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Does Intergenerational Mobility Increase Corporate Profits?*

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Abstract

We find that firms located in areas with higher intergenerational mobility are more profitable. Building off the work of Chetty and Hendren (2018a and 2018b)—who provide measures of intergenerational mobility for all commuting zones (essentially, metropolitan areas) within the U.S.—we are the first to show the positive association between intergenerational mobility and corporate profitability. Our regressions compare firms in the same industry at the same point in time and fully control for time-varying state-level shocks. As such, our findings cannot be explained by either differences in industry composition across localities or by variation in state-level economic conditions; nor can our results be explained by differences in firm characteristics or by local economic conditions. Rather, we argue that our findings are best explained by intergenerational mobility influencing human capital formation. Areas with higher mobility do a better job in unlocking their residents' innate talents, which in turn is associated with improved performance by locally headquartered firms. In essence, our results uncover a positive link between greater equality of opportunity and increased corporate profitability.

JEL classification: J62, J24, G30, G32, R10
Keywords: Intergenerational mobility; corporate profitability; human capital

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1. Introduction

In this paper, we examine the link between intergenerational mobility and corporate profits. Our analysis builds off seminal work by Chetty et al. (2014a) and Chetty and Hendren (2018a and 2018b), who show that intergenerational mobility—i.e., the degree to which people's income in adulthood is determined by the earnings of their parents—varies substantially across areas within the United States.

One interpretation of their findings is that areas with higher observed intergenerational mobility simply do a better job at unlocking the innate talents of people growing up there, such that the economic circumstances into which people are born are less determinative of their economic success later in life. Or, to express the same concept negatively, areas with lower intergenerational mobility stifle human capital formation. Accordingly, we hypothesize that if areas with greater mobility indeed do a better job in unlocking their residents' innate talents, then the overall stock of human capital in such areas should be greater. As a consequence, since the supply of available talent is likely to affect corporate performance, firms located in areas with higher intergenerational mobility should, all else equal, be more profitable. Consistent with this hypothesis, we find substantial support for exactly such a positive association between intergenerational mobility and corporate profits. Our results therefore indicate that greater equality of opportunity need not come at the expense of corporate profits; rather, firms are more profitable in areas where equality of opportunity is greater.[1]

Specifically, our tests examine the relationship between the profitability of public firms, as measured by their return on assets (ROA), and intergenerational mobility at the commuting zone

[1] In an extreme case, the benefits of higher worker human capital may accrue entirely to workers in the form of higher wages, implying no effect on firm profitability; in practice, however, firms have sizeable bargaining power, and thus would be expected to capture a substantial share of the benefit.

(CZ) level,[2] based on the location of the firm's headquarters. In these tests, we take numerous precautions to ensure that the relationship between corporate profits and intergenerational mobility that we uncover is not confounded by obvious omitted variables. First, our regressions include industry-by-year fixed effects, which control for all time-varying industry-level shocks and so ensure that our results cannot be explained by differences in industry composition across localities. Second, we also include state-by-year fixed effects in our regressions; thus, our tests always compare firms located within the same state at the same point in time, guaranteeing that neither time-invariant state-level characteristics nor time-varying state-level economic shocks or policy changes can account for our findings. In essence, after adjusting for industry-level variation, our tests identify the effects of mobility on corporate profits by comparing firms located in different CZs (basically, metropolitan areas) within the same state. Third, our results are robust to controlling for a host of firm-level characteristics. Fourth, the results are also robust to controlling for local economic conditions at the CZ-level, as measured by the local unemployment rate, population growth, and income growth.[3] Below, we further discuss other potential confounds that might affect our results.

Within this framework, we take as explanatory variables measures of CZ-level intergenerational mobility from Chetty and Hendren (2018a and 2018b). Our baseline mobility measure captures the "relative mobility" between people growing up in the same CZ depending on their parents' income. Specifically, for each CZ, this measure is defined as the difference between: (i) the average percentile rank in the national income distribution in adulthood for individuals whose parents' incomes are the 25th percentile of the income distribution; versus (ii)

[2] CZs are geographical aggregations of counties that are similar to metropolitan areas but cover the entire United States used in Chetty et al. (2014) and Chetty and Hendren (2018a and 2018b).

[3] The latter result is consistent with Chetty et al.'s (2014) finding that there is no systematic correlation between mobility and local labor market conditions.

the same measure for those whose parents' incomes are at the 75th percentile. To adopt the terminology of Chetty et al. (2014a), measures (i) and (ii) separately can be described as "absolute mobility" at a given percentile rank (in this case, 25th and the 75th percentiles).[4] In addition to testing for the effects of relative mobility, we also examine the effects of these absolute mobility measures. The baseline mobility measures we use are computed by Chetty and Hendren (2018a) from the subset of the population who don't move CZs during childhood – i.e., "permanent residents."[5] The larger our relative mobility measure, the better people from lower-middle-class families (i.e., those whose parents' incomes are at the 25th percentile) are expected to do relative to people from upper-middle-class families (i.e., those whose parents' incomes are at the 75th percentile). One advantage of the relative mobility measure that we use is that any local characteristics that affect the upper-middle-class and lower-middle-class in the same way would be "differenced out;" thus, the measure captures what traditionally may be thought of as "intergenerational mobility" – i.e., how people from poorer families do compared to people from richer families. (Below, we also consider measures of mobility based on college attendance at the 25th and 75th percentiles.)

We find that a one standard deviation increase in our baseline measure of relative mobility (i.e., better mobility) is associated with a 2.5 percentage point increase in profitability (ROA), compared to a sample mean of 7.7 percent. In other words, this key result indicates that firms are significantly more profitable in areas where intergenerational mobility, and thus equality of opportunity, is higher.

[4] Specifically, for each separate CZ, Chetty et al. (2014a) and Chetty and Hendren (2018a) model a person's expected rank in the national income distribution in adulthood as a linear function of their parents' rank; CZ-level measures of absolute mobility at a given parental percentile rank (e.g., the 25th and 75th percentiles) are therefore based on fitted values from this regression. Hence the baseline relative mobility measure that we use captures differences across CZs in the *slope* of the relationship between expected individual percentile ranks and parental percentile ranks.
[5] As explained below, we also separately consider mobility measures based on those who move to different CZs, as estimated in Chetty and Hendren (2018b).

Interestingly, when we decompose this relative mobility measure to separately examine the effect of absolute mobility for people whose parents are at the 25th and 75th percentiles of the income distribution, we find that the effects run in opposite directions. As absolute mobility at the 25th percentile increases, so does corporate profitability – a one standard deviation increase in this measure is associated with a 2.6 percentage point increase in profitability. Thus, corporations are more profitable in areas where people from poorer families are more upwardly mobile. In contrast, a one standard deviation increase in absolute mobility at the 75th percentile is associated with a 5.3 percentage point *decline* in profitability. We regard the latter result as particularly striking. Of note is that, for the average CZ, a person whose parents are at the 75th percentile of the income distribution is expected themselves to fall to the 57th percentile. CZs with higher mobility at the 75th percentile may therefore be interpreted as ones with less *downward mobility* among those from richer families, where higher parental incomes effectively insulate people from falling too far in the income distribution. Our results suggest, therefore, that less downward mobility among those from richer families may come at the expense of broader, society-wide, human capital formation, which in turn manifests itself in lower corporate profitability.

Based on the work of Chetty and Hendren (2018a), we can be confident that the mobility measures that we use reflect the causal effects of CZs on their residents' mobility. An alternative hypothesis would be that observed variation in mobility across CZs is due to systematic differences in the types of people living in each area (i.e., selection). However, Chetty and Hendren (2018a) convincingly reject the latter hypothesis; their analysis exhaustively demonstrates that the majority of the variation in mobility across areas is due to causal effects of place.[6]

[6] Specifically, by exploiting variation in the age at which children move to different CZs, Chetty and Hendren (2018a) show that the outcomes of movers improve linearly in proportion to the amount of time spent in better areas (as measured by the outcomes of those already living there—i.e., permanent residents). They identify the causal effects

In an important robustness check, we confirm that the association between corporate profits and mobility also reflects the causal effects of CZs on resident mobility (as opposed to selection). Specifically, we use alternative measures of mobility estimated in Chetty and Hendren (2018b) based on the subset of families who move to different CZs. Chetty and Hendren (2018b) estimate the causal effect of each separate CZ on mobility by measuring differences in outcomes based on the age at which children move.[7] When we examine the association between corporate profits and relative mobility using these causal mobility estimates, we find similar results to our baseline specification. In particular, a one standard deviation increase in this causal relative mobility measure (i.e., better mobility) is associated with a 1.9 percentage point increase in profitability. The result indicates that the link between corporate profits and intergenerational mobility is not driven by differences in the types of people living in each area, but rather by CZs having causal effects on resident mobility.

A potential remaining concern is that even though the mobility measures we use reflect the causal effects of place on resident outcomes, the association between mobility and profits that we present is nonetheless correlational in nature. This raises the possibility that the relationship between corporate profits and mobility may be driven by reverse causality. For example, it may be that more profitable firms cause nearby residents to have higher mobility—i.e., more profitable firms create job opportunities, which may raise mobility. While we cannot completely rule out this possibility, we believe that, on the whole, the direction of causality predominantly runs in the other direction—that is, from mobility to corporate profits (rather than vice-versa). Several pieces of evidence support this view. First, we find that although the association between corporate profits

of neighborhoods by comparing the outcomes of siblings within families (using family fixed effects) and studying moves triggered by displacement shocks, among other tests.

[7] Their specifications include origin-by-destination fixed effects.

and mobility is positive for those with parental incomes at the 25th percentile, it is negative for those with parental incomes at the 75th percentile. We regard this finding as especially important in strengthening the case against reverse causality as an explanation of our results. For the reverse causality explanation to hold, more profitable firms would therefore have to simultaneously cause greater upward mobility among those from poorer families and *less* upward mobility among those from richer families. This seems implausible.

Furthermore, as noted above, our estimates are robust to controlling for local economic conditions (including income growth); a result that mirrors the finding in Chetty et al. (2014a) that there is no systematic correlation between mobility and local labor market conditions. This suggests that our findings cannot be explained by more profitable firms improving local labor markets, and in turn having a notable positive effect on local mobility (as implied by a reverse causality explanation).

In addition, Chetty and Hendren (2018b) identify several major determinants of mobility that are unlikely to be directly affected by the profitability of locally headquartered firms, giving us further confidence that our results are not driven by reverse causality. Specifically, they show that worse mobility is associated with: high residential segregation (both race and income based), more urban sprawl, more uneven distribution of income, lower school quality (adjusted for parental income), and lower measures for social capital (e.g. based on the index by Rupasingha and Goetz, 2008).[8] All of these can broadly be described as environmental factors, reflecting differences in local institutions and culture. In other words, it appears that mobility is largely determined by CZ characteristics that are more fundamental than the profitability of locally headquartered firms. In

[8] These determinants of intergenerational mobility remain statistically significant even in specifications including state fixed effects (Chetty et al., 2014a), meaning that the same determinants remain important in our setting, given that we also absorb-out state level variation.

sum, although the relationship between corporate profits and intergenerational mobility that we present is correlational in nature, we believe that a reasonable conclusion, based on the totality of the evidence, is that the direction of causality predominantly runs from mobility to corporate profits (and not vice-versa).

In our final test, we present further evidence that human capital formation is the channel through which mobility and firm profitability are connected. Specifically, in addition to the mobility measures already discussed, Chetty and Hendren (2018b) also provide causal estimates of CZs on college attendance rates for people with parental incomes at the 25th and 75th percentiles. When we use these measures as explanatory variables within the same regression framework as before, we find a strong positive relationship between college attendance at the 25th percentile and corporate profitability (while college attendance at the 75th percentile has no effect). A one standard deviation increase in a CZ's estimated effect on college attendance at the 25th percentile is associated with a 1.3 percentage point increase in profitability. That is, firms are more profitable in areas where people from poorer families have more educational opportunities. Such areas may be interpreted as ones that are better able to unlock their residents' innate talents, where, to the benefit of locally headquartered firms, there is a greater supply of human capital.

Our paper connects to two main literatures. First, and most obviously, we contribute to the literature on intergenerational mobility—reviewed by Solon (1999) and Black and Devereux (2011), and extended by Chetty et al. (2014a) and Chetty and Hendren (2018a and 2018b). Our contribution, in essence, is to show empirically that the debate over equality of opportunity, and the extent to which society reflects meritocratic ideals, is not only about fairness or equity; rather, greater equality of opportunity also appears to benefit firms and shareholders in the form of high profitability.

In this sense, our paper provides a link between the literature on intergenerational mobility and research emphasizing the importance of human capital in fostering economic development and growth, going back to theoretical contributions by Lucas (1988) and Romer (1990), among others. Empirical work in this area has shown that higher average educational attainment is positively associated with regional economic performance (Gennaioli et al., 2014), and has positive spillover effects on worker wages and firm productivity (Moretti 2004a, 2004b, 2004c).[9] We add a new dimension to this literature by using measures of human capital formation based not on average educational attainment but rather on intergenerational mobility. A crucial lesson from our analysis, therefore, is that a key channel that connects economic performance with human capital formation is the degree of equality of opportunity within society.

2. Data

Our analysis relies on two principal data sources: CZ-level measures on intergenerational mobility based on the analysis of Chetty and Hendren (2018a and 2018b), and firm-level data on U.S. public companies from Compustat. We supplement these with data on local economic outcomes from the Bureau of Economic Analysis (BEA) and the Bureau of Labor Statistics (BLS).

2.1. Measures of intergenerational mobility

Chetty et al. (2014a) and Chetty and Hendren (2018a and 2018b) estimate intergenerational mobility at the CZ-level using anonymized tax records linking the earnings of tens of millions of parents and children.[10] Here, we provide a brief overview of their data and estimation strategy

[9] Related work on the role of human capital at the city- and regional-level has emphasized the benefits of agglomeration and "knowledge spillovers" in explaining plant productivity (Greenstone, Hornbeck, and Moretti, 2010) and firm investment patterns (Dougal, Parsons, and Titman, 2015); see also, Glaeser and Mare (2001), Berry and Glaeser (2005), and Glaeser (2012).
[10] The estimates are publicly available at https://opportunityinsights.org/data/. The authors also provide mobility estimates at the county-level. However, we focus our analysis on the CZ-level because CZs (by design) better reflect

insofar as it relates to our analysis (the reader is referred to their papers for further details). Chetty et al.'s full sample covers children born 1980-1988, whose parental income is measured as the average over the five year period spanning 1996-2000. For each CZ, they estimate measures of intergenerational mobility by relating children's percentile rank in the national income distribution in adulthood to their parents' rank, doing so for both "permanent residents" (parents who remain in the same CZ during the period 1996-2012) as well as for "movers". Their mobility estimates for permanent residents are obtained from running the following regression:

(1) $$y_i = \alpha_{cs} + \beta_{cs} p_i + \varepsilon_i$$

where y_i is a person's percentile rank in the income distribution, p_i is their parents' rank, and α_{cs} are CZ-by-birth-cohort fixed effects. The coefficient β_{cs} varies by CZ (and birth cohort), and provides a measure of "relative mobility" for each CZ—i.e., the degree to which people from higher-income families are expected to earn more in adulthood relative to people from lower-income families growing up in the same CZ. In their online data, Chetty et al. provide measures of "absolute mobility" at the 25th and 75th percentiles for each CZ, based on fitted values from the above regression (i.e., p_i=25 and p_i=75). For each CZ, these measure a person's expected rank in the income distribution conditional on their parents' percentile rank. The baseline mobility measure that we consider is one of relative mobility, which we compute as the difference, within each CZ, between absolute mobility at the 25th and 75th percentiles. As such, higher values of this measure represent better relative mobility—i.e., there is a smaller gap in expected outcomes for those at the 25th percentile compared to those at the 75th. An advantage of the relative mobility measure is that any local characteristics that affect poorer versus richer people growing up in the same CZ in the same way would be "differenced out," thus giving us a purely relative measure of

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the boundaries of the local labor market, and hence the supply of human capital available to locally headquartered firms.

how people from poorer families do compared to people from richer families. Nonetheless, we also examine the effects of absolute mobility at the 25th and 75th percentiles.

Chetty et al.'s baseline mobility measures are estimated using a person's income in adulthood at the age of 26. We use these as our baseline mobility measures as well. In robustness, we also use alternative measures provided by Chetty et al., where mobility is estimated based on a person's income at the age of 30.

An important issue in our context is whether to use mobility estimates based on a person's household income (at the age of 26 and 30) or their individual income. Essentially, our aim is to use CZ-level measures of intergenerational mobility as a way of gauging the extent to which the local environment is able to unlock its residents' innate talents, thus contributing to the overall stock of human capital within the CZ. As such, we use mobility estimates based on a person's individual income rather than their household income, since the latter will be strongly influenced by differences in the likelihood of marriage (at the age of 26) and therefore may not be a good measure of CZ-level human capital formation.[11]

In addition to the mobility of permanent residents, Chetty and Hendren (2018b) also provide mobility estimates based on movers—i.e., those who move CZs during childhood. As discussed in the introduction, these estimates address the concern that variation in intergenerational mobility may reflect differences in the types of people living in each area, and thus may be due to selection as opposed to causal effects of CZs on their residents' mobility. The analysis in Chetty and Hendren (2018a) thoroughly rejects the latter hypothesis by showing that neighborhoods have significant childhood exposure effects on mobility; i.e., their main test shows that the outcomes of movers converge linearly to the outcomes of permanent residents based on the amount of time

[11] For more evidence on the role of marriage patterns in explaining household measures of intergenerational mobility, see Rothstein (2019).

spent in each area, even in specifications comparing the outcomes of siblings within families (using family fixed effects). Chetty and Hendren (2018b) extend this logic by estimating, for each CZ, the causal effects of place on mobility based on the subset of movers, exploiting differences in the age at which children move for identification purposes (in specifications that include origin-by-destination fixed effects as well as other controls). In their online data, Chetty et al. provide these causal estimates of absolute mobility for people with parental incomes at the 25th and 75th percentiles. We therefore use these mobility estimates based on the subset of movers in robustness checks, examining the effects of both relative and absolute mobility on corporate profits.[12] In the same way, Chetty and Hendren (2018b) also provide causal estimates of college attendance rates (between the ages of 18 and 23) again based on the subset of movers, for people with parental incomes at both the 25th and 75th percentiles. We use these as additional explanatory variables to examine the link between human capital formation and corporate profitability.

Table 1, Panel A, contains summary statistics of the CZ mobility measures. Our firm-level sample (described below) links to 309 CZs.[13] As Panel A shows, there is notable variation in intergenerational mobility across CZs. For example, consider the mobility of permanent residents (those who don't move CZs during childhood): while for the average CZ a person with parental income at the 25th percentile of the national income distribution is themselves expected to end up at roughly the 44th percentile at age 26, the standard deviation of this measure across CZs in 3.7. Indeed, for this mobility measure, comparing a relatively high mobility CZ (i.e., a CZ at the 75th percentile) to a relatively low mobility CZ (one at the 25th percentile), a person's expected

[12] In particular, we use Chetty and Hendren's (2018b) raw fixed effects estimates based on individual income at age 26 for people with parental incomes at the 25th and 75th percentiles; estimates using income at age 30 are not available for these measures.
[13] The number of CZs in our sample is therefore less than the total number of CZs in the U.S. (741) because many CZs (e.g., rural CZs) are not home to any publicly traded firms.

individual rank falls from approximately the 47th to 42nd percentile—a rather considerable difference in expected outcomes. Likewise, there is notable variation in the mobility of permanent CZ residents with parental income at 75th percentile—while for the average CZ such an individual is expected to themselves fall to roughly the 57th percentile by age 26, some CZs exhibit less "reversion to the mean" than others. All of this translates into notable variation in our baseline relative mobility measures (the difference in expected percentile ranks for permanent CZ residents with parental income at the 25th versus the 75th percentile, as measured at age 26). Looking at the mobility of permanent residents as measured at age 30, one sees similar patterns; indeed, the descriptive statistics are only slightly different from those for mobility as measured at age 26, just as in Chetty and Hendren (2018a). Figure 1 maps our baseline relative mobility measure, illustrating how mobility varies geographically.

Chetty and Hendren's (2018b) mobility estimates based on cross-CZ movers, also shown in Panel A of Table 1, are measured on a different scale compared to those of permanent residents. In particular, the data represent the estimated exposure effects from spending one more year of childhood in a given CZ. For example, for movers with parental income at the 25th percentile, spending an additional year during childhood in the average CZ is estimated to raise their percentile rank at age 26 by 0.07.[14] Again, however, there is notable variation across CZs: for every additional year in a CZ at the 75th percentile, such a person's percentile rank is estimated to rise by 0.20, while every additional year in a CZ at the 25th percentile is estimated to lower such a person's percentile rank by 0.16. Analogously, there is also notable variation in movers' relative mobility, and in the estimated effects of CZs on college attendance rates. Geographic variation in

[14] These numbers could be multiplied by 20, to get a rough approximation of the effect of spending one's entire childhood in a given CZ; for caveats to such an exercise, see Chetty and Hendren (2018b).

both movers' relative mobility and relative college attendance rates are shown in Figures 2 and 3, respectively.

In our regressions, to facilitate easy comparison of the effects of each of these mobility measures, we standardize by dividing each one by its standard deviation, as reported in Panel A of Table 1.

2.2. Firm-level data

Our firm-level sample consists of U.S. public companies listed on the NYSE, Amex, or Nasdaq during the 1990-2014 fiscal years,[15] as contained in the merged CRSP-Compustat Fundamentals Annual database and subject to the following filters.[16] Following the literature, we exclude financial firms (SIC 6), utilities (SIC 49), public-sector entities (SIC 9), non-U.S. firms, firms with missing or negative assets, and firms with missing return on assets.

Our main measure of profitability is return on assets (ROA), which is defined as earnings before interest, tax, depreciation and amortization (EBITDA) divided by total assets. For robustness, we also consider an alternative measure of profitability, defined as net income divided by total assets.

We identify firm headquarters locations based on their zip code from Compustat and link this to counties (and hence CZs) using the 1999 Census zip-to-county crosswalk, which we further supplement with HUD's 2017 zip-to-county crosswalk (to reflect the creation of new zip codes).[17] A firm's headquarters location as reported in Compustat suffers from a major flaw in that Compustat backfills historical information on a firm's headquarters location, and instead only

[15] We choose 1990 as the starting year of our sample because it approximately corresponds with Chetty et al.'s estimation window. However, our results are not sensitive to the starting year; we find qualitatively similar results if we start our sample in 1980 or 2000.
[16] Data are obtained from the Center for Research in Security Prices, CRSP/Compustat Merged Database, Wharton Research Data Services, http://www.whartonwrds.com/datasets/crsp/.
[17] This follows the approach described in the online appendix of Chetty et al. (2014a).

reports its current headquarters location. This would therefore represent a potentially significant measurement error problem in our context since firms sometimes change their headquarters location. To fix this potential problem, we restrict our analysis to the subset of firms that don't change their headquarters' location (which represents just over 90% of firms).[18]

We winsorize all firm-level variables at the 1st and 99th percentiles. To ensure a constant sample across specifications, we further impose the condition that all firm-level outcome and control variables be non-missing. With these filters, our final regression sample consists of 59,934 firm-years for 6,323 firms. Table 2 presents descriptive statistics of these firm-year level variables. The average firm's ROA is 0.077, with a standard deviation of 0.21; all told, our firm-level sample is similar to numerous other studies using Compustat data.

2.3. CZ-level economic data

We additionally include information on CZ-level economic variables to control for local economic conditions. From the BEA's Regional Economic Accounts we include information on income growth (i.e., total wages and proprietors' income) and population growth. From the BLS, we include information on the local unemployment rate. In both cases, we compute CZ-level measures from the underlying county-level data. All variables are winsorized at the 1st and 99th percentiles. Summary statistics of these CZ-year level variables are presented in Panel B of Table 1.

2.4. Miscellaneous measurement issues

Several measurement issues arise in the context of our analysis. First, as explained above, we link firms to CZs based on their headquarters' location. Of course, the relatively large publically traded firms in our sample are likely to have operations in other parts of the country beyond the

[18] We thank Alexander Ljungqvist for providing us with the data on firm headquarters relocations; for further details, see Heider and Ljungqvist (2015).

CZ in which they are headquartered. This raises the question of whether the headquarters' location is adequate. In fact, we believe that for our purposes the measure is ideal. The reason for this is that the corporate headquarters can be thought of as the nerve-center of a firm, where all key strategic and operational decisions are made. It is therefore precisely at the headquarters' location where variation in the stock of available human capital is likely to have the most direct impact on the overall quality of corporate decision-making, at all levels of the organization; the totality of these decisions should in turn affect corporate profitability.

A second measurement issue is that mobility is based on the CZ where people grow up, and not the CZ to which they ultimately move in adulthood. This represents a form measurement error in our analysis and adds noise to our regressions, suggesting that our estimates of the effect of intergenerational mobility on corporate profits may in fact be a lower bound. Nonetheless, this type of measurement error is not likely to be large for the simple reason that most people do not move. For example, Chetty and Hendren's (2018a) sample consists of 19.5 million families who never move CZs (i.e., permanent residents), but only 4.4 million movers.[19] Indeed, within-US migration has been on the decline for several decades, with recent estimates indicating that over a five year period only 12.9% of the population move across CZs (Molloy, Smith, and Wozniak, 2011). Notably, the typical American adult lives only 18 miles from their mother.[20]

A final measurement issue relates to the fact that mobility is measured based on people's incomes at age 26 or 30. If intergenerational mobility varies considerably over time, then the mobility of older workers (and thus to stock of human capital in a given CZ) may not be adequately captured by the mobility of those aged 26 or 30. As it turns out, however, this is not a concern,

[19] See Table 1 of Chetty and Hendren (2018a).

[20] Bui and Miller, *The New York Times*, Dec. 23, 2015.

since Chetty et al. (2014b) and Chetty et al. (2016) present evidence that percentile-rank based measures of mobility have remained stable for decades, potentially going as far back as the 1940's.

3. Methodology

To evaluate the connection between intergenerational mobility and corporate profitability, we estimate regressions of the following form.

$$(2) \qquad Profitability_{ijct} = \alpha_{jt} + \alpha_{st} + \beta \cdot Mobility_c + \gamma' X_{ijct} + \delta' Z_{ct} + \varepsilon_{ijct}$$

The unit of observation is the firm-year. The subscripts i, j, c, s, and t index firms, industries, CZs, states, and years, respectively. In the baseline analysis, the dependent variable, $Profitability_{ijct}$, is measured as the ratio of EBITDA to assets. The terms α_{jt} and α_{st} are SIC2 industry-year fixed effects and state-year fixed effects, respectively. $Mobility_c$ represents the measures of intergenerational mobility from and Chetty and Hendren (2018a and 2018b), described in section 2.1, and discussed further below. The estimate of β, the coefficient of interest, reflects the change in firm profitability associated with a one standard deviation increase in the measure of intergenerational mobility being considered. X_{ijct} is a vector of time-varying firm controls, including size (the natural log of total assets), firm age (the number of years in Compustat), market-to-book ratio (the market value of assets to the book value of total assets), asset tangibility (the ratio of net property, plant, and equipment to total assets), and leverage (the ratio of long-term debt to total assets). Z_{ct} is a vector of time-varying CZ controls capturing local economic conditions, including the unemployment rate, population growth, and income growth (total wages and proprietors' income). ε_{ijct} is the idiosyncratic error term. We cluster standard errors by CZ.

The fixed effects play a central role in controlling for potentially confounding and otherwise unobserved variation in the data. The industry-year fixed effects ensure that our results cannot be explained by either geographic differences in industry composition across CZs or by time-varying industry-level shocks, both of which are likely to affect corporate profitability. The state-by-year fixed effects ensure that our tests always compare firms located within the same state at the same point in time, guaranteeing that neither time-invariant state-level characteristics nor time-varying state-level economic shocks or policy changes can account for our findings. In essence, after adjusting for industry-level variation, our tests identify the effects of mobility on corporate profits by comparing firms located in different CZs within the same state. Including state-year fixed effects also rules out that our results are simply due to regional differences on mobility (see Figures 1 to 3), which in turn may be related to other unobserved factors affecting corporate profitability.

4. Findings

4.1 Baseline findings and robustness checks

Table 3 presents our baseline results and key robustness checks. As a starting point, in column 1, we omit the firm- and CZ-level control variables and consider the association between corporate profitability and our main intergenerational mobility measure based on the relative outcomes of permanent CZ residents. As mentioned above, higher values of this mobility measure represent better relative mobility—i.e., there is a smaller gap in outcomes for those with parental income at the 25th percentile compared to those with parental income at the 75th percentile. The statistically significant positive relationship shown in column 1 therefore means that firms are more profitable in areas where there this is better intergenerational mobility, and thus greater equality of opportunity. Moreover, the estimated economic magnitude is substantial. A one standard deviation improvement in mobility is associated with a 2.3 percentage point increase in profitability, which represents roughly an 11% increase in this variable relative to its standard deviation.

Next, in column 2, we add the firm-level control variables, described above, to determine the potential influence of firm characteristics in explaining our results. In fact, doing so has minimal effect on our estimates. The magnitude of the coefficient estimate increases marginally from 2.3 percent to 2.6 percent, and the statistical significance increases to the 1% level. The result indicates the variation in firm characteristics cannot account for our finding.

An additional concern is that areas with higher mobility may also be preforming better economically (even relative to other areas in the same state), and that this may in turn translate into higher profitability for locally headquartered firms. Although this hypothesis is contrary to the finding in Chetty et al. (2014a) that there is no systematic correlation between mobility and local labor market conditions, we nonetheless test for it. Specifically, in column 3, we control for local economic conditions by including CZ-level measures of the unemployment rate, population growth, and income growth. As shown, this has virtually no effect on our results – the coefficient estimate is barely changed at 2.5 percent, thus ruling out variation in CZ-level economic conditions as a potential confound.

Since the estimates in column 3 of Table 3 include a comprehensive set of control variables, we take this as our baseline result. In Figure 4 we present a binned scatterplot corresponding to this specification. The plot indicates that the association between mobility and profitability is indeed approximately linear and is not driven by outliers in the data.

In column 4, we decompose our relative mobility measure to separately examine the effects on corporate profitability of absolute mobility – i.e., the expected outcomes of people, growing up in the same CZ, with parental incomes at the 25th and 75th percentiles of the income distribution. The results for this specification are rather striking. As absolute mobility at the 25th percentile increases, so does corporate profitability. A one standard deviation increase in this measure is associated with a 2.6 percentage point increase in profitability. Thus, corporations are more profitable in areas where the people from poorer families are more upwardly mobile. In contrast, a one standard deviation increase in absolute mobility at the 75th percentile is associated with a 5.3 percentage point *decline* in profitability. To understand this result, recall that for the average CZ a person whose parents are at the 75th percentile of the income distribution is expected themselves to fall to the 57th percentile (see Table 1). CZs with higher mobility at the 75th percentile may therefore be interpreted as ones with less *downward mobility* among those from richer families, where higher parental incomes effectively insulate people from falling too far in the income distribution. Our results suggest, therefore, that less downward mobility among those from richer families may come at the expense of broader, society-wide, human capital formation, which in turn manifests itself in lower corporate profitability.

Taken together, our findings so far demonstrate that firms stand to benefit considerably in settings where there is greater equality of opportunity, and peoples' economic success is presumably more a function of their own abilities as opposed to their parents' income. In other words, we find strong empirical support for the idea that firms are more profitable in locations that appear to be more meritocratic.

In the final two columns of Table 3, we perform robustness tests. In column 5 we consider a measure of relative mobility based on people's percentile rank in the income distribution at age 30, instead of at age 26. This has little effect on our results. In column 6 we use an alternative measure of corporate profitability—the ratio of net income to assets. This, too, has minimal effect. These results therefore indicate that our findings are neither sensitive to the age at which mobility is measured, nor are they an artefact of the particular measure of corporate profitably that we use;

rather, the positive link between intergenerational mobility and corporate profits that we uncover is robust to alternative measures.

4.2 Causal estimates of mobility based on movers

The work of Chetty and Hendren (2018a) exhaustively demonstrates that variation in mobility across CZs largely reflects the causal effects of place on resident mobility. In other words, their analysis comprehensively rejects the alternative hypothesis that observed variation in the mobility of permanent CZ residents is due to systematic differences in the types of people living in each area (i.e., selection). For this reason, we use mobility measures based on the outcomes of permanent residents as our baseline measures, since these are the most straightforward and are estimated by Chetty and Hendren (2018a) using the broadest possible sample of observational data, consisting of tens of millions of records. In the next set of robustness checks, however, we confirm that our findings hold if we use alternative causal estimates of mobility, provided by Chetty and Hendren (2018b), based on the outcomes of cross-CZ movers.

Specifically, Chetty and Hendren (2018b) estimate intergenerational mobility for the subset of families who move to different CZs, obtaining identification of the causal effect of each CZ on mobility by measuring differences in outcomes based on the age at which children move. In Table 4, we relate corporate profitability to Chetty and Hendren's (2018b) raw causal estimates of mobility, as estimated based on the subset of cross-CZ movers.[21]

Column 1 shows that using estimates of relative mobility based on the subset of movers yields broadly similar effects on corporate profitability as our earlier measure (based on the mobility of

[21] Chetty and Hendren (2018b) also provide forecasts of each CZ's effect on mobility, which are computed as weighted averages of the mobility of permanent residents and the raw causal estimates (with weights chosen to minimize the forecasts' mean squared error). Since we have already shown the effects using the mobility of permanent residents, we now simply present the effects using Chetty and Hendren's (2018b) raw causal estimates.

permanent CZ residents). As before, higher values of this mobility measure represent better relative mobility. Column 1 shows that a one standard deviation improvement in relative mobility is associated with a 1.6 percentage point increase in profitability; the estimated effect is statistically significant and only slightly smaller in magnitude than our estimate based on the mobility of permanent CZ residents.

In column 2, we follow our earlier approach and decompose the relative mobility measure to separately examine the effects on corporate profitability of absolute mobility at the 25th and 75th percentiles (again, now based on movers instead of permanent residents). As in column 4 of Table 3, we find that absolute mobility at the 25th percentile is positively associated with profitability, although the point estimate is notably smaller and not statistically significant in this setting. Consistent with our earlier results, we also find that mobility at the 75th percentile is negatively associated with profitability. The result is statistically strong and indicates that a one standard deviation increase in absolute mobility of movers with parental income at the 75th percentile is associated with a 1.7 percentage point decline in profitability. In other words, just as explained earlier, it appears that less downward mobility among those from richer families translates into lower corporate profitability.

All told, we interpret the results in Tables 3 and 4 as broadly similar—whether mobility is measured based on permanent residents or movers, firms are more profitable in areas with greater intergenerational mobility, and thus greater equality of opportunity.

4.3 Human capital formation

In our next set of tests, we provide further evidence consistent with the role of human capital formation in explaining the link between intergenerational mobility and corporate profits.

Specifically, Chetty and Hendren (2018b), in addition to estimating measures of international mobility, also provide causal estimates of each CZ on college attendance rates. These estimates are based on the subset of movers, and can be interpreted as causal in the sense that Chetty and Hendren (2018b) obtain identification of the effect of each CZ on college attendance in exactly the same way as they did for intergenerational mobility – i.e., by exploiting differences in the age at which children move. As such, in Table 5, we substitute the prior measures of intergenerational mobility used in Table 4 with the analogous measures of college attendance rates.

The rationale behind these tests is fairly straight forward: college attendance directly reflects the type of skill acquisition and human capital formation from which locally headquartered firms are likely to benefit. Of course, college attendance rates will probably only provide a partial measure of the available stock of human capital—i.e., even conditional on college attendance, there is likely to be variation in human capital; conversely, conditional on non-attendance of college, there too is likely to be variation in human capital. In this sense, our analysis using college attendance rates as an explanatory variable may be interpreted as illustrative of a broader linkage between human capital formation and corporate profitability.

Column 1 of Table 5 examines the relationship between corporate profitability and relative college attendance rates—i.e., the estimated difference in college attendance rates of people with parental income at the 25th percentile of the income distribution versus those with parental income at the 75th percentile. CZs with higher values of this measure therefore exhibit more equality of opportunity in college attendance. Our results indicate that a one standard deviation improvement in relative college attendance (i.e., more equality of opportunity) is associated with a 1.2 percentage point increase in corporate profitability. This estimate is statistically significant at the 5% level and mirrors our earlier results using intergenerational mobility.

In column 2, we follow our earlier approach and decompose the relative measure of college attendance to separately examine the effects on corporate profitability of "absolute" college attendance rates for people with parental incomes at both the 25th and 75th percentiles. Here we find the striking result that as college attendance increases for those at the 25th percentile, so too does corporate profitability. A one standard deviation increase in college attendance rates for people with parental income in the 25th percentile is associated with a 1.3 percentage point increase in profitability, a result that is statistically significant at the 5% level. In other words, corporations are more profitable in areas where people from poorer families have better opportunities to go to college. In contrast, although college attendance rates for those at the 75th percentile are slightly negatively related to profitability, the coefficient estimate is not statistically significant.

Overall, the results are consistent with the notion that human capital formation provides a key channel underpinning the positive link between intergenerational mobility and corporate profitability. Our findings show that when individuals from less privileged backgrounds have greater opportunities to attend college and thus develop their human capital, corporations are among the beneficiaries.

5. Conclusions

To what extent should society pursue policies that enhance equality of opportunity? Often, this issue is discussed from the perspective of equity or fairness. In this paper, we examine the role of equality of opportunity from a different perspective. Specifically, we study the connection between equality of opportunity, as measured by intergenerational mobility, and corporate profitability. Areas where intergenerational mobility is greater may be interpreted as simply doing a better job of unlocking their residents' innate talents, such that the economic circumstances into which people are born are less determinative of their economic success later in life. The overall stock of human capital should therefore be higher in such areas—and since human capital matters for corporate performance, locally headquartered firms potentially stand to benefit. Consistent with this view, we find strong empirical support for exactly such a positive link between intergenerational mobility and corporate profitability. A key lesson from our analysis, therefore, is that greater equality of opportunity need not come at the expense of corporate profits; rather, firms are more profitable in areas where there is more equality of opportunity. Our findings ultimately suggest that policies aimed at bolstering equality of opportunity may in turn have positive effects on key economic outcomes in addition to corporate profits, such as productivity and growth.

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Figure 1: Mobility map for permanent CZ residents

This figure presents a map of the relative intergenerational mobility of permanent CZ residents, as estimated by Chetty and Hendren (2018a). For each CZ, mobility is measured as the difference in the expected percentile rank in the national income distribution at age 26 for individuals whose parents' incomes are the 25th percentile of the income distribution, versus those whose parents' incomes are at the 75th percentile. Higher (less negative) values indicate a smaller gap between those at the 25th and 75th percentiles, meaning greater relative intergenerational mobility. CZs in which no sample firms are based are shaded gray.

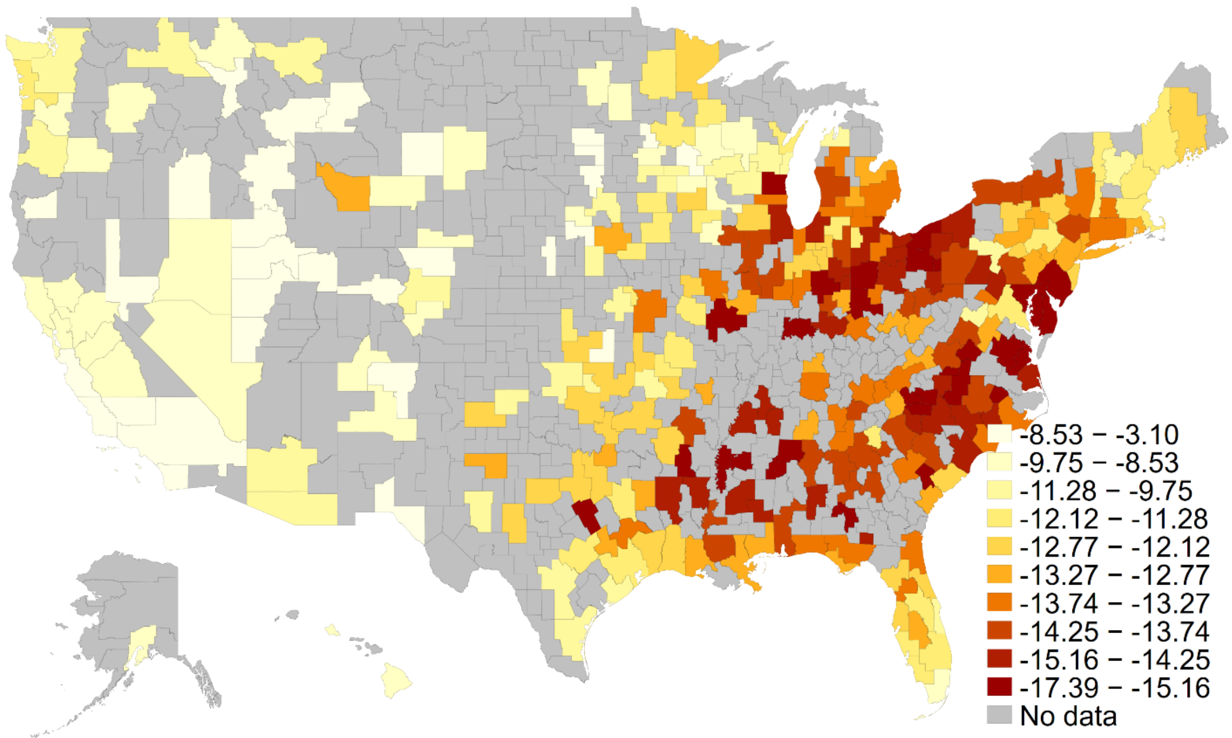

Figure 2: Mobility map for cross-CZ movers

This figure presents a map of the relative intergenerational mobility of cross-CZ movers, based on Chetty and Hendren's (2018b) raw causal estimates. For each CZ, mobility is measured as the difference in the expected percentile rank in the national income distribution at age 26 for individuals whose parents' incomes are the 25th percentile of the income distribution, versus those whose parents' incomes are at the 75th percentile. Higher values indicate greater relative intergenerational mobility. CZs in which no sample firms are based are shaded gray.

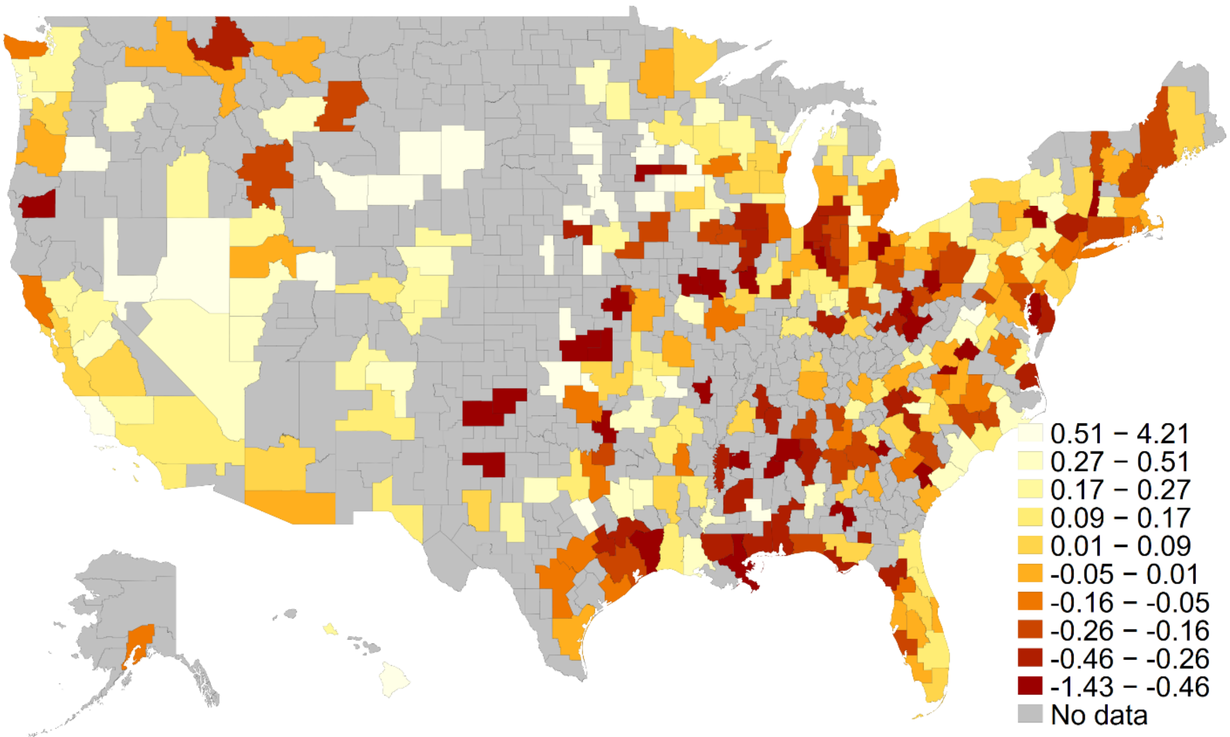

Figure 3: Mobility map based on college attendance rates

This figure presents a map of relative college attendance rates, as estimated by Chetty and Hendren (2018b) based on the outcomes of cross-CZ movers. For each CZ, this measure is the difference in college attendance rates between the ages of 18 and 23 for individuals whose parents' incomes are the 25th percentile of the income distribution, versus those whose parents' incomes are at the 75th percentile. Higher values indicate greater relative mobility, as measured by of college attendance rates. CZs in which no sample firms are based are shaded gray.

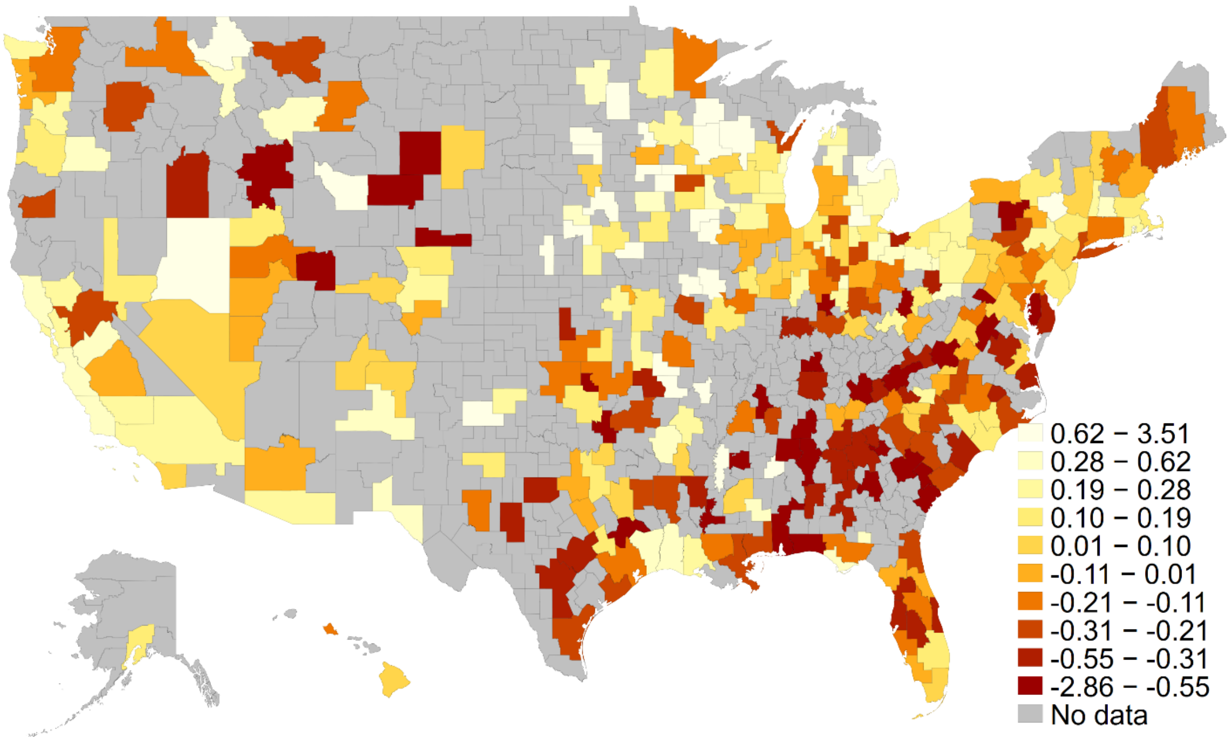

Figure 4: Mobility and corporate profitability

This figure provides a binned scatterplot of the association between intergenerational income mobility and corporate profitability. It corresponds to our baseline specification described in section 4.1, the estimates of which are displayed in column 3 of Table 3.

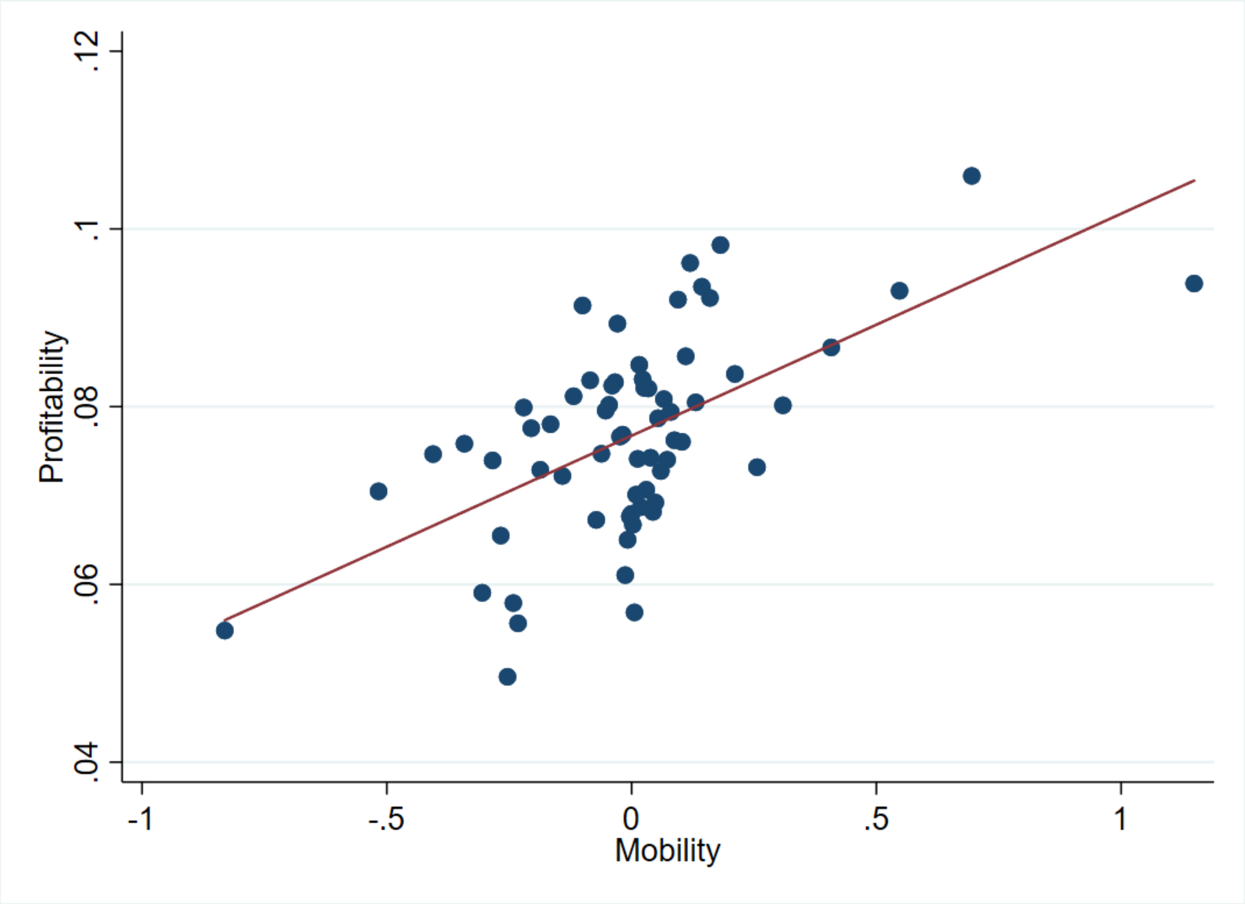

Table 1. Descriptive statistics: Intergenerational mobility and CZ economic conditions

Panel A of this table reports descriptive statistics of the CZ-level measures of intergenerational mobility used in the analysis, as estimated by Chetty and Hendren (2018a and 2018b) and defined in Section 2.1 of the text. Panel B of this table provides summary statistics of yearly CZ-level economic variables over the period 1990 to 2014, which are used as controls in our baseline regressions; for further details, see Section 2.3 of the text.

Panel A: Commuting zone mobility measures

				Percentile		
	Obs.	Mean	S.d.	25th	50th	75th
Permanent residents' income mobility:						
Age 26, p=25	309	44.4	3.7	41.7	43.9	46.7
Age 26, p=75	309	56.6	2.9	54.7	56.1	58.7
Age 26, p=25 minus p=75	309	-12.2	2.6	-14.0	-12.8	-10.3
Age 30, p=25	309	43.5	3.5	40.8	42.9	45.6
Age 30, p=75	309	56.7	2.9	54.6	56.2	58.9
Age 30, p=25 minus p=75	309	-13.2	2.4	-14.9	-13.5	-11.7
Movers' income mobility, causal estimates:						
Age 26, p=25	309	0.07	0.45	-0.16	0.01	0.20
Age 26, p=75	309	0.01	0.41	-0.20	0.01	0.19
Age 26, p=25 minus p=75	309	0.06	0.54	-0.19	0.01	0.21
College attendance, causal estimates:						
Age 18-23, p=25	309	0.10	0.63	-0.30	0.08	0.36
Age 18-23, p=75	309	0.08	0.40	-0.13	0.05	0.26
Age 18-23, p=25 minus p=75	309	0.02	0.58	-0.27	0.01	0.23

Panel B: Commuting zone yearly economic variables

				Percentile		
	Obs.	Mean	S.d.	25th	50th	75th
Unemployment rate	5,346	5.8	2.1	4.3	5.4	7.1
Population growth	5,346	1.0	1.0	0.3	0.9	1.5
Income growth	5,346	4.5	3.6	2.7	4.6	6.5

Table 2. Descriptive statistics: Firm-year variables

The sample consists of 59,934 firm-years for all nonfinancial and non-utility U.S. companies that are traded on the NYSE, Amex, or Nasdaq over the period 1990 to 2014, from the merged CRSP-Compustat Fundamentals Annual database. This table reports descriptive statistics for the dependent and control variables. For further details, see Section 2.2 of the text.

				Percentile		
	Obs.	Mean	S.d.	25th	50th	75th
Dependent variables:						
Editda / Assets	59,934	0.077	0.214	0.052	0.119	0.178
Net income / Assets	59,934	-0.021	0.236	-0.023	0.038	0.082
Control variables:						
log(Assets)	59,934	5.578	1.899	4.171	5.440	6.833
Age	59,934	16.37	14.00	5.00	12.00	24.00
Market-to-book	59,934	1.837	1.683	0.854	1.281	2.131
Asset tangibility	59,934	0.266	0.228	0.086	0.196	0.382
Leverage	59,934	0.163	0.191	0.001	0.099	0.268

Table 3. Corporate profits and intergenerational mobility: Baseline results

This table presents our main results on the association between CZ intergenerational mobility and the profitability of locally headquartered firms. The unit of analysis is the firm-year. Mobility is measured based on the outcomes of permanent CZ residents, as estimated Chetty and Hendren (2018a). In columns 1 to 5, the dependent variable is the ratio of EBITDA to assets (ROA). In column 6, the dependent variable is the ratio of net income to assets. Columns 1, 2, 3, and 6 investigate the effects of relative mobility—i.e., the difference in expected outcomes, as measured at age 26, of permanent CZ residents with parental income at the 25th percentile of the income distribution, versus those with parental incomes at the 75th percentile. Column 4 decomposes the relative mobility measure to separately examine the effects of absolute mobility at both the 25th and 75th percentiles. Column 5 examines the effect of relative mobility, analogous of column 3, except that mobility is measured at age 30. All regressions include SIC2 industry-year fixed effects and state-year fixed effects. Standard errors are clustered by CZ and are shown in parentheses underneath the coefficient estimates. *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) ROA	(2) ROA	(3) ROA	(4) ROA	(5) ROA	(6) NI/Assets
Perm. res., age 26, p25 – p75	0.023**	0.026***	0.025***			0.022***
	(0.011)	(0.010)	(0.008)			(0.007)
Perm. res., age 26, Abs., p25				0.026***		
				(0.009)		
Perm. res., age 26, Abs., p75				-0.053***		
				(0.014)		
Perm. res., age 30, p25 – p75					0.021**	
					(0.008)	
Size		0.037***	0.037***	0.037***	0.037***	0.039***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age		0.001***	0.001***	0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Market-to-book		-0.002	-0.002	-0.002	-0.002	-0.005
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Asset tangibility		0.137***	0.136***	0.136***	0.136***	0.044***
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Leverage		-0.074***	-0.073***	-0.074***	-0.074***	-0.157***
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Unemployment rate			0.644	0.377	0.560	0.580
			(0.466)	(0.296)	(0.439)	(0.401)
Population growth			-0.677	-1.033***	-0.694	-0.716
			(0.472)	(0.372)	(0.459)	(0.444)
Income growth			0.102**	0.158***	0.099**	0.137**
			(0.043)	(0.052)	(0.044)	(0.067)
SIC2*year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State*year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,934	59,934	59,934	59,934	59,934	59,934
Adjusted R^2	0.189	0.291	0.292	0.293	0.292	0.219

Table 4. Corporate profits and causal intergenerational mobility estimates: Movers

This table presents results on the association between corporate profitability and Chetty and Hendren's (2018b) causal estimates of intergenerational mobility, based on the outcomes of cross-CZ movers. The unit of analysis is the firm-year. The dependent variable is the ratio of EBITDA to assets (ROA). Column 1 investigates the effect of relative mobility—i.e., the difference in expected outcomes, as measured at age 26, of cross-CZ movers with parental income at the 25th percentile of the income distribution, versus those with parental income at the 75th percentile. Column 2 decomposes the relative mobility measure to separately examine the effects of absolute mobility at both the 25th and 75th percentiles. All regressions include SIC2 industry-year fixed effects and state-year fixed effects. Standard errors are clustered by CZ and are shown in parentheses underneath the coefficient estimates. *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) ROA	(2) ROA
Movers, age 26, p25 – p75	0.016**	
	(0.007)	
Movers, age 26, Abs., p25		0.005
		(0.009)
Movers, age 26, Abs., p75		-0.017**
		(0.009)
Size	0.037***	0.037***
	(0.003)	(0.003)
Age	0.001***	0.001***
	(0.000)	(0.000)
Market-to-book	-0.002	-0.002
	(0.003)	(0.003)
Asset tangibility	0.136***	0.136***
	(0.011)	(0.011)
Leverage	-0.075***	-0.075***
	(0.013)	(0.013)
Unemployment rate	0.836	0.744*
	(0.537)	(0.436)
Population growth	-0.517	-0.743*
	(0.524)	(0.405)
Income growth	0.125***	0.135***
	(0.044)	(0.049)
SIC2*year fixed effects	Yes	Yes
State*year fixed effects	Yes	Yes
Observations	59,934	59,934
Adjusted R^2	0.291	0.291

Table 5. Corporate profits and human capital formation

This table presents results on the association between corporate profitability and Chetty and Hendren's (2018b) causal estimates of college attendance rates, based on the outcomes of cross-CZ movers. The unit of analysis is the firm-year. The dependent variable is the ratio of EBITDA to assets (ROA). Column 1 investigates the effect of relative college attendance rates—i.e., the difference in expected college attendance rates, between the ages of 18 and 23, for those with parental income at the 25th percentile of the income distribution, versus those with parental incomes at the 75th percentile. Column 2 decomposes the relative measure to separately examine the effects of absolute college attendance rates at both the 25th and 75th percentiles. All regressions include SIC2 industry-year fixed effects and state-year fixed effects. Standard errors are clustered by CZ and are shown in parentheses underneath the coefficient estimates. *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	ROA	ROA
College Attendance, age 18-23, p25 – p75	0.012**	
	(0.006)	
College Attendance, age 18-23, Abs., p25		0.013**
		(0.007)
College Attendance, age 18-23, Abs., p75		-0.003
		(0.008)
Size	0.037***	0.037***
	(0.003)	(0.003)
Age	0.001***	0.001***
	(0.000)	(0.000)
Market-to-book	-0.002	-0.002
	(0.003)	(0.003)
Asset tangibility	0.137***	0.137***
	(0.011)	(0.010)
Leverage	-0.075***	-0.074***
	(0.013)	(0.013)
Unemployment rate	0.797	0.824
	(0.562)	(0.551)
Population growth	-0.321	-0.155
	(0.511)	(0.489)
Income growth	0.109**	0.101**
	(0.042)	(0.043)
SIC2*year fixed effects	Yes	Yes
State*year fixed effects	Yes	Yes
Observations	59,934	59,934
Adjusted R^2	0.291	0.291