Firm Startups, Population Growth and Domestic Migration

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Across a variety of measures, the level of dynamism in the U.S. economy has fallen in recent decades. For example, workers are changing jobs less often and moving less than they used to. On the firm side, the startup rate has fallen sharply since the 1980s, and more importantly there are fewer high-growth young firms (Decker et al., 2016).

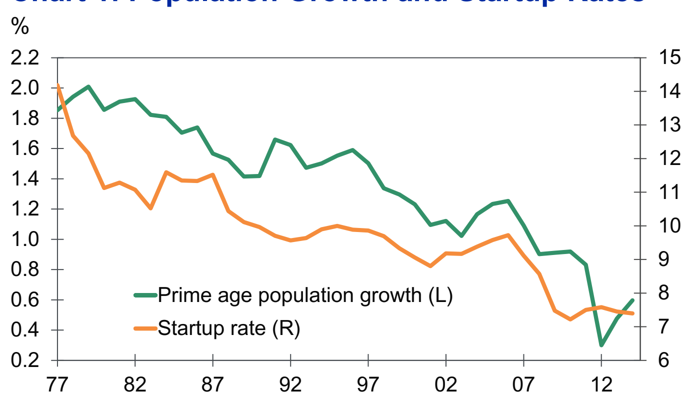
If all firms were equally productive and innovative, startups and the startup rate would be of little importance for economic growth. However, there is a significant difference between the most and least productive firms, even within small or narrowly defined industries. Using the four-digit NAICS definitions[1], manufacturers at the 90th percentile of productivity had twice the productivity of those at the 10th percentile (Syverson, 2004). This is why in "The Facts of Economic Growth," Charles Jones argues that the literature on misallocation provides "our best candidate answer to the question of why are some countries so much richer than others" (Jones, 2016).

The first order importance of misallocation means that mechanisms that reallocate output and employment to the most productive firms can have large effects on aggregate productivity. Startups are important because they represent a significant source of this kind of reallocation. A popular perception is that small businesses drive job growth, but Haltiwanger, Jarmin and Miranda (2009) show that it is young firms, not small ones, which generate an outsize share of job growth: Startups make up 10% of firms but 20% of gross job creation. Startups also have a high failure rate, with most closing in the first few years, while those that do survive have faster employment, output and productivity growth (Decker et al., 2014). Low productivity firms are more likely to fail, and high productivity firms are more likely to survive and grow (Foster, Grim and Haltiwanger, 2009). Overall, startups exhibit an "up or out" dynamic and as a result are an important source of aggregate productivity-enhancing reallocation. Haltiwanger et al. (2016) find that half of labor productivity growth within an industry for continuing firms is due to reallocation from less to more productive firms, and that fast growing young firms contribute to this significantly.

Because of the importance of startups for job and productivity growth, the decline in startup rates in recent decades has been met with concern from policymakers and economists. Yet the cause of the decline remains largely unidentified. Startup rates are down across a variety of industries, including retail, manufacturing and services, suggesting an industry-specific cause is unlikely. The decline is also occurring across states, including those with significantly different business climates—such as California and Texas—suggesting state-level policies are not the primary driver. Indeed, Karahan, Pugsley and Sahin (2016) look across industry and state together and find that the startup rate fell in 85% of state-by-industry pairs. Summarizing the literature, Decker et al. (2014) argue that "[w]e do not yet fully understand the causes of the decline in indicators of business dynamism and entrepreneurship."

Recent research has begun looking to demographics as a potential explanation. As the aggregate time-series data show, the decline in startup rates has occurred alongside a slowing of U.S. population growth (see Chart 1). The relationship is even closer when

Chart 1: Population Growth and Startup Rates



Sources: Census Bureau, Moody's Analytics

1 North American Industry Classification System

focusing on the growth of the working-age population age 25 to 64. A handful of studies have offered theoretical and empirical support for the relationship.

Karahan, Pugsley and Sahin (2016) hypothesize that population growth increases the startup rate through higher labor supply. They argue that if firm lifecycle dynamics remain unchanged, an expanded labor supply will be accommodated through increases in the entry rate. This theory is also consistent with a demand-side explanation: Population leads to an increase in local demand that, given fixed firm lifecycle dynamics, must be accommodated through an increase in entry. In theory, either more labor supply or aggregate demand could also be accommodated by expansions at existing firms: Lifecycle dynamics need not remain unchanged. However, using a state panel model they show that changes in the growth rate of the workforce, as instrumented by 20-year lagged birthrates, affect only the startup rate and not the growth rate or survival rate of existing firms.

In the closest paper to ours, Hathaway and Litan (2014) look at metro-area level startup rates and compare them to changes in population growth over time. They model the effect of population growth on startup rates using 350 metro areas with two models. First, they use a single cross-section regression using the change in startup rates from 1980 to 2006 compared with total population growth over this time period. They also utilize a panel model with metro-area fixed effects using every year between 1980 and 2012. In both models they find a significant effect of population growth rates on metro-area startup rates.

We build upon Hathaway and Litan (2014) in several ways. Endogeneity is the most significant challenge to interpreting the simple metro-area panel models as identifying the causal effect of population growth on startup rates. It is plausible that startups increase and this draws in more population. We attempt to rule out endogeneity in multiple ways. First, we show that the effect of population in a fixed-effects panel is robust to the inclusion of employment growth, which is the most plausible mechanism through which startup rates would cause population growth. Second, we utilize an instrumental variable for population growth based on metro-to-metro migration rates from Howard (2017). In brief, this creates a proxy for annual migration into a specific metro area that is not subject to reverse causality by using historical migration patterns combined with annual changes in migration flows between other metro areas. Finally, we utilize dynamic panel models to further test for causality and control for omitted variables.

Main findings

Overall, we find a robust and consistent effect of population growth on startup rates. The effect appears to operate through the working-age population, consistent with labor supply rather than demand.

This article is a first step to *causally* link the effects of population growth to firm startups, and thus indirectly to productivity growth, by relying on the identification mechanism proposed in Howard (2017). There are theoretical reasons to believe that economies of scale occur in the size of the market, at least ever since Arrow's influential paper on learning-by-doing (1962) and the endogenous growth literature it preceded. More recent literature on agglomeration economies proposes another channel (see Rosenthal and Strange, 2001; Glaeser and Gottlieb, 2009). This article is more proof of concept than an attempt to illuminate any specific mechanism, by establishing a robust relationship between migration, population growth and firm startups.

To test for this link we rely on a number of methodological approaches:

- We first control for employment growth in a simple cross-sectional regression along the lines of Hathaway and Litan (2014) to identify if population growth has any independent, statistically meaningful role.
- We then separate more and less dynamic business environments, dividing up our sample of metro areas by startup rates (low, middle and high).
- In a similar vein, we investigate if a particular age range of population growth is associated with firm formation.
- To rule out bias from other factors we are not explicitly considering, we then move on and utilize the panel structure of our data to remove individual metro-area heterogeneity.
- Finally, we build on the instrumental variable work of Howard (2017) and address potential reverse causality, that is, startup growth causing population growth. These final results are the most central presented in this article.

Irrespective of the particular approach, we indeed find that population growth of the working-age population in and of itself robustly raises firm startup rates, suggesting a causal link beyond a mere labor demand effect.

This link between demographic developments and firm formation complements broader, current literature on productivity slowdown.

A declining average number of startups may not by itself be a reason for concern. As Decker et al. (2016) notes, the churning of jobs across firms does not have social value per se, but matters as a mechanism for reallocation, putting resources to their most productive use. There is no law of nature that dictates young firms have to be the perennial champion of this process. As a result exact causes of the declining dynamism are also important in weighing the welfare consequences. For example, if technology has increased the returns to scale, this could lead to an increase in the optimal size and age of firms. However, recent research suggests that declining dynamism—manifest in falling U.S. startup rates—are a contributing factor to the recent slowdown in productivity growth, drawing a more pessimistic picture (Decker et al. 2017).

In the remaining sections, we outline in detail our data, methodology and statistical results in support of these findings.

Data

The Census Bureau's Business Dynamics Statistics database provides data on firm births and deaths, including by size, age and metro area. The BDS is compiled from the Census Bureau's Longitudinal Business Dataset and covers all legally operating private businesses in the U.S. excluding agriculture

(Haltiwanger, Jarmin and Miranda, 2009). The startup rate is defined as the number of firms born in a year divided by the total number of firms. This allows the estimation of startup rates for metro areas from 1978 to 2013.

The IRS meanwhile publishes information from tax filings as the richest geographic source for patterns of U.S. migration. Specifically, the agency reports county of origin and destination of tax filings that registered an address change in any given tax year. Since not every individual *person* files tax returns, the IRS provides the number of exemptions as a proxy for the number of individuals associated with each tax return. Exemptions then yield a rough estimate for overall migration from county to county. To match these data to the BDS data, we aggregate county-level information available from 1990 to 2011 to the 2000 Census metro area definitions. Population estimates—total and by age group—are available from the Census Bureau. We further rely on Bureau of Labor Statistics payroll employment figures. Overall, we have matched BDS and IRS data for 341 metro areas from 1995-2011.

Methodology

The purpose of this article is to demonstrate a) that population growth is systematically related to firm startup rates across metro areas and b) that there is plausible reason to assume causality running from population growth to startup rates.

While simple panel regression can help establish accurate measures of correlation, addressing the problem of causality is trickier. Robust correlation between population growth and startup rates does not imply that population growth *causes* firm formation. Specifically, the mechanism may run the other way, in what economists refer to as reverse causality: More dynamic entrepreneurial environments, characterized by high firm startup rates, conceivably bolster labor markets and incomes, potentially allowing the population to expand more quickly. This is consistent with neoclassical economics, in the sense that population growth may simply scale with economic production; the larger the metro area output, the larger is the population.

To address this point we pursue two methods. First, we specifically account for job growth in our analysis to capture mere labor supply effects. Second, and more importantly, we follow Howard (2017) and build an instrumental variable for population growth, which both explains the variation in population growth rates across metro areas and at the same time is plausibly exogenous to firm formation. The particular instrument we are suggesting relies on historical patterns of metro area in-migration as a driver of population growth.

As is well-documented (see Molly, Smith and Wozniak, 2011), geographic patterns of migration have been remarkably stable in the IRS data. Households have moved out of the Northeast and the Midwest for years, headed to the South, in particular Texas and the Florida, and to the West, specifically to the Northwest and parts of the interior Mountain West. While overall migration has slowed down in our sample period, partly related to the Great Recession in 2008, the *direction* of migration flows has broadly been resistant to current economic events, at least past a certain distance. However, since a lot of migration is local and neighboring metro areas plausibly experience the same economic shocks, we rely on two additional constraints. First, we consider only migration that is sufficiently distant to not be subject to the same local economic events. Howard (2007) suggests migration beyond 100 miles, and we follow his suggestion.[2] Second, we rely on historic migration patterns from 1990 to 1994 to predict subsequent population growth in any year from 1995 to 2011, which ensures that migration flows are not caused by later changes in firm startup rates.

An example of how this instrument is constructed is as follows. Say we wish to build an instrument for in-migration to Philadelphia-Camden-Wilmington (PHI). To do so, we follow these steps:

- First, we identify all metro areas that are more than 100 miles away from PHI. All of the following calculations are based only on this sample.
- For each of these metros areas, we then calculate the average share of out-migration to PHI from 1990 to 1994. For example, from 1990-1994 on average 0.7% of the out-migrants from San Francis-Oakland-Fremont (SAF) went to PHI.
- To obtain predicted migration from another metro area to PHI in any subsequent year, we multiply the historical out-migration share by the actual overall out-migration from that metro area in that year. In 2000, for instance, the IRS lists about 74,000 migrants leaving SAF, resulting in predicted migration from SAF to PHI of about 500 individuals (0.7% x 74,000).
- In a penultimate step, we obtain total predicted in-migration to PHI by adding all in-migrants from all other metro areas farther than 100 miles, as exemplified in step 3.
- Finally, since large metro areas attract larger numbers of migrants, we scale predicted in-migration by the receiving metro area's population.

Table 1a and Table 1b report summary statistics and basic pairwise correlations for the variables used in this article. We have startup rates for 341 metro areas from 1995-2011, based on the BDS data. The IRS covers slightly more metro areas.

Total U.S. actual and predicted migration should be very similar to each other by construction, since we drop international migration (which has incomplete coverage in IRS data because foreign migrants do not always file taxes right away) and since we exclude metro areas within 100 miles. However, for any given metro area, the two do not have to be identical, as illustrated by different maximum values. Notice that the average metro area in our sample had about 8,000 in-migrants per year, while the largest metro area (Los Angeles) had about 180,000. The in-migrant/population ratio on average is about 1.2% within our sample.

Perhaps more relevant than simple summary statistics in the present context is the correlation between our variables of interest. Both population and employment growth are positively associated with startup rates, as one might expect. A more dynamic entrepreneurial environment should generate

2 We examined other thresholds, such as say 500 miles as well. This did not qualitatively change our findings.

Table 1a: Descriptive Statistics

Variable	Period	Obs	Mean	StDev	Min	Max
Startup rates	1995-2000	341	8.40%	1.45%	4.99%	13.84%
	2001-2005		7.92%	1.63%	5.13%	15.15%
	2006-2011		6.86%	1.41%	4.11%	12.57%
Population growth, y/y	1995-2000	341	1.12%	1.09%	-1.33%	6.18%
	2001-2005		1.04%	1.08%	-0.68%	8.78%
	2006-2011		0.91%	0.82%	-1.56%	4.03%
Employment growth, y/y	1995-2000	341	2.29%	1.14%	-0.42%	7.01%
	2001-2005		0.75%	1.50%	-3.39%	7.56%
	2006-2011		-0.28%	0.93%	-2.79%	2.78%
In-migration (actual), #	1995-2000	374	8,674	19,473	0	167,462
	2001-2005		9,056	20,913	0	172,268
	2006-2011		9,516	22,060	0	184,753
In-migration (predicted), #	1995-2000	374	8,674	19,747	0	186,479
	2001-2005		9,056	20,688	0	196,067
	2006-2011		9,482	21,692	0	208,598
In-migration (predicted)/population (actual)	1995-2000	366	1.20%	0.94%	0.00%	5.57%
	2001-2005		1.21%	0.95%	0.00%	5.61%
	2006-2011		1.27%	1.00%	0.00%	6.04%

Sources: BLS, Census Bureau, IRS, Moody's Analytics

Table 1b: Pairwise Correlation, Pooled Cross-Section 1995-2011

	Startup rates	Population growth, y/y	Employment growth, y/y	In-migration (actual)	In-migration (predicted)	In-migration (predicted)/ population (actual)
Startup rates	1					
Population growth, y/y	0.7139	1				
Employment growth, y/y	0.6304	0.6021	1			
In-migration (actual), #	0.3973	0.1806	0.0898	1		
In-migration (predicted), #	0.3838	0.1484	0.0673	0.984	1	
In-migration (predicted)/population (actual)	0.5594	0.4289	0.2765	0.2938	0.3073	1

Sources: BLS, Census Bureau, IRS, Moody's Analytics

more jobs and that, in turn, may attract more in-migrants. Similarly, a bigger population means greater labor supply, which could allow more firms to open their doors.

Importantly for our purposes, total predicted in-migration based on historical migration patterns and scaled by population size is positively related to population growth (see Chart 2). This suggests that in-migration is a potential candidate as an instrument for population growth.

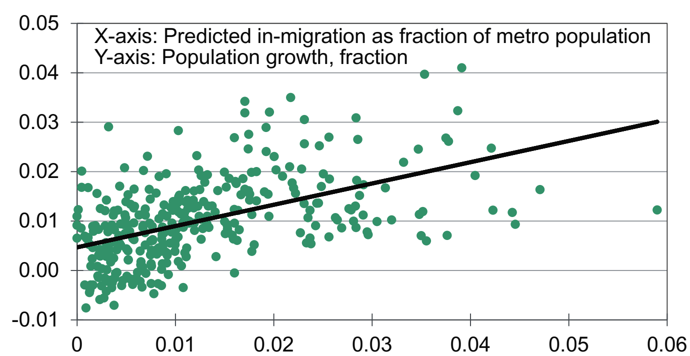
Empirical results

Ordinary least squares

To examine the basic determinants of firm formation, we begin by running a set of benchmark cross-section regressions of the average annual metro area firm startup rates from 1995-2011 against average annual employment growth and population growth. At this stage, we make no explicit effort to control for other factors causing firm formation or to identify causality (see Table 2, column 1)

Chart 2: Population Growth and In-Migration

U.S. 2000 metro areas, 1995-2011



Sources: Census Bureau, IRS, Moody's Analytics

We find a positive relationship between startup rates and both population growth and employment growth. Specifically, if annual population growth were to increase by

Table 2: Benchmark OLS Regressions - Firm Startup Rates

	(1) Full sample	(2) 1st quartile startups	(3) Interquartile startups	(4) 4th quartile startups
Employment growth	0.260***	-0.047	0.199***	0.584***
	(0.083)	(0.072)	(0.071)	(0.193)
Population growth	1.097***	0.473***	0.420***	0.269
	(0.081)	(0.120)	(0.082)	(0.168)
Constant	0.064***	0.059***	0.070***	0.084***
	(0.001)	(0.001)	(0.000)	(0.003)
N	341	62	214	62
R2	0.66	0.23	0.27	0.43

Sources: BLS, Census Bureau, Moody's Analytics. The dependent variable is the average, annual firm startup rate by metro area from 1995-2011. Employment growth is the average change in nonfarm payroll jobs. Numbers in parentheses are heteroscedasticity robust standard errors. Conventional levels of signficance are marked with asterisks as follows: *** 1%, ** 5%, * 10%. N indicates the number of observations and R2 is the adjusted R-squared.

1 percentage point in any given metro area, the result suggests that startup rates in said metro area should increase by about 1.1 percentage points. Since the average metro area in our sample experienced about 1% population growth, this implies that a doubling of population growth would increase firm formation by about 13%.

Comparatively speaking, the effect of employment growth is weaker. The baseline estimates suggest that a percentage point increase in employment growth would lead to an increase in firm formation of about 0.25 percentage point. This in turn implies that a doubling of job growth would increase firm formation by about 3%. Controlling for employment growth represents a first and important robustness test. If reverse causality is driving the results, meaning startup rates cause population growth, then the likely mechanism for this is through greater labor demand. Controlling for employment growth suggests reverse causality is at least not the full story.

Of course, these estimates come with copious amounts of salt. If there is any variable that affects both firm startups and either population growth or employment growth, these estimates will be biased, perhaps even severely. Potential candidates are not hard to come by: Say, for instance, that firm startups increase with the amount of local savings available to entrepreneurs. Further, say that the overall amount of saving in a metro area will increase in population size. By ignoring savings in the baseline regression, the estimated effect of population growth will then display upward bias. Other examples would include infrastructure, tax codes, and availability of skilled labor among others.

Since many such unobserved variables may exist, trying to include every single factor in a regression is tedious and hampered by technical problems. Measures of abstract but likely relevant factors, such as consumer sentiment shared by individuals living in the same space, are difficult to come by.

One simple, coarse method to allow for basic heterogeneity across metro areas is to repeat the exercise in column 1 in subsamples. In columns 2 through 4 of Table 2, we specifically split our sample into the bottom quartile, the interquartile range, and the top quartile by startup rate to separate the most dynamic business environments from the least dynamic ones. The basic result remains qualitatively unchanged. If anything, these results suggest that population growth has a stronger association with firm formation in those areas that have low startup rates and those in the middle percentiles (columns 2 and 3), while the association is statistically insignificant in the high startup areas (column 4). Importantly this suggests that the basic correlation is not just a result of a number of high-growth metro areas.

Our basic hypothesis states that population growth drives firm formation beyond merely adding to the labor supply available to local firms. While the results in Table 2 are consistent with this hypothesis, since we explicitly control for employment growth and find both factors to be significant, this only establishes correlation. However, before moving on to more sophisticated statistical approaches, there is one additional test of our hypothesis permissible in basic least squares: If population size indeed affects productivity and firm formation, one would expect the association to flow through only the *working-age* population. Alternatively, omitted variables like higher savings availability could operate just as easily through growth in the population of older workers. To investigate this notion we repeat the baseline regressions from Table 2, but replace population growth with the average annual percentage change in each age bracket of the working-age population past 25 (see Table 3).

The most robust association between growth in population shares and startup rates occurs in the age ranges of 35-44 and 55-64, with the exception of the top quartile (column 4). A growing share of the relatively young (25-34) and the mid-range (45-54) appears to be insignificant across subsamples. To a degree, this finding is intuitive at least for the younger 25-34 range. Workers in the middle of their career are more relevant for firm formation, as they bring more capital and expertise to the plate compared with relative beginners. But this cannot explain the insignificant coefficient estimate in the group of 45-54. In reality, this finding results from the limitations of cross-sectional regression. The 35-44 and 45-54 population groups are almost multicollinear. If the share of those age 35-44 is excluded, for instance, popula-

Table 3: Benchmark OLS Regressions - Firm Startup Rates, by Age Share

	(1) Full sample	(2) 1st quartile startups	(3) Interquartile startups	(4) 4th quartile startups
Employment growth	0.317***	-0.06	0.151*	0.583**
	(0.117)	(0.098)	(0.087)	(0.252)
Population growth in 25-34	-0.069	-0.020	0.065	-0.067
	(0.104)	(0.124)	(0.071)	(0.196)
Population growth in 35-44	0.538***	0.341**	0.145*	0.158
	(0.113)	(0.148)	(0.083)	(0.221)
Population growth in 45-54	0.142	-0.072	-0.070	0.452*
	(0.149)	(0.149)	(0.114)	(0.258)
Population growth in 55-64	0.325***	0.282*	0.224***	-0.060
	(0.096)	(0.156)	(0.072)	(0.127)
Population growth in 65+	0.093	-0.043	0.089	-0.225*
	(0.082)	(0.101)	(0.057)	(0.113)
Constant	0.06	0.058***	0.067***	0.083***
	(0.003)	(0.003)	(0.002)	(0.009)
N	341	62	214	62
R2	0.65	0.2	0.3	0.54

Sources: BLS, Census Bureau, Moody's Analytics. The dependent variable is the average, annual firm startup rate by metro area from 1995-2011. Employment growth is the average change in nonfarm payroll jobs. Population growth by age range is the annual percentage change in the age share in each age bracket. Numbers in parentheses are heteroscedasticity robust standard errors. Conventional levels of signficance are marked with asterisks as follows: *** 1%, ** 5%, * 10%. N indicates the number of observations and R2 is the adjusted R-squared.

tion growth in the group of those age 44-54 displays a significant and positive relationship with startup rates. It is, thus, not clear that any specific group in the working-age range is more relevant than another. However, the striking result in Table 3 is that growth in the share of age 65 and older individuals is either insignificant, or in some instances negatively associated with firm startups. This result is highly suggestive: If a metro area is gaining in its share of retirement-age population, be it by virtue of low birthrates, out-migration of the young, or in-migration of older individuals to retirement communities, said metro area has either no higher or potentially even lower startup rates.

Panel regressions

To first address the empirical problems of ordinary least squares, we run a set of panel regressions replicating the specifications in Tables 2 and 3. The main innovation herein is that the panel setting allows us to remove any unobserved time-invariant omitted metro area characteristic that could bias the coefficient estimates presented earlier.

We revisit our benchmark model, once as a random effects specification, which is in essence a cross-section repeated over time, and once as a fixed effects specification, which controls for metro-level differences that do not vary over time. This approach is equivalent to using variables measured as deviations from metro-area mean rather than absolute values (see Table 4).

Two things are apparent from columns 1 and 2 of Table 4. First, basic specification tests prefer fixed over random effects, which falls in line with the expectation that metro area startup rates are associated not only with employment and population growth, but with a slate of potential other variables.[3] At the same time, the difference between both models is minimal. While the coefficient estimates on employment growth are virtually the same, the estimated effect of population growth is slightly smaller in the fixed effects specification. Not finding any major difference between the two approaches, the remaining models in this article, hence, employ fixed rather than random effects.

Quantitatively, the estimated relationships are noticeably weaker in the basic panel than in the cross-section, which, given the aforementioned concerns surrounding omitted variables, is to be expected. Repeating the split into quartiles (columns 3-5) yields significant coefficient estimates for all quartiles, including a significant, positive effect in the upper quartile as well. Notice that we allow metro areas to switch from one quartile to another in any given year.

Finally, we repeat the age distribution regressions from above in a fixed panel specification (see Table 5). Again, the most significant group is of working age, but growth in the share of the 25-34 group is also a significant contributor in the panel setting. As before, retirement populations are not positively associated with startup rates in a robust manner and, in some specifications, display a negative coefficient estimate.

Instrumental variables

Results in Parts 1 and 2 of this section establish a basic plausible, cross-sectional relationship between population growth and startup rates, specifically controlling for employment growth and the age distribution of workers. Results in Part 2 address the likely problem of omitted variable bias and provides more accurate quantitative predictions

3 Based on the p-value of the Breusch-Pagan Lagrangian Multiplier Test (1979), we reject the null hypothesis of random effects in favor of the alternative hypothesis of fixed effects.

Table 4: Benchmark Panel Regressions - Firm Startup Rates

	(1)	(2)	(3)	(4)	(5)
	Full sample		1st quartile startups	Interquartile startups	4th quartile startups
	RE	FE	FE	FE	FE
Employment growth	0.054***	0.054***	0.002	0.021***	0.047***
	(0.008)	(0.008)	(0.009)	(0.007)	(0.014)
Population growth	0.172***	0.132***	0.055**	0.043**	0.220***
	(0.044)	(0.037)	(0.026)	(0.022)	(0.050)
Constant	0.084***	0.085***	0.065***	0.081***	0.098***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
N	341	341	271	333	205
T (avg)	17	17	5.3	8.7	7.1
Pseudo-R2	0.46	0.71	0.5	0.66	0.46
Breusch-Pagan Lagrangian Multiplier Test		0.000			

Sources: BLS, Census Bureau, Moody's Analytics. The dependent variable is the annual firm startup rate by metro area. The sample ranges from 1995-2011. Employment growth is the average change in nonfarm payroll jobs. Numbers in parentheses are heteroscedasticity robust standard errors, clustered at the metro level. Conventional levels of signficance are marked with asterisks as follows: *** 1%, ** 5%, * 10%. N indicates the number of observations and T indicates the average number of time periods per group. The number for the Breusch-Pagan Lagrangian Multiplier Test is the p-value of a null hypothesis of random effects versus the alternative of fixed effects. RE indicates a random effects specification, and FE indicates fixed effects.

Table 5: Benchmark Panel Regressions - Firm Startup Rates by Age Group

	(1) Full sample	(2) 1st quartile startups	(3) Interquartile startups	(4) 4th quartile startups
Employment growth	0.047***	0.000	0.020***	0.051***
	(0.007)	(0.009)	(0.007)	(0.012)
Population growth in 25-34	0.070***	0.042***	0.040***	0.085***
	(0.011)	(0.010)	(0.009)	(0.018)
Population growth in 35-44	-0.014	-0.013	-0.008	0.043
	(0.017)	(0.020)	(0.015)	(0.038)
Population growth in 45-54	0.069***	-0.016	0.013	0.140***
	(0.018)	(0.025)	(0.013)	(0.030)
Population growth in 55-64	0.031**	-0.048**	0.009	-0.042
	(0.016)	(0.020)	(0.015)	(0.030)
Population growth in 65+	-0.062***	0.048*	-0.047***	-0.086***
	(0.022)	(0.026)	(0.016)	(0.031)
Constant	0.085***	0.065***	0.082	0.097***
	(0.001)	(0.002)	(0.001)	(0.002)
N	341	271	333	205
T (avg)	17	5.3	8.7	7.1
Pseudo-R2	0.73	0.51	0.69	0.50

Sources: BLS, Census Bureau, Moody's Analytics. The dependent variable is the annual firm startup rate by metro area. The sample ranges from 1995-2011. Employment growth is the average change in nonfarm payroll jobs. Numbers in parentheses are heteroscedasticity robust standard errors, clustered at the metro level. Conventional levels of signficance are marked with asterisks as follows: *** 1%, ** 5%, * 10%. N indicates the number of observations and T indicates the average number of time periods per group. All specifications are fixed effects models.

Table 6: Instrumental Variable Regressions - Firm Startup Rates

First stage regressions - Population growth

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		Fixed effects		System GMM	
First stage: Firm startup rates						
Employment growth	0.721***	0.698***	0.128***	0.128***	NA	NA
	(0.057)	(0.057)	(0.016)	(0.016)		
Instrument/pop	0.093**	0.309***	0.636***	1.357***		
	(0.024)	(0.086)	(0.190)	(0.428)		
(Instrument/pop)^2		-5.363***		-13.495**		
		(1.876)		(6.856)		
Second stage: Firm startup rates						
Population growth	6.421***	5.340***	0.723**	0.834***	1.007***	1.042***
	(2.228)	(1.281)	(0.322)	(0.292)	(0.122)	(0.121)
Employment growth	-3.900**	-3.059***	0.147***	0.133***	0.227***	0.225***
	(1.796)	(.1033)	(0.040)	(0.036)	(0.019)	(0.019)
N	341	341	341	341	341	341
Specification tests						
Underidentification	NA	NA	0.005	0.011	NA	NA
Weak instruments	5.15	6.96	48.06	30.97	NA	NA
Overidentifying restrictions	NA	0.35	NA	0.53	0.13	0.14
AR(1)	NA	NA	NA	NA	0.01	0.01
AR(2)	NA	NA	NA	NA	0.29	0.34

Sources: BLS, Census Bureau, IRS, Moody's Analytics. The dependent variable is the firm startup rate by metro area (average in OLS, annual in the panel specification). The sample ranges from 1997-2011. Employment growth is the average change in nonfarm payroll jobs. Numbers in parentheses are either heteroscedasticity robust standard errors, or clustered at the metro level. Conventional levels of signficance are marked with asterisks as follows: *** 1%, ** 5%, * 10%. N indicates the number of observations. The constant term is not reported for OLS and system GMM, and transformed out for the panel fixed effects regression. The underidentification test for the fixed effects is the p-value of the Kleinberg-Paap rk LM statistic with the null of underidentification. Weak instruments are either robust F-statistics of the first stage or the Cragg-Donald-Wald statistics. The test for overidentifying restrictions is the p-value of the Hansen J-Statistic. AR(1) and AR(2) are Arellano-Bond tests with the null hypothesis of autocorrelation of the first and second order. A correctly specificed dynamic panel should display first-order autocorrelation, induced by first differencing, but reject second-order autocorrelation. All models are estimated using the generalized method of moments. System GMM further uses the robust two-step estimator. First stage system GMM estimates are ommitted due to the larger number of instruments.

to the role of population and employment growth in the context of firm formation.

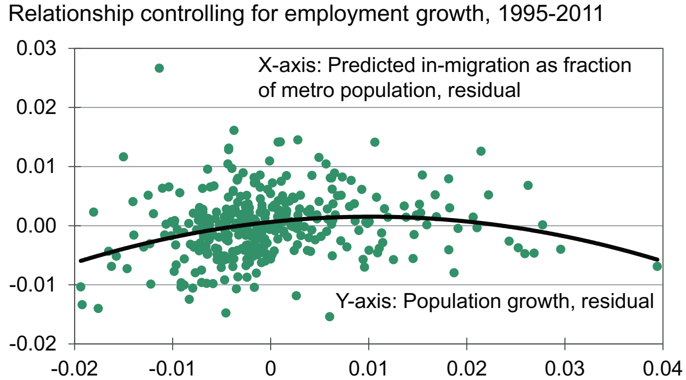
In this third part, we turn to perhaps the most relevant question: What causes what? Specifically we report three sets of instrumental variable regressions, relying on predicted in-migration based on historical patterns from 1990-1994 as an instrument. First, we instrument for population growth in a basic cross-section. Second we repeat this exercise in a fixed effect panel setting. Finally, to include additional instruments, we employ a dynamic, system generalized methods of moments (GMM) panel specification along the lines of Blundell and Bond (2000) (see Table 6).

For each setting, we present two variations, one including only the level of the predicted in-migration/population ratio as an instrument, and one including both the level and its square term. There are two motivations for doing the latter. First, the relationship between population growth and predicted in-migration appears roughly quadratic (see Chart 3). Further, for an instrument to pass the standard specification tests, two criteria need to be met:

- The instrument needs to sufficiently explain the potentially endogenous variable. This is typically tested using the F-statistic of a first stage regression of the endogenous variable against the instrument and comparing it to critical values established by Stock and Yogo (2005) in cross-section, or by obtaining the Cragg-Donald (1993) Wald F-statistic in a panel setting. These tests are referred to as weak instrument tests.
- Second, the instrument has to be uncorrelated with the error of a second stage

Chart 3: Population Growth and In-Migration

Relationship controlling for employment growth, 1995-2011



Sources: BLS, Census Bureau, IRS, Moody's Analytics regression of the variable of interest against the potentially endogenous variable. Depending on the setting, this is usually tested via a Sargan test (1958) or Hansen J-test (1982). These tests are referred to as tests for over-identifying restrictions. Since more than one instrument is required for this second category of specification tests, we also rely on the square of our instrument.

The cross-sectional results in columns 1 and 2 of Table 6 are suggestive, especially when the quadratic term is included, suggesting that the effect of in-migration on population growth is potentially nonlinear. The second-stage results suggest that the exogenous component of population growth has a positive and significant effect on startups. However, the large coefficient estimates on population growth and the negative estimate for employment growth are warning signs. Either the instrumented variable induces multicollinearity, or we have a weak instrument problem in the cross-section. While the F-statistic of the first stage regression passes at conventional levels of significance in column 2, it does not meet the stronger criterion of about 10 demanded by Stock and Yogo (2005). The remaining specification tests, however, do not suggest problems. Specifically, the test for over-identifying restrictions does not reject the null hypothesis of over-identification, suggesting that historical patterns of migration are indeed exogenous to firm startup rates.

With the reservations of the simple cross-section in mind, we move on to the panel IV-results (column 3 and column 4). Using only a single instrument yields results strongly in favor of our hypothesis: Predicted in-migration is positively associated with population growth in the first-stage regression, and the predicted strength almost matches that of the cross-section.[4] Further, a Cragg-Donald Wald F-statistic above 45 quells any fears of weak instrument problems, given a critical value of about 20. Adding a squared term to allow for a test of over-identifying restrictions restores the by now familiar quadratic pattern, and easily passes both the weak instrument test, and the test for over-identifying restrictions itself. These findings are the most central ones presented in this article.

As an alternative method to instrument for population growth, we rely on a basic dynamic panel estimation employing the system GMM estimator developed by Blundell and Bond (2000). A technical definition of this approach exceeds the scope of this article, but a brief description is in order. The system GMM estimator falls into the category of dynamic panels pioneered by Arellano and Bond (1991), which remove the fixed effect of a series by relying on first differences from one point in time to another. Since such first differencing induces endogeneity (the error of the differenced dependent variable is a function of its own lag), these authors suggest using the lags of the dependent variable, starting with the second, as instruments for its differences. Blundell and Bond (2000) add a level equation to this mix, which in turn employs lagged differences as instruments for these levels. The motivation for a level equation stems from the fact that lags tend to be weak instruments for first differences for highly persistent series such as macroeconomic phenomena.

In addition, the dynamic panel approach allows for the inclusion of other instruments, such as our predicted patterns of in-migration. The results in columns 5 and 6 of Table 6 confirm our previous finding with and without a square term of historical in-migration. This can be taken as evidence that the instrument works fine in level terms, as it passes both tests for weak instruments and over-identifying restrictions. Quantitatively, the dynamic panel results are remarkably similar to those in cross-section, suggesting a robust positive effect running from population growth to firm startup rates.

Conclusion

This article is a first attempt to causally link population growth to firm formation, yet much work remains to be done. Arguably a second instrument along the lines of that proposed in Card (2001) would allow for stronger identification than relying on variations of a single instrument proposed here. Similarly, further stratification of the sample in the instrumental variable regressions, such as by age group, will help to better understand the link between demographics and population growth. We will address these issues in a forthcoming paper.

4 The results are not perfectly comparable since the fixed effect IV regressions omit a constant term in an effort to avoid potential identification issues. Qualitatively, however, this omission is not material.

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