] On this account, prices should have increased in cities with large immigration inflows. In terms of model (3), this argument implies that the term $\beta\theta$ should be positive. The empirical analysis reveals, however, that the parameter β is not significantly different from zero, suggesting that the unexpected arrival of immigrants did not significantly increase marginal retail costs. This accords with the common perception that the Israeli economy was sliding into a recession during 1989 so that the retail sector was likely to be operating below full capacity when the FSU immigrants started to arrive in 1990.[55]

V. Conclusions

This paper examines what happened to prices during 1990 following the unexpected arrival of almost 200,000 FSU immigrants to Israel, representing 4 percent of Israel's population. Essentially, I trace the effect of the arrival of FSU immigrants into a city on the prices of products sold in the city. The variation in the ratio of immigrants to natives across cities was large, and I use this variation to identify the immigration effect. The data are monthly, store-level prices on 915 products sold by 1,837 stores in 52 cities across Israel during the year 1990.

The main empirical finding is that, contrary to the predictions of the standard perfectly competitive model, the arrival of immigrants into a city chosen at random had a moderating effect on prices. A one-percentage-point increase in the ratio of immigrants to natives decreases prices by 0.5 percentage point. This estimate implies that, when the size of the native population and all other factors are held constant, prices in a city with the average immigrants/natives ratio should be lower by 2.6 percent in December 1990 compared to the case in which no immigrants settled in the city.

This result is consistent with the FSU immigrants—the new consum-

[54] Expanding retail space can be a significant investment that is mostly irreversible from the firm's point of view. Because of the uncertainty during 1990 regarding the future flows of FSU immigrants, it is likely that investors waited before they committed resources to open new stores or to expand retail space beyond what was planned. Retail space was therefore fixed in the short run. Furthermore, there is no evidence that entry of new stores was significant during 1990. The only source of data on the number of stores by city is the Value Added Tax Authority. These data are problematic because, among other things, they do not capture the opening or closing of branches of multibranch firms since the VAT is paid by the central office. With this caveat in mind, the growth rate in the number of firms paying VAT in eight major cities in Israel between 1989 and 1990 was not significantly different from that between 1990 and 1991. Data for years before 1988 are not available. The cities are Beer Sheva, Eilat, Givataym, Haifa, Holon, Jerusalem, Ramat Hasharon, and Tel Aviv.

[55] All macroeconomic indicators paint a contractionary picture. For example, unemployment was 8.9 percent in 1989, up from 6.4 percent in 1988, whereas GDP growth was 1.4 percent in 1989, down from 3.6 percent in 1988.

ers—having higher price elasticities and lower search costs than the native population. The evidence shows that immigrants do indeed spend more time shopping than natives. As a result, stores tend to lower their prices in order to attract these new customers. This explains the negative immigration effect. Significantly, the negative immigration effect holds for almost all product categories and is stronger in products for which immigrants have a larger share of the expenditure supporting a demand-based interpretation of this effect.

More generally, the paper shows that by changing the composition of demand, immigrants can have an effect on product markets. It is not unreasonable to expect newly arrived immigrants to have higher price elasticities and lower search costs than the native population, at least in the short run. When this occurs and immigration is large enough, stores will be more reluctant to raise their prices. Thus immigration can have a moderating effect on inflation through its direct effect on product markets, and not only by increasing the supply of labor.

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