Does Absolute or Relative Income Motivate Migration?

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Abstract

This paper examines the extent to which relative income – that is, one's position in the income distribution – matters in migration choice. Virtually all studies of migration focus on absolute income. This is at odds with the mounting evidence that suggests people care about their relative position in the income distribution. We argue that, in order to test between the absolute income and relative income theories of migration, one needs individual-level panel data on before and after migration outcomes. Indeed, since one has to estimate counterfactual migrant earnings of non-migrants, if migrants are selected on unobservables then cross-sectional estimates will systematically bias the predicted migrant earnings of non-migrants. We estimate the relative importance of the two main theories in explaining interstate migration in the U.S. using a panel of individuals. Relative income is calculated with respect to those persons in the same U.S. state. We find that, although migration leads to a substantial rise in absolute income, the trigger for migration is low relative income and not low absolute income.

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"People care greatly about their relative income, and they would be willing to accept a significant fall in living standards if they could move up compared with other people."
- Richard Layard (2005, p.43) "*Happiness: Lessons from a New Science*"

1 Introduction

Until recently, economists were in almost universal agreement that happiness (or well-being) increased monotonically with income.[1] Economic agents were therefore defined by making choices – including migration – to maximise expected income. In the context of migration choice this implies that the higher the income gain from migrating, the more likely migration occurs. It is no surprise then that almost all models of migration assume the incentive to migrate comes from the expected income differential between the source and destination.[2,3] Chief among these is Borjas' (1987, 1991) model of income differentials, which remains the most popular theory of migration twenty-five years after it was published.[4]

However, there is a possible turning of the tide. Recent survey evidence shows that happiness and life satisfaction are determined by relative income (to others in some comparison group) as well as absolute income, and that once a threshold level of income – needed for the essentials in life – is exceeded, happiness no longer increases with absolute income but relative income instead.[5] One way to change relative income is through mi-

[1]The mechanism is that higher income implies higher consumption – for given prices – and, in turn, greater happiness.

[2]Throughout this paper we use 'source' to denote the pre-migration location (or area) of residence and 'destination' refers to the post-migration location of residence. Therefore, migration is the flow of people from the source to the destination.

[3]See, for example, the seminal works of Sjaastad (1962), Todaro (1969), Harris and Todaro (1970) and Borjas (1987, 1991).

[4]We refer to "absolute income" interchangeably with "Borjas" to describe the mechanism proposed by Borjas (1987, 1991).

[5]See, among others, Blanchflower and Oswald (2000), Frey and Stutzer (2002), Layard (2005) and Luttmer (2005). The idea that individuals care about relative income is not new: over sixty years ago

gration.[6] If relative income is important, then we may observe situations that run counter to the absolute income hypothesis. For example, if migration results in a deterioration in relative income, people may choose not to migrate even when there is a potentially large absolute income gain from doing so. Conversely – as the opening quote from Layard (2005) suggests – people may migrate to improve their income position even when the absolute income gain from migration is zero – or possibly, even negative (a case often seen for return migrants).

The migration literature has been slow to catch-on. One visionary, Oded Stark, did theorise that migration may depend on so-called *relative deprivation* before Borjas' (1987) paper was published.[7] Stark (1991) assumes people care about their relative position in the income distribution (of some comparison group) and that high relative deprivation increases the propensity of outmigration. More specifically, Stark (1991) measures relative deprivation for an individual as the fraction of people with higher incomes than that individual multiplied by their average excess income.[8,9]

There is little or no empirical evidence to suggest which of these independently-researched theories is the more important and, theoretically authors have either not attempted to or not succeeded in distinguishing them. The purpose of this paper is therefore to analyse migration when both relative and absolute income motives exist and,

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Duesenberry (1949) argued that saving depends not on absolute income but on relative income.

[6]For example, migration will improve relative income when migration increases absolute income and the incomes of the comparison group are unchanged or, if migration involves no change in absolute income but a change in the comparison group to one on lower incomes.

[7]Stark (1984, 1991, 2006) and Stark and Yitzhaki (1988). See also Mehlum (2002) for how migration is self-perpetuating (within and across generations) when relative deprivation is important.

[8]We refer to "relative deprivation" interchangeably with "Stark" to refer to Stark's mechanism.

[9]The term *relative deprivation* originates from the social psychology literature. It refers to the feeling of being deprived when comparing oneself to the better-off in one's 'reference group'. We feel this is a little unfortunate since deprivation typically conveys hardship and a lack of the necessities in life. In contrast, by relative deprivation we mean the inverse of some measure of relative income or, more precisely, one's position in the income distribution. Despite its slightly misleading language, we stick with the term *relative deprivation* because it was used by Stark (1991) and, among others, it has been used in the study of income inequality and mortality (see, for example, Deaton (2001)).

ultimately, to use panel data on interstate migration in the United States to ascertain their relative importance in migration decisions.

Why is it important? If – and it's a big if – relative income is found to be important and even dominate absolute income considerations in migration choice, it will have profound implications. In summary, almost all existing economic models of migration will be wrong, population forecasts will need to be recalculated and migration policy rethought. Indeed, the two theories can diverge in their predictions. Firstly, under Stark's relative deprivation theory, migrants from the source region tend to be low-skilled (the deprived); whereas if the gain in income from migration is greater for the high-skilled, then Borjas' absolute income theory tends to predict that migrants from the source are high-skill (a brain drain). In section 3, we show formally that this divergence is due to the asymmetric nature of Stark's relative deprivation measure – it assumes people compare themselves only with those on higher incomes (and not lower incomes) than themselves. Second, a common concern of high-income regions is that opening their borders to migrants from low-income regions will lead to a flood of immigrants. To take one example, when Romania and Bulgaria joined the European Union in 2007 many of the existing members introduced transitional restrictions on immigration from these two countries.[10] If, however, the relative deprivation theory is correct, then the fears of high-income states may be overstated; indeed, if migrants switch their reference group (with which income comparisons are made) to the high-income destination, then they will likely find that their relative deprivation worsens, which might make them more likely to return-migrate.[11] Finally, and we think most importantly, policy makers are not only concerned with aggregate outcomes but individual outcomes too. In fact, every vote counts. If the relative

[10]Seventeen (out of twenty-five) EU member states restricted the free movement of labour from Romania and Bulgaria when they joined the EU in 2007. Previously, transitional restrictions – which can last for up to seven years from the date of accession – were imposed on migrants from the 2004 EU accession countries, with the notable exceptions of Cyprus and Malta.

[11]Here the issue is whether there is a relative income motive for migration *and*, conditional on a relative income motive, whether one changes his or her reference group upon migration.

deprivation story is correct, then a transfer of income from the rich to the poor is one way to improve the lives of the poor and stem their outmigration; if however the absolute income story is correct then the only way to improve the situation of the poor is to increase the income of everyone.

While at first glance relative deprivation and absolute income motives may appear easy to disentangle – after all they seem to speak to different moments of the income distribution – the point is rather more subtle. Indeed, in aggregate data, both Borjas and Stark can predict the 'same' relationship between income inequality and the skill of migrants. More specifically, when income inequality is higher in the source than the destination, both theories predict that migrants will be of low-skill – for any given average income differential. The theories, of course, differ in their underlying mechanism. In Borjas (1987, 1991) income inequality reflects the return-to-skill, where a more unequal income distribution implies a higher return-to-skill. Therefore when income inequality is higher in the source than the destination, the return-to-skill is higher in the source and, it is the low-skilled that are more likely to migrate from the source (for any given average income differential).[12,13] This confounds Stark's relative deprivation theory, which also predicts that the low-skilled (and, hence, low relative income) are more likely to migrate from the source.

In section 3 we argue that the two theories can only be distinguished using individual-level panel data on before and after migration outcomes. The reason is that, with cross-sectional data, by definition one only observes income at a single point in time, and for migrants this is typically after migration.[14] In such a case, one has to estimate the pre-

[12] The average income differential and the cost of migration affect the volume of migration but not the selection-on-skill of migrants.

[13] These results require a certain degree of transferability of skills between the source and destination (see Borjas (1987)).

[14] A survey can only document migration if it has already occurred, whereas income is typically recorded at the time of the survey.

migration income of migrants. However, if migrants are selected on unobservables, then the estimated earnings of migrants will be systematically biased. In contrast, with panel data we can directly compute relative income prior to migration. Naturally, we will still want to estimate the counterfactual earnings of non-migrants, but with panel data to hand we can control for unobserved skill heterogeneity. In other words, we need panel data to identify the high- and low-skilled.

The empirical literature on the determinants of migration has almost entirely neglected to test for a relative income motive. Furthermore, the few papers that do are systematically biased because they fail to control for the selection of migrants on unobservables. To fill the void and test between the two theories, we use panel data on individual interstate migration in the United States from the Panel Study of Income Dynamics (PSID). We use the Current Population Survey (CPS) to estimate the distribution of income by state, from which we compute relative deprivation for each individual-year in our PSID sample.

Since relative income is subjective, how researchers should measure it from knowledge of the income distribution is open to debate. What is clear is that a workable definition needs to be specific about two things. First, who constitutes the group with which interpersonal comparisons are made? The evidence from social psychology suggests that this reference group must be known to the individual (that is, they know the income of the group members) and the person feels that the income of the group members is a realistic expectation for himself. This suggests geographical proximity and connectedness are important elements to be considered. Naturally then, in the study of migration we will assume attachment is to the people that reside within a geographical identifier, in our case a U.S. state.[15] That is, we assume the reference group for a person is the population

[15] The geographical identifier for the reference group may well be much narrower than the state level. Luttmer (2005) finds that an increase in the earnings of those in the same U.S. Public Use Microdata Area (PUMA) – which in 1990 had an average population of roughly 150,000 – reduces happiness.

of the U.S. state that he or she resides in (or used to reside in).[16]

We will see that a pertinent question is whether, post-migration, a person changes his or her reference group from the source to the destination; if so, then we say reference substitution occurs. In Stark's early papers (Stark, 1984, 1991, Stark and Yitzhaki, 1988), the reference group was assumed to be the source irrespective of migration. From this viewpoint relative deprivation is a push factor since it is only the source income distribution that matters. Stark and Wang (2000, 2005) were the first to acknowledge that individuals who care about relative income may in fact use migration in order to substitute their reference group for another in the destination. From this viewpoint relative deprivation is both a push and pull force for migration.[17] Reference group substitution opens the possibility that an individual may migrate to decrease relative deprivation (or increase relative income) even if it involves no change in absolute income – or possibly, even lowers income. We will consider the cases with and without reference group substitution.

The second thing that a definition of relative income must include is the functional form for how one's position in the income distribution equates to relative income. Stark has proposed an 'upward comparison' view where an individual compares his or her income with those people on higher incomes in the reference group. More specifically, relative deprivation for person i is measured as the product of the proportion of people with income higher than i and the mean excess income of these people (Stark, 1984, Stark and Yitzhaki, 1988). This is based on evidence from social psychology that people look up and not down (see, for example, Stouffer et al. (1949) and the references within

[16]Blanchflower and Oswald (2004) find that, for U.S. states, an increase in the average income in the person's state reduces that person's happiness; however, if that person's income rises in line with the state average then that person gains overall.

[17]Stark (2006) suggests that Borjas' (1987) theory can be empirically distinguished from his own relative deprivation theory because Borjas (1987) emphasises income inequality in the destination whereas Stark's theory pertains to income inequality in the source. However, this is not a good distinction, particularly if migration induces reference group substitution.

Frey and Stutzer (2002)). For completeness we also consider the symmetric case where individuals simply compare their income to the average in the reference group.[18]

The empirical analysis in this paper yields some novel results. First we show that, using the sample of migrants, migration is associated with an increase in absolute income and a reduction in relative deprivation. Therefore, the extreme case that migrants move to lower their relative deprivation even when it involves a cut in absolute income is not supported in the data. However, there is weak evidence that the observed percentage increase in relative income post-migration is larger than would result purely from the observed percentage increase in absolute income and no change in average income. By implication, there is tentative evidence that migrants tend to target states where their position in the income distribution improves holding absolute income constant.

Second, using the full sample of migrants and non-migrants, there is robust evidence to support the relative deprivation hypothesis. We find that an increase in relative deprivation increases the propensity to migrate from the source state. Surprisingly, we find little or no evidence to support the absolute income theory of migration that dominates the migration literature and the thinking of policy makers. To be clear, although migrants tend to realise a rise in income post-migration, our findings suggest that this is not the trigger for migration. Rather our estimations suggest that – conditional on income and the estimated income gain from migration – the trigger for migration is a rise in relative deprivation. These results hold after controlling for state-level compensating differentials such as the price level, unemployment rate, and climate – as well as personal character-

[18] Blanchflower and Oswald (2004) study the determinants of happiness in the U.S. and find evidence that individuals compare their income to the simple average income in the individual's state. More specifically, they also define relative income as the ratio of individual income to the state average and they estimate its coefficient in a happiness regression to be positive and significant, even after controlling for absolute income. The authors do, however, caution that relative income is not a complete explanation for the absence of increasing happiness in the U.S. over time. The authors also experiment with other measures of relative income, comparing individual income to quintile averages. They find tenuous evidence for the upward comparison view; more specifically, the ratio of individual income to the top quintile performs better than the ratio with any other quintile.

istics such as age, education, marital status, number of children, home ownership, and individual fixed effects.

Throughout the paper we use the convention that migration – if it occurs – is from the 'source' to the 'destination'. Therefore, by definition, the source is the pre-migration region and the destination is the post-migration region. Migrants, outmigrants and immigrants all refer to people that have moved from the source to the destination. Also, we use the term 'positive selection' to refer to the situation where migrants from the source have an average skill that exceeds the average skill in the source. 'Negative selection' is used to describe the situation where the average skill of migrants is below the average skill in the source.

The rest of the paper is organised as follows. Section 2 discusses the related literature. In section 3 we formally identify the problems with distinguishing Borjas' absolute income model from Stark's relative deprivation model of migration. Section 4 contains a description of the data, the empirical strategy and the estimation results. Finally, section 5 concludes.

2 Literature Review

Since a major contribution of this paper is to highlight the current literature's failure to estimate the effect of relative income on migration, it seems appropriate to dedicate a whole section to reviewing the related literature. Our work is related to four distinct literatures. In decreasing order of importance (for our work) they are: (1) a handful of papers that claim to jointly estimate the importance of relative and absolute income motives for migration; (2) papers that proclaim to test empirically the selection predictions of Borjas' model; (3) papers that show relative income affects utility and can help to explain a number of (not migration related) economic puzzles and, (4) papers that show migrants respond to absolute income differentials (without controlling for relative income considerations). We take each of these in turn.

Stark and Taylor (1989, 1991) find that, after controlling for the expected absolute income gain, relatively deprived Mexican households were more likely to migrate to the United States. However, in section 3 we show that if migrants are positively selected on unobservables, then their result is biased in favour of the finding that relative deprivation matters. The problem is that they use cross-sectional data, which precludes controlling for selection on unobservables when they estimate counterfactual income of migrants and non-migrants from the earnings of non-migrants and migrants, respectively. Quinn (2006) studies the effect of relative deprivation (and not just in terms of income but wealth too) on the migration of Mexican households. However, this suffers from the same problem as Stark and Taylor (1989, 1991) because it is cross-sectional.

In a working paper, Basarir (2012) uses panel data on internal migration in Indonesia to study absolute and relative motives for migration.[19] The empirical analysis uses the final two waves of data, 2000 and 2007, of the Indonesian Family Life Survey. The author estimates the effect of absolute and relative measures of expenditure, income and assets on the propensity to migrate. He finds that men are more likely to migrate if they expect to improve their expenditure *rank*, even if it involves a loss in absolute expenditure. The future ranking of income and assets is statistically insignificant. The author finds that initial absolute expenditure is negatively related to the propensity to migrate; whereas the effects of initial income and assets are insignificant. There is no evidence to suggest that a low initial rank increases migration propensity holding absolute measures constant. Basarir (2012) is similar to our paper in both its aims and its use of panel data; however, there are some key differences. The dependent variable in Basarir (2012) is a dummy

[19]We only became aware of Basarir (2012) after completion of this paper.

variable for whether the individual moved out of the source sub-district for a period of more than six months between 2000 and 2007, so there is no precise information on the timing of migration, nor can the author identify return migrants. In contrast, our PSID data follows individuals annually (or biennially) and so we can pin-point the timing of migration and ascertain whether the individual is returning to a state he or she previously resided. In particular, we can relate the migration decision to the socio-economic characteristics of the individual at the time of migration. Also, Basarir (2012) uses actual future values of expenditure, income and assets to proxy for the expected gain from migration. Such an approach introduces endogeneity concerns. Instead, using our long-running PSID panel dataset, we estimate the contemporaneous counterfactual (migrant) income of non-migrants, whilst controlling for unobserved heterogeneity. Still, Basarir (2012) can be considered complementary to our paper since it studies migration within a developing country, Indonesia, whereas our data is for the U.S..

A number of papers have sought to test Borjas' selection theory. Unfortunately, none of these are a test between Stark's and Borjas' theory. Moreover, where evidence has been found in favour of Borjas' theory (and, on the whole, the evidence is mixed) it equally can serve as evidence for the relative income story.[20] Early work on this looked at variation in the earnings of U.S. immigrants with the same observable skills and related this to income inequality in their source countries.[21] Borjas (1987) uses U.S. Census data from 1970 and 1980 and compares the earnings of U.S. immigrants from 41 countries with income inequality (measured as the ratio of the top 10 percent to the bottom 20 percent) in the source. He finds weak evidence in support of his selection theory: income inequality

[20]The vast majority of papers that seek to test Borjas' selection predictions study Mexico-to-U.S. migration. Since income inequality is higher in Mexico than the U.S., Borjas' model predicts negative selection of migrants. Of course, low-skilled migrants also tend to be relatively deprived.

[21]See Borjas (1994, pp. 1690) for a summary. Later we will argue that, to test between the relative and absolute income stories, it is necessary to use individual-level data. Since many of these studies use country-level migration data, this confounds absolute and relative income motives for migration.

in the source has a small negative impact on immigrant quality.[22] Ramos (1992) finds that U.S. immigrants from Puerto Rico are on average less educated than Puerto Ricans who remained in Puerto Rico. Further, those U.S. immigrants who subsequently returned to Puerto Rico were more educated than the pool of migrants that did not return. Since the return-to-skill in Puerto Rico was higher than that on the U.S. mainland, this result is consistent with Borjas' selection theory. Nonetheless, we argue that the findings of Ramos (1992) are also consistent with the relative deprivation theory since the less educated tend to be the more relatively deprived.

The more recent evidence on Borjas' selection theory is mixed. Liebig and Sousa-Poza (2004) look at the *intention* to migrate using data from the 1995 International Social Survey Programme for a cross-section of 23 countries.[23] Again, their dataset is cross-sectional so they cannot control for selection-on-unobservables. Nonetheless, the survey asks whether the respondent is willing to move to another country to better work or living conditions. They correlate this with measures of income inequality (including the Gini coefficient) in the source country, while controlling for other individual socio-demographic characteristics. They find that higher income inequality in the source is correlated with a higher aggregate propensity to migrate even after controlling for the level of income. Stark (2006) interprets this finding of Liebig and Sousa-Poza as evidence in favour of relative deprivation and shows algebraically that his measure of relative deprivation is positively related to the Gini coefficient. Interestingly, however, Liebig and

[22]The negative effect of source income inequality on U.S. immigrant quality vanishes when income per capita in the source is controlled for. Borjas suggests this is due to the high negative correlation between income inequality and income per capita across countries. Using the change in the percentage of GNP that is spent by government in the source as a proxy for the change in income inequality over time, Borjas finds that this measure is positively correlated with the change in immigrant quality, which is consistent with his selection theory.

[23]The authors argue that, by studying the intention to migrate, they reduce the difficulties with identifying selection that occur when using host country data on migrants from different source countries. In particular, the skills of immigrants are likely to be highly distorted by (skill-biased) immigration policy and migration networks. In our empirical analysis, immigration policy is not an issue since we study interstate migration.

Sousa-Poza (2004) find that the positive effect of education on migration is much larger than the negative effect of income inequality and so conclude that migrants are typically positively selected (on education) irrespective of income inequality. In other words, higher income inequality reduces the positive selection of migrants, but it remains positive. This is not what Borjas (1987) or Stark predicts; it is however consistent with Borjas (1991) in which he extends his earlier (1987) theory to allow for selection on observables (such as education) as well as selection on unobservables. Indeed, Borjas (1991) shows that – assuming observable education is uncorrelated with the unobservables – it is possible to have positive selection on education and negative selection on unobservables (or vice versa). This would occur if the return to education is higher in the destination than the source and yet, income inequality within the group of persons with the same observed education is higher in the source than the destination. Whilst theoretically possible, we would expect (and hope) that education and unobserved ability are positively correlated. Therefore, Liebig and Sousa-Poza (2004) (who make no mention of relative deprivation) suggest it is evidence in favour of Chiswick (1999), which can be viewed as an extension of Borjas (1987) to a situation where time-equivalent migration costs are decreasing with ability and predicts positive selection of migrants.[24]

More specifically, Chiswick (1999) hypothesizes that migration costs have a shorter time-equivalent for high-ability (and therefore high-income) workers compared to low-ability (and low-income) workers.[25] Consequently, migrants are positively selected from the source skill distribution. In contrast, Borjas (1987) assumes the time-equivalent mov-

[24]More recent theoretical contributions include Clark et al. (2007) who extend the Borjas model to account for non-pecuniary benefits and various costs of migration but do not consider relative income (or relative deprivation) motives.

[25]This can be achieved in a number of ways. Chiswick (1999) first assumes that out-of-pocket costs are independent of individual ability such that time-equivalent migration costs are lower for higher ability workers (that is, the same cost is scaled by a higher wage for higher ability workers). Alternatively, as Chiswick (1999) says, if high-ability workers are more efficient at moving then they have lower absolute out-of-pocket migration costs and, additionally, may spend less time on migration thereby reducing foregone earnings (therefore, forgone earnings are not a constant proportion of earnings across abilities).

ing cost is identical for all skill types (that is, the cost of migration is proportional to the source wage). In summary, Chiswick (1999) predicts that, although Borjas' mechanism is still valid, it is not enough to overturn the positive selection; hence Borjas' mechanism (that higher source income inequality implies negative selection from the source) merely leads to 'less favourable selectivity' but, importantly it is still positive.[26] Chiquiar and Hanson (2005) use the 1990 and 2000 Mexican and U.S. Censuses and find that, rather than Mexican migrants being selected from the left tail of the (more unequal) skill distribution in Mexico (as Borjas' theory would suggest), they tend to come from the middle. The authors propose that this is consistent with Borjas' theory if the costs of migration fall with education, as hypothesised by Chiswick (1999). In recent work, Ambrosini and Peri (2011) find support for Borjas' theory from individual-level panel data on Mexico-U.S. migration. They control for selection on observables and unobservables and find that on average there is negative selection of U.S. immigrants from Mexico. This is consistent with Borjas' story because income inequality is higher in Mexico than the U.S.. Importantly, they find that almost all of the negative selection is on unobservable characteristics, which they claim is why cross-sectional studies of Mexico-U.S. migration (such as Chiquiar and Hanson (2005)) do not find negative selection. In terms of how this differs from our work, the authors do not consider relative income (or relative deprivation) as an explanatory variable for migration and, therefore, cannot distinguish between Borjas and Stark. Indeed, given that income is more unequal in Mexico than the U.S., evidence of negative selection is consistent with both Borjas and Stark's story – the low-skilled tend to be the relatively deprived..

The notion that relative income – in addition to absolute income – may drive migration

[26]There is some evidence that after some initial downgrading of earnings for new immigrants, eventually the earnings of the foreign-born outperform those of natives, even after controlling for observable characteristics such as education (see Chiswick (1978, 1986a,b) for the U.S. and Bloom and Gunderson (1991) for Canada). This suggests positive selection of migrants, although Borjas (1985) questions the overtaking for the U.S..

choice is persuasive given the recent evidence that subjective well-being (or happiness) is increasing in relative income as well as absolute income. There exist a number of country-level surveys that ask people to rate how happy they feel on a scale, for example, from 1 to 10. In a series of papers Richard Easterlin found that, whilst rich people are happier than poor people within the same country, across countries those in rich countries were on average no happier than those in poor countries (Easterlin, 1974, 1995, 2001).[27] This became known as the Easterlin paradox. Further, while at any point in time the rich are markedly happier than the poor within a country, over time as per capita incomes have increased there has been no discernible change in happiness (see, for example, Frey and Stutzer (2002) and Layard (2005)). A common explanation advanced by Easterlin and others is that happiness depends on relative income; that is, a person compares his income to the incomes of those in the same country or locality. Moreover, above a threshold income needed to buy the essentials in life, happiness seems to be determined solely by relative income.[28]

Luttmer (2005) uses the U.S. National Survey of Families and Households to study the relationship between individual well-being (measured by self-reported happiness) and average income in the Public Use Microdata Area (PUMA) that the individual inhabits. Luttmer finds that, controlling for own household income, an increase in PUMA average earnings reduces reported happiness. Importantly, the result holds after controlling for individual fixed effects. Also, while the coefficient estimate on own household income is positive and larger (in absolute value) than the coefficient estimate on PUMA average earnings, they are not statistically different from each other; hence, Luttmer cannot reject the hypothesis that only relative income matters.

[27]A notable exception is the very poor countries who are less happy.

[28]The idea that relative income – and, in particular, relative deprivation – matters for well-being has been met with increasing acceptance in social psychology. The term relative deprivation was first coined by Stouffer et al. (1949) to explain why army personnel satisfaction increased with army rank.

However, the relative income hypothesis has been heavily disputed by Deaton (2008) and Stevenson and Wolfers (2008). They find that rich countries are happier than poor countries and, the ratio is roughly the same as that between rich people and poor people within the same country. Moreover, Stevenson and Wolfers (2008) find no evidence of a satiation point for happiness as income grows – only absolute income matters. In response, Layard et al. (2010) argue that Deaton (2008) and Stevenson and Wolfers (2008) are mainly cross-sectional in nature and so it is unclear whether income is proxying for unobservables. Focussing on developed countries, Layard et al. (2010) find that – within-countries and *over time* – there is a positive link between relative income and happiness. In the U.S., average happiness has not risen since the 1950s and this is at a time when average income has increased dramatically. Stevenson and Wolfers (2008) admit that this is something of a puzzle. Easterlin et al. (2010) look at a large sample of both developed and developing countries and find that over time happiness does not increase with a country's income.

A very much related literature is that which uses external habits (that is, keeping-up or catching-up with the Joneses) to explain a broad range of anomalies in economics (Clark et al., 2008), including mortality (Wilkinson, 1996). Theoretically, relative income can also provide an explanation for the phenomenon that is return migration (Stark, 1991). Return migration – the process of returning to a region once resident in – represents a large percentage of two-way migration flows (see, for example, Eldridge (1965) for U.S. interstate migration). A lower average income in the source than the host region may provide an incentive to return to the source if individuals care about their position in the income distribution.

Finally and more generally, our paper is related to the large empirical literature on the determinants of migration. A vast number of papers (far too many to mention) have found that absolute income differentials influence migration (see Greenwood (1975, 1985) for surveys on the determinants of internal migration). These papers do not control for relative income. In our empirical analysis we will want to control for those variables that explain migration and are potentially correlated with individual income and average income in a region. Potential confounding factors are regional compensating differentials that act as a counterbalancing force to the income differential between the source and destination. Examples of compensating differentials mentioned in the literature are differentials in the unemployment rate (Todaro, 1969, Harris and Todaro, 1970); prices (Djajic, 1989, Dustmann, 1995, 2003, Stark et al., 1997, Dustmann and Weiss, 2007); and climate (Graves, 1980) between the source and destination. In our estimations of the propensity for interstate migration, we control for these at the state level. In addition, we control for a number of personal characteristics that have been found to influence migration.

2.1 Discussion

There is a related and interesting side order. Given the large, persistent differences in per capita income that exist across countries and regions, a migration theory based on income maximisation alone would seem to predict much larger migration flows than we actually observe. To reconcile this, the advocates of income maximisation have offered three explanations. The first is that international migration is highly regulated and so there are people that want to move but do not meet the criteria for legal entry. Whilst certainly part of the story, it is clearly not a full explanation because there are many counterexamples. Indeed, where migration is unregulated (such as within the European Union and regionally), big differences in per capita incomes exist yet only a small portion of the population migrate.[29] Second, the absolute income camp would

[29] Eurostat figures for 2010 show that just 3.2 percent of European Union residents were born in a different member state to the one they currently reside in.

argue that unemployment (or the probability of finding a job) is the counter-balancing force (Todaro, 1969, Harris and Todaro, 1970).[30] However, there is little evidence to support the Harris-Todaro prediction of compensating unemployment differentials for wage differentials, at least among the less educated (see, for example, Fields (1982) for Colombia and Schultz (1982) for Venezuela). The implication is that expected income differentials across regions exist. Indeed, as Raimon (1962) finds, the U.S. states with above average earnings tend to have above average employment increases. Third – and we think the most convincing response – is that the costs of migration (monetary and psychic) are very high. Since the monetary (one-off) costs of moving would have to be implausibly high, it appears non-pecuniary (or psychic) costs are large. It is, however, not satisfactory to have no theory to explain (endogenise) these non-pecuniary costs. There are a couple of candidates; place attachment is the obvious one but another is relative income (or relative deprivation).

3 The Issue

In this section, we will show that – under some conditions – all three theories (absolute income, relative income and relative deprivation) predict the same aggregate relationship between (1) income inequality and the selection of migrants and, (2) income inequality and the outmigration rate from the source. To be clear, there is no general result here; we simply show that under some conditions the three theories lead to the same predictions.

[30] Todaro (1969) presents a model of rural-urban migration in less-developed countries based on the expected wage differential – that is, the product of the urban-rural earnings differential and the probability of being employed. In Todaro (1969), increased rural-to-urban migration reduces the expected urban wage because it reduces the probability of employment. Accounting for the probability of unemployment can simultaneously explain two phenomena: persistent wage differentials across regions (unemployment is the clearing mechanism in the presence of urban wage rigidities) and yet continued migration to urban areas facing unemployment. Although Todaro's model is targeted at explaining rural-urban migration in LDCs, it has relevance for regional and international migration. Furthermore, Todaro's mechanism moves towards a general equilibrium framework since migration affects the probability of employment.

Indeed, a simple counterexample is all we need to refute the claims of those that purport empirical evidence on migrant selection to prove or disprove any one theory. To show this we take Borjas' (1987) absolute income model of migration and extend it to include a relative income and a relative deprivation motive for migration.[31]

We make three assumptions: (A1) the distribution of skill (or ability) in the source is Normally distributed; (A2) the ordinal ranking of individuals in the source does not change if moved to the destination (that is, if we moved the whole population of the source to the destination, the ordinal ranking of these individuals in the destination income distribution is unchanged from that in the source income distribution); (A3) migration is modelled as a one-shot decision (static model), there are no strategic interactions between individuals and no feedback effects of migration on income.[32] Regarding assumption A3, it will help to think of our model as an experiment where we consider simultaneously moving everyone in the source to the destination and we ask who in the source is likely to agree to this. The simultaneous movement of everyone allows us to ignore general equilibrium effects of migration.[33] One could get different results from changing one or more of these assumptions. We make such stark assumptions for the sake of clarity and exposition, but these assumptions are relaxed later in the empirical analysis.

In what follows we use a subscript 0 to denote the source and a subscript 1 to denote

[31]Borjas (1987) formalises the Roy (1951) model of self-selection into different occupations and applies it to migration.

[32]Borjas (1987) assumes (A1) and (A3); among others, Borjas and Bratsberg (1996) assumes (A2). These assumptions are more restrictive than we actually need; for example, the (ordinal) ranking of skills in the destination and source need not be identical – our results would go through if the correlation between the income distribution of the source and destination is sufficiently high (see Borjas (1987)). These assumptions are not used in the empirical analysis of Section 4.

[33]If interaction or feedback effects of migration occur, then the migration decision of a person will depend on the migration choice of others. For example, immigration will increase labour supply in the destination and this may lower the wage. Also, migration will change the distribution of income for those left behind and this may affect their decision to migrate if individual utility depends on the incomes of others (see Stark (1984)).

the destination. Log income in the source is assumed to be

$$\log y_0 = \mu_0 + \eta\epsilon, \tag{1}$$

where μ_0 is average income in the source, ϵ is skill (or ability) and, $\eta \geq 0$ is the return to skill in the source. We assume skill is unobservable; however, we know that skill in the source population is independent, standard Normally distributed: $\epsilon \sim \mathcal{N}(0, 1)$. Let Y_0 denote the random variable for income in the source, it is Log-normally distributed: $Y_0 \sim \text{Log-}\mathcal{N}(\mu_0, \eta^2)$.

If all those in the source migrate to the destination, log earnings in the destination is assumed to be

$$\log y_1 = \mu_1 + \epsilon, \tag{2}$$

where μ_1 is average income that migrants receive in the destination if all persons from the source migrate to the destination. Notice we have normalised the return to skill in the destination to unity so that η is now the return to skill in the source relative to that in the destination.[34] The relative return to skill in the source, η, is implicitly the outcome of differences in endowments and redistributional policy between the source and destination. The random variable for income (of the source population) in the destination is Log-normally distributed: $Y_1 \sim \text{Log-}\mathcal{N}(\mu_1, 1)$. For expositional purposes, in what follows we will assume that average income is higher in the destination than the source:

[34] We have used our assumption of constant rank here; that is, the distribution of skills in the source is the same as that in the destination. This assumption rules out movement within the skill distribution upon migration – the ordinality (or ranking) of skills is the same in both regions. Borjas' (1987) paper allows for variable correlation (ρ) between the skill distribution of the two regions. Borjas characterises selection conditional on ρ as well as the relative return to skill η. Our model is the special case of Borjas where $\rho = 1$, which is reasonable for U.S. interstate migration since the transferability of skills across states is high.

$E(Y_1) > E(Y_0), \forall \eta$ and $\mu_1 > \mu_0$.[35]

A definition of relative income and relative deprivation needs to be specific about who constitutes the 'reference group' to which income comparisons are made. We assume the reference group is the population of the source; however, whether we use their source incomes or their (potential) destination incomes will depend on whether we assume 'reference substitution' occurs post-migration. For a non-migrant, his or her reference is assumed to be the source income distribution; for a migrant his or her reference remains the source income distribution except when we assume reference substitution takes place, in which case the reference switches to the destination income distribution. Let $F_{Y_j}(y)$ denote the (Log-normal) cumulative distribution function of income in reference j. Then, for an individual with income y and reference j, we define his or her absolute income $(AI(y))$, relative income $(RI(y,j))$ and relative deprivation $(RD(y,j))$ as

$$AI(y) \equiv y; \tag{3}$$

$$RI(y,j) \equiv \frac{y}{E(Y_j)}; \tag{4}$$

$$RD(y,j) \equiv \int_y^{\infty} (x-y) dF_{Y_j}(x) \tag{5}$$

$$= [1 - F_{Y_j}(y)][E(Y_j | Y_j > y) - y] \tag{6}$$

$$= \int_y^{\infty} [1 - F_{Y_j}(x)] dx. \tag{7}$$

That is, relative income $(RI(y,j))$ is the ratio of individual income to average income in the reference group. The symmetric nature of relative income means that a greater sense of happiness (unhappiness) is felt when income is further above (below) the mean. Our measure of relative deprivation in equation (5) is identical to that proposed by Stark (1991). Relative deprivation of a person with income y and reference j, $RD(y,j)$, is the

[35] $E(Y_1) = \exp(\mu_1 + 0.5)$ and $E(Y_0) = \exp(\mu_0 + 0.5\eta^2)$. Therefore, for $E(Y_1) > E(Y_0)$ we require $\mu_1 - \mu_0 > 0.5(\eta^2 - 1)$.

sum of the excess income above y over all those people in j with higher incomes than y. Equation (6) follows directly from expanding the integral in (5). It says that $RD(y,j)$ equals the proportion of people in j with higher incomes than y weighted by their mean excess income over y.[36] Equation (7) results from integration by parts of equation (5) (Yitzhaki, 1979).[37] Note a difference between relative deprivation and relative income as we have defined it above is that relative deprivation is not symmetric: everyone is deprived apart from the top person who feels nothing. This will affect the results.[38] In the remainder of this section, we first solve the model with a utility function that nests the absolute income and relative income motives. The model with relative deprivation is deferred to subsection 3.2.

3.1 Absolute and Relative Income

Assume individual utility depends on both absolute income and relative income. Specifically, the indirect utility of a person with income y is assumed to be

$$U(y,j) = \frac{y}{[E(Y_j)]^{\delta}},$$

where $\delta \in [0,1]$ is the weight attached to relative income in utility. Clearly, if $\delta = 0$ then $U(y,j) = AI(y)$ and, if $\delta = 1$ then $U(y,j) = RI(y,j)$.

First consider the case where post-migration the reference remains the source income

[36] $\int_y^{\infty}(x-y)dF_{Y_j}(x) = \int_y^{\infty} x dF_{Y_j}(x) - y\int_y^{\infty} dF_{Y_j}(x) = E(Y_j|Y_j > y)[1-F_{Y_j}(y)] - y[1-F_{Y_j}(y)]$.

[37] $\int_y^{\infty}(x-y)dF_{Y_j}(x) = (x-y)F_{Y_j}(x)\Big|_y^{\infty} - \int_y^{\infty} F_{Y_j}(x)dx = \int_y^{\infty}[1-F_{Y_j}(x)]dx$.

[38] Although in Stark's measurement of relative deprivation only those with higher incomes are included in the calculation, those on lower incomes affect the calculation via their effect on the weights.

distribution (that is, no reference substitution takes place). Then migration is optimal if

$$\frac{y_1}{[E(Y_0)]^\delta} > \frac{y_0 + C}{[E(Y_0)]^\delta},$$

where $C \geq 0$ is the cost of migration. After taking logs we have

$$\log y_1 > \log y_0 + \log\left(1 + \frac{C}{y_0}\right),$$

which does not depend on δ. We follow Borjas (1987) and assume the time-equivalent cost of migration $\pi \equiv \frac{C}{y_0}$ is constant. This implies that the cost of migration is proportional to income in the source. Then the condition for migration is approximately[39]

$$(1-\eta)\epsilon > -\left(\mu_1 - \mu_0 - \pi\right), \tag{8}$$

and the probability of migration is

$$\begin{aligned} &\Pr(\text{Migrate}) = 1 - \Phi(z^{NRS}); \\ &\qquad \text{where} \quad z^{NRS} = \frac{-\left(\mu_1 - \mu_0 - \pi\right)}{|1-\eta|}, \end{aligned} \tag{9}$$

where Φ denotes the distribution function of the standard Normal and the superscript NRS stands for No Reference Substitution. The selection of migrants from the source income distribution is given by the average income in the source conditional on migration

$$\begin{aligned} E(\log y_0|\text{Migrate}) &= \mu_0 + \eta E\left(\epsilon \left| \frac{(1-\eta)\epsilon}{|1-\eta|} > z^{NRS}\right.\right) \\ &= \mu_0 + \left[\frac{\eta|1-\eta|}{(1-\eta)}\frac{\phi(z^{NRS})}{1-\Phi(z^{NRS})}\right]. \end{aligned} \tag{10}$$

[39] $\log(1+x) \approx x$ for small x.

The term in square brackets is the selection bias of migrants; the sign (or direction) of selection bias hinges on the return to skill (η). If the return to skill is higher in the source ($\eta > 1$), then $E(\log y_0|\text{Migrate}) < \mu_0$, which implies migrants are negatively selected from the source income (or skill) distribution. Recall that negative selection of migrants means that on average migrants are of lower skill (and income) than the average in the source population. Conversely, if $\eta < 1$ then migrants are positively selected from the source; that is, on average migrants have a higher skill than the average in the source population. This is exactly the prediction of Borjas (1987), which is not surprising since setting $\delta = 0$ is Borjas' model. Importantly we have shown that this holds for any $\delta \in [0, 1]$; indeed, assuming no reference substitution, the relative and absolute income models give identical predictions for both the outmigration rate in equation (9) and the selection effect in equation (10). The intuition is simple. When no reference group substitution takes place, the only way to improve relative income is to increase absolute income. Hence, for any $\delta \in [0, 1]$, the individual will migrate if the income differential – net of the migration cost – is positive.

For future reference we note an additional important insight from equation (9). Assume the time-equivalent cost of migration π is sufficiently small that $\pi < \mu_1 - \mu_0$. Hence, $z^{NRS} < 0$ and the average person will migrate. Then, under negative selection ($\eta > 1$) and holding average income constant, an increase in income inequality in the source lowers the outmigration rate since

$$\left.\frac{\partial[1 - \Phi(z^{NRS})]}{\partial \eta}\right|_{\eta>1} = -\phi(z^{NRS}) \left.\frac{z^{NRS}}{(1 - \eta)}\right|_{\eta>1} < 0.$$

Conversely, under positive selection ($\eta < 1$), an increase in income inequality in the source increases outmigration. Of course, this result holds for all δ. The intuition is that a mean-preserving increase in spread (higher η) encourages those above the mean income in the source to stay and those below the mean to migrate. When $\eta > 1$, those below the mean chose to migrate before the increase in spread so they clearly continue to do so after. In contrast, when $\eta > 1$, those above the mean who previously had a very small gain from migration now find it beneficial to stay in the source. This raises an important point, how can a theory based on pure absolute income differentials predict an aggregate relationship between income inequality and migration? The gain from migration is linear in skill[40]; however, the binary migration decision generates a non-linearity between individual skill (and, hence, income) and individual migration.[41]

We now show that the above results hold irrespective of whether reference group substitution occurs post-migration. Assuming reference group substitution, migration is optimal if

$$\frac{y_1}{[E(Y_1)]^\delta} > \frac{y_0 + C}{[E(Y_0)]^\delta}.$$

After taking logs and again assuming $\pi \equiv \frac{C}{y_0}$ is constant, the condition for migration is approximately

$$(1-\eta)\epsilon > -\left(\mu_1 - \mu_0 - \pi - \delta \log\left[\frac{E(Y_1)}{E(Y_0)}\right]\right),$$

and the probability of migration is

$$\Pr(\text{Migrate}) = 1 - \Phi(z^{RS});$$
$$\text{where} \quad z^{RS} = \frac{-\left(\mu_1 - \mu_0 - \pi - \delta \log\left[\frac{E(Y_1)}{E(Y_0)}\right]\right)}{|1-\eta|}, \tag{11}$$

[40]From equation (8), the gain from migration is $\mu_1 - \mu_0 + (1-\eta)\epsilon - \pi$, which is linear in the skill level (ϵ).

[41]A simple application of Jensen's inequality implies that, when the underlying individual relationship between income and migration is non-linear, in the aggregate both average income and income inequality affect migration.

where the superscript RS denotes Reference Substitution. The average income in the source conditional on migration is

$$E(\log y_0|\text{Migrate}) = \mu_0 + \frac{\eta|1-\eta|}{(1-\eta)} \frac{\phi(z^{RS})}{1-\Phi(z^{RS})}. \tag{12}$$

Once again, from equation (12) we see that migrants are negatively selected when $\eta > 1$ and positively selected when $\eta < 1$. Therefore, the selection predictions of Borjas (1987) equally apply to a model of pure relative income ($\delta = 1$) as they do for a model of absolute income ($\delta = 0$), irrespective of whether reference group substitution takes place. Indeed, δ only enters equation (12) through the inverse Mills ratio $\frac{\phi(z^{RS})}{1-\Phi(z^{RS})}$, which is always positive so δ does not affect the sign of selection bias. The intuition is simple. Consider the case of a higher return to skill in the source than the destination: $\eta > 1$. Under $\delta = 0$ there is negative selection because – compared to the destination – in the source low-skill individuals incur a higher markdown in income for their low skill. Under $\delta = 1$ there is negative selection because – compared to the destination – in the source low-income individuals are further away from the mean.

There is another useful insight. From equation (11), the outmigration rate is decreasing in δ; that is, there is lower outmigration under the relative income motive ($\delta = 1$) than the absolute income motive ($\delta = 0$).[42] Intuitively, the reason why there is more migration under $\delta = 0$ is because the mean income is higher in the destination and under $\delta = 0$ individuals care about this mean (holding the return to skills constant), whereas under $\delta = 1$ individuals do not care about the mean but only how far they are from the mean. In our model, lower outmigration necessarily implies greater selection bias of migrants. Finally, the relationship between income inequality and outmigration derived earlier for the case of no reference substitution typically also holds under reference sub-

[42] $\frac{\partial[1-\Phi(z^{RS})]}{\partial\delta} = -\phi(z^{RS}) \log\left[\frac{E(Y_1)}{E(Y_0)}\right] < 0$, which is negative because we assumed $E(Y_1) > E(Y_0)$.

stitution. That is, assuming $z^{RS} < 0$, under negative selection ($\eta > 1$) an increase in income inequality in the source lowers the outmigration rate.[43] Conversely, under positive selection ($\eta < 1$), an increase in income inequality in the source increases outmigration.

3.2 Relative Deprivation

Now assume the indirect utility of an individual with income y and reference j is given by the negative of relative deprivation: $U(y, j) = -RD(y, j)$. Consider Stark's measure of relative deprivation in equation (7), which we reproduce here for ease of viewing

$$RD(y, j) = \int_{y}^{\infty} \left[1 - F_{Y_j}(x)\right] dx \geq 0. \tag{7}$$

Accordingly, everyone is relatively deprived except those with the highest income, who feel nothing. It is easy to show that the first derivative of relative deprivation ($RD(y, j)$) with respect to y is non-positive and its second derivative is positive.[44] Therefore, relative deprivation falls as income rises but at a decreasing rate. Based on this – and assuming migration increases income – Stark argues that the propensity to migrate is highest for the lower-tail of the income distribution since they have the most to gain from a unit increase in income. Consequently, Stark predicts that migrants are negatively selected from the source; that is, migrants have – on average – lower income (and lower skill) than the source average. This is always the case – Stark's work does not predict positive selection. To take one pertinent example, a person on the highest income has no incentive to migrate, his or her relative deprivation is zero and life cannot get better than this. Further, Stark predicts that a rise in income inequality will increase outmigration.

43 $\left.\frac{\partial[1-\Phi(z^{RS})]}{\partial\eta}\right|_{\eta>1} = -\phi(z^{RS})\left.\left(\frac{z^{RS}}{(1-\eta)} - \frac{\delta\eta}{|1-\eta|}\right)\right|_{\eta>1}$, which is negative for reasonable parameter values.

44 $\frac{\partial RD(y,j)}{\partial y} = -\left[1 - F_{Y_j}(y)\right] \leq 0$ and $\frac{\partial^2 RD(y,j)}{\partial y^2} = f_{Y_j}(y) > 0$, where $f_{Y_j}(y)$ is the density function corresponding to the distribution function $F_{Y_j}(y)$.

Stark's predictions on selection and the aggregate relationship between income inequality and the outmigration rate should be contrasted with those that we derived for Borjas' absolute income model and the relative income model. There are two clear differences. First, when $\eta < 1$ Borjas (and the relative income model) predicts positive selection, whereas Stark never predicts positive selection. Second, when $\eta > 1$, Borjas (and the relative income model) predict that an increase in income inequality in the source decreases the outmigration rate, whereas Stark predicts the opposite. The reason for the difference is that Stark's measure of relative deprivation is asymmetric: when people compare themselves they look up at those people on higher incomes; they do not look down at those on lower incomes.

There is something missing from our above representation of Stark's theory in the sense that no mention was made of the incomes on offer in the destination. We now consider what happens when we account for the income distribution in the destination, separately for the cases of no reference substitution and reference substitution.

First, assume no reference substitution takes place post-migration. Then an individual will optimally migrate if there is an absolute income gain – net of migration costs – to be made. Whilst it is true that the most deprived have the most to gain from a unit increase in income, one needs to take account of how the income offered in the destination varies by skill. If average income is higher in the destination but income inequality is higher too, then the low skilled will gain less (or lose more) from migration. Therefore, at least when looking at migration from the source to a particular destination, it is not necessarily true that migrants are negatively selected when one accounts for the distribution of incomes in the destination. Empirically, when estimating the effect of relative deprivation on the propensity to migrate, it is crucial that one controls for the absolute income gain from migration.

Now consider what happens when reference substitution takes place post-migration. To do this one needs to know what a person with income y in the source earns in the destination post-migration. This mapping is possible because of our assumption that rank is preserved under migration. To this end, define $p \equiv F_{Y_j}(y)$ as the rank (or percentile) of an individual with income y in the income distribution of the reference j. Since income is monotonically increasing in skill level ϵ, it is also true that $p = \Phi(\epsilon)$. For ease of exposition, let $\log y_j = \mu_j + \sigma_j \epsilon$ such that $Y_j \sim \text{Log-}\mathcal{N}(\mu_j, \sigma_j^2)$. From equation (7), the relative deprivation of an individual with income y in reference j can be written as

$$
\begin{aligned}
RD(y,j) &= \int_y^\infty \left[1 - F_{Y_j}(x)\right] dx \\
&= \int_y^\infty \left[1 - \Phi\left(\frac{\log x - \mu_j}{\sigma_j}\right)\right] dx \\
&= \sigma_j \int_\epsilon^\infty [1 - \Phi(z)] \exp(\sigma_j z + \mu_j) dz \\
&= \sigma_j \int_{\Phi^{-1}(p)}^\infty [1 - \Phi(z)] \exp(\sigma_j z + \mu_j) dz \\
&\equiv RD(p,j),
\end{aligned}
$$

where the third equality uses the change of variables $z = \frac{\log x - \mu_j}{\sigma_j}$. To see the effect on relative deprivation of switching reference j (to the destination income distribution) holding rank p constant, we compute the partial derivative of $RD(y,j)$ with respect to the scale (or variance of log income) parameter σ_j. Clearly, $\frac{\partial RD(p,j)}{\partial \sigma_j} \geq 0$. That is, switching reference to a more unequal income distribution increases relative deprivation for everyone.[45] Therefore, conditional on reference substitution and our assumptions, the relative deprivation theory predicts zero migration to a destination with a more unequal

[45]The exceptions are those people with the highest skill (or, equivalently, highest income), they are indifferent, which is why the inequalities are weak. The result that relative deprivation is increasing in the variance of the income distribution is sensitive to the nature of the mean-preserving spread. See the appendix of Deaton (2001) for a counterexample where a mean-preserving spread is achieved by hollowing out the distribution.

income distribution than the source. Conversely, moving to a destination with a more equal income distribution leads to a reduction in relative deprivation. To determine the selection bias of migrants, we would like to know how this reduction in deprivation varies by rank p. The cross-partial derivative of $RD(p, j)$ with respect to σ_j and p is

$$\frac{\partial^2 RD(p, j)}{\partial p \partial \sigma_j} = -\frac{\left[\sigma_j \Phi^{-1}(p) + 1\right](1 - p) \exp\left(\sigma_j \Phi^{-1}(p) + \mu_j\right)}{\phi(\Phi^{-1}(p))} < 0.$$

The cross-partial implies that, when the destination is more equal than the source, the low skill (low rank) have a bigger incentive to migrate compared to the high-skill (high rank) individuals. When the costs of migration are factored in, this would imply negative selection of migrants. Recall that this is exactly what Borjas' absolute income model (and the relative income model) predicts.

Furthermore, conditional on non-zero migration, the relative deprivation hypothesis predicts that an increase in source inequality increases outmigration. Recall that, in contrast, we showed that Borjas predicts – conditional on negative selection *and that the average person migrates* – there is a negative relationship between source income inequality and the volume of outmigration. This divergence is due to the asymmetric nature of the relative deprivation measure. However, if instead the average person chooses not to migrate – which is the most likely scenario – then Borjas predicts a positive relationship between source income inequality and outmigration. Therefore, in aggregate data the absolute and relative deprivation hypotheses tend to yield the same predictions.

In summary we have shown that, under some conditions, the three theories (absolute income, relative income and relative deprivation) lead to the same predictions for the aggregate relationship between income inequality and selection, and income inequality and the outmigration rate. The confounding is made worse by aggregation. At the individual-level, variation in the three measures and individual migration can be used to jointly estimate the relative importance of the three theories. There are two types of useful variation. The first is variation in the three measures across individuals in the source. Indeed, across individuals the values for absolute income, relative income and relative deprivation are not perfectly correlated when the individuals belong to different source reference groups (say, different U.S. states). For example, two people with the same income will not have the same relative income or relative deprivation if they belong to different reference groups (and these reference groups have different values for mean income and income inequality). However, this variation is of little or no use in distinguishing the absolute income model from the relative income model when no reference group substitution occurs – a rise in income is the only way to improve relative income when no reference substitution occurs. The second is variation in the three measures between the destination and the source. Upon migration to the destination individuals receive a new income, a new relative income and a new relative deprivation. As the opening quote to this paper by Layard (2005) suggests, evidence that individuals migrate to improve relative income (or lower deprivation) even when doing so involves taking a cut in absolute income would represent clear evidence against the absolute income hypothesis and in favour of relative income (or deprivation) and reference substitution post-migration.

Unfortunately, individual-level panel data on international migration does not exist; hence, the empirical literature on testing Borjas and Stark's theories is dominated by cross-sectional studies.[46] With cross-sectional data, one either has pre-migration outcomes or post-migration outcomes, but by definition not both. Typically – as is the case with the U.S. Census and the Current Population Survey – cross-sectional datasets record

[46]For confidentiality reasons, no statistical agency would ever release the personal information (names) needed to link the pre and post international migration records of migrants. Abramitzky et al. (2011) manage to link – by name of the person – the 1900 U.S. Census with the 1865 Norwegian Census but, of course, these people are long dead so confidentiality is no longer an issue. The best that international studies can do is to categorise individuals with similar observable characteristics into cohorts and link the cohorts across surveys (see, for example, Ambrosini and Peri (2011)). Some studies have inferred outmigration from sample attrition but this is guesswork. In general countries make either very little or no effort to record outmigration.

post-migration (or end-of-period) outcomes.[47] This leads to an endogeneity problem because the migration choice effects post-migration outcomes. Any variable that is either directly or indirectly chosen by an individual after migration is potentially determined endogenously – including income, employment status and education, among many others. Typically only age, race and gender may be considered exogenous to the migration decision.

There is, of course, an even bigger problem with cross-sectional data; that is, one cannot control for unobserved heterogeneity (for example, innate ability and intrinsic motivation). With no information on income prior to migration, one needs to estimate the (counterfactual) non-migrant earnings of migrants. This is done by estimating an earnings equation using only the subsample of non-migrants, and then using the coefficient estimates on the regressors to predict counterfactual earnings for migrants. Since migrants are self-selected, one needs to control for the selection bias that arises from estimating an earnings equation using only the subsample of non-migrants. Failure to account for selection will bias the predicted counterfactual earnings. Indeed, we know the majority of earnings variation is due to unobservables (Autor et al., 2008). If migrants are selected-on-unobservables, and these unobservables have a direct effect on earnings, then the counterfactual income estimates will be biased.

Consider Stark and Taylor's (1989, 1991) cross-sectional finding that, after controlling for the expected absolute income gain, relatively deprived Mexican households were more likely to migrate to the United States.[48] The authors estimate counterfactual earnings

[47]This is naturally the case because one only realises migration after migration takes place and most socio-economic variables are recorded at the time of the survey. For example, the U.S. Census long-form questionnaire asks respondents where they lived five years earlier, which along with the current region of residence identifies migrants and non-migrants over a five-year period. All other questions – for example on income and employment status – refer to the year immediately preceding the Census and, hence, better reflect end-of-period outcomes.

[48]Stark and Taylor (1989, 1991) argue that the reference group of the Mexico-to-U.S. migrants does not change post-migration because at least some household members stay in their Mexican village and the migrants remit (they also do not stay in the U.S. for long).

of migrants and non-migrants using the observed earnings of non-migrants and migrants, respectively. In doing so, they correct for selection-on-observables into the sample of migrants and non-migrants using Heckman's procedure. Using the estimated counterfactual earnings of migrants, they compute relative deprivation for each household in their Mexican village's income distribution and include this as the variable of interest in a probit or logit model for the probability of migration. The problem is that, if migrants are *positively selected on unobservables*[49], then the selection equation fails to fully capture the negative selection of non-migrants and the estimated coefficients in the non-migrant earnings equation are biased downward. In turn, the predicted non-migrant earnings of migrants are underestimated because they are constructed from the attenuated coefficient estimates of the non-migrant earnings equation. This systematically shifts down the estimated position of migrants in the source income distribution, which biases the result in favour of Stark-Taylor's finding that relative deprivation increases the probability of outmigration. This potential bias is pertinent for two reasons. First, we know that unobservables account for most wage variation (Autor et al., 2008). Second, it is precisely when the opposite of Stark's relative deprivation theory occurs (that is, positive selection) that systematically biases the result in favour of Stark-Taylor's finding of negative selection.

If individual-level panel data on before and after migration outcomes were available then there would be no problem. One could directly observe income – and, hence, be able to compute relative income – prior to migration. Furthermore, when predicting the counterfactual (migrant) earnings of non-migrants, one can control for unobserved heterogeneity. This leads us to the next section, which uses a panel dataset on interstate migration in the U.S. to estimate the relative importance of the absolute and relative income theories.

[49] Or observables omitted from the selection equation.

4 Empirics

In this section we estimate the relative importance of absolute income, relative income and relative deprivation in determining interstate migration in the United States.[50]

4.1 The Data

The data is from two main sources, the University of Michigan's Panel Study of Income Dynamics (PSID) 1968-2009, and the March Current Population Survey 1968-2009. The PSID is a panel survey that since 1968 has continuously followed 4,802 original families living in the United States.[51] The sample size has grown substantially over time as individuals have split-off to form new households and the additional household members have been added to the sample. The survey is annual from 1968 to 1997, and biennial since 1997. Crucially for our purposes, the PSID records the U.S. state of residence at the time of the survey. From this we construct an indicator variable for (in-sample) interstate migration. We assume it is infeasible to migrate interstate more than once within a two-year time span and, hence, we can continuously track an individual's migratory behaviour whilst in the PSID. This is consistent with the United Nation's definition of migration based on length of stay, which requires a change in the place of primary residence for a period of at least a year.[52] In a small number of cases, due to missing values and non-response the gap between records exceeds two years. Since it is crucial that we know the timing of migration, we code the migration decision as missing immediately prior to

[50]It is worth noting that even if panel data existed on international migration, studying regional (interstate) migration would still have a couple of advantages over international migration. First, international migration is heavily influenced by government immigration policy, which is a major influence on the selection of migrants. Second, studying regional migration circumvents problems with non-comparability of, for example, reported education of international migrants and natives.

[51]The PSID is the world's longest-running panel survey. In some cases, individuals and their family unit have been followed for 42 years, which allows for an analysis of migration over the life-cycle.

[52]People moving for a period of less than one year are termed visitors, not migrants. Nonetheless, the results do not significantly change if we drop the biennial observations.

a gap in records of more than two years.

Our measure of individual income in the PSID is pre-tax total labour income, which is the sum of wages, bonuses and the amount of business income attributable to labour.[53] We choose to use total labour income rather than the wages and salaries series because the latter excludes the earnings of the self-employed.[54] Income refers to that in the year prior to the survey so we lag income one year for the annual survey years and two years for the biennial survey years. We express income in constant 1999 dollars using the U.S. CPI-U index. In addition to income, the PSID records an array of individual socio-economic characteristics. In particular we will make use of age, years of schooling, whether the individual has a college degree, marital status, number of children, employment status and home ownership.[55] Our estimations use the sampling weights supplied by the PSID.[56]

Our working PSID sample consists of those individual-year observations that satisfy all of the following criteria: (1) the individual is the household head; (2) the individual is of working age (16-64); (3) the individual is in the labour force at the time of the PSID survey; and (4) the individual is non-institutionalised and not in the armed forces (and living off base). The motivation for the sample selection criteria is the following. First, we restrict the analysis to heads of households since we feel that – of all family members – the head is most likely to make migration decisions. In reality migration is likely to be a joint decision between the head and "wife" (if present) but including both would be double-counting. Naturally a better model would treat the family as the decision maker

[53]This is not ideal, we would prefer income to be measured after-tax and inclusive of benefits. We made no attempt to calculate after-tax income since the PSID does not record the necessary data to do so.

[54]As a robustness check, we also present the results for when the self-employed are excluded from our sample and our findings are not significantly altered.

[55]We will control for individual fixed effects, hence time-invariant explanatory variables such as gender and race are redundant.

[56]Of the original PSID families, 1,872 were low-income families from the Survey of Economic Opportunity (SEO). The sampling weights account for their over-selection. Also, the sampling weights are time-varying to adjust for sample attrition.

and optimise subject to the bargaining weights of each family member and their personal circumstances; however, this is beyond the scope of the paper. Moreover, the PSID records far more information about the head than any other family member. Second, we want to include only those in the labour force since the migration theories that we wish to test speak about income and using migration as a means to improve income or relative income. Therefore, these theories will only be appropriate for those individuals that are either working or looking for work. Finally, we drop those in institutions since the migration of these groups – if it occurs while institutionalised – is typically for involuntary reasons. In particular, those in the armed forces (living on or off base) are often moved as part of their job. The Appendix contains the source and construction of each PSID variable used in the empirical analysis.

We assume that the reference group for an individual is the whole population of the U.S. state of residence. That is, people compare themselves to all those resident in the same state at the same time.[57] Although this definition implies reference group substitution, we can roughly infer what happens to relative income and relative deprivation in the absence of reference substitution by simply looking at the change in absolute income.

The March Current Population Survey (CPS) is used to compute the income distribution for each state-year.[58] The CPS is an annual, large and representative cross-sectional sample of the U.S population. Nonetheless, we apply the sampling weights supplied by the CPS. The CPS income series we use is total (pre-tax) income from wages and salaries.[59] We lag income to account for the fact that income refers to that in the year prior to the

[57]In reality the reference group may be much narrower than the state. The publicly-available PSID files only record the U.S. state that the household resides and not the county or ZIP. Narrower geographical identifiers are available on application and this seems a promising area for future research. State identifiers, however, have one advantage in the sense that we are interested in a study of migration and not commuting.

[58]We accessed the March CPS through the University of Minnesota's IPUMS-CPS.

[59]The alternative is to use the CPS total personal income series; however, this includes asset income not due to labour which is a substantial deviation from our PSID labour income series.

survey. Income is expressed in constant 1999 dollars using the CPI-U. We restrict the sample to individuals of working age (16-64) that report being in the labour force and, have a strictly positive CPS sampling weight. Pre-1976 the CPS grouped some of the smaller states together. We drop the observations where the state cannot be uniquely identified. This leaves us with 19 states 1968-1971 and 13 states 1972-1975. From 1976 we have data on all 50 states plus the District of Columbia.

From the CPS distribution of income in each state-year we compute relative income and relative deprivation for each PSID individual-year observation. Relative income is measured as the ratio of PSID individual income to the CPS average (mean) income in the state-year reference. Relative deprivation for each PSID individual-year observation is constructed using the expression in equation (6). We compute this in Stata by taking the following steps. First we append our PSID observations to our CPS dataset. We assign the PSID observations a (approximately) zero CPS weight.[60] Separately for each state-year, we compute the empirical cumulative distribution function for income (F_{Y_j}) using the CPS weights. Tied income values (of which there are many) receive the same cumulative value. Second, we estimate the sample analogue of $E(Y_j|Y_j > y)$, which is the sample mean income of all those individuals in state-year reference j with higher income than y. We then have all we need to calculate relative deprivation for each PSID observation.

Finally, we will want to control for possible state-level compensating differentials, including the state price level, unemployment rate and climatic conditions. It is important that we control for *state-level* consumer prices in our regressions because they are positively correlated with average state income. Moreover, if state price levels matter for migration choice, then this is consistent with the absolute income motive, where of

[60] This way the PSID observations are treated as part of the income distribution, which means Stata's 'cumul' command locates their position in the empirical income distribution, yet their (close to) zero weight ensures they have a negligible effect on the income distribution.

course it is real income adjusted for state-level prices that matters. Unfortunately, official estimates of state-level prices do not exist. We construct state-level price indexes using the estimates of state-level prices in Aten (2007) for the year 2000. To get a state-level time-series, we apply the CPI-U inflation for the main metropolitan area in the state (or simple average of the metropolitan area CPI-Us when more than one was available per state). If no metropolitan area CPI-U is available for a state then we use the CPI-U for the region that the state belongs to. We normalise state level prices such that, in each year, the average price level across all states is one.[61]

Estimates of the annual average unemployment rate by U.S. state are obtained from the Bureau of Labor Statistics (BLS). From 1976 onwards the BLS publishes official annual average model-based estimates for the state unemployment rate.[62] This covers the 50 U.S. states and the District of Columbia. In order to backcast these estimates to 1968, we use two sources. First, the BLS provided us with annual average CPS-based estimates from 1970 to 1975 for 29 states (the larger ones). The less populous states have very small samples – too small for reliable estimation. These CPS estimates are not directly comparable to the official model-based estimates from 1976 onwards. Second, prior to the early 1970s, states produced estimates independently using their own methodology. These estimates for the years 1963-73 and 1973-77 are reported in the "Manpower Report of the President" for all 50 states plus the District of Columbia. These estimates are neither comparable to the official BLS data from 1976 nor the CPS-based estimates from 1970-1975. Importantly, the "Manpower" time series 1973-77 overlaps the official BLS series; hence, we backcast the official BLS time series for each state by applying the ratio of the

[61]Note that, in additional to our previous theoretical arguments for the need to use individual-level data to distinguish between the three theories, there are additional empirical reasons too. First, in aggregate data the correlation between income inequality and average income is strong – in the U.S. poor states tend to be more unequal, which confounds the sum of relative deprivation over individuals and average income. Second, prices tend to be higher in richer regions (the Penn effect).

[62]We downloaded the data from the BLS Local Area Unemployment (LAU) Statistics website: www.bls.gov/lau/rdscnp16.htm.

two time series in the overlapping year and apply the ratio backwards. Where possible, we switch to the CPS-based estimates for the 1970-75 period, again by applying the ratio of the series to that of the "Manpower" series in the overlapping years.

Our data on climatic conditions for U.S. states is from the National Climatic Data Center (NCDC) of the National Oceanic and Atmospheric Administration (NOAA). The climatological variables are temperature (in °F), precipitation (in inches), heating and cooling degree days. The raw data consists of monthly time series from 1895 to 2010 for the 48 conterminous states. The District of Columbia is not separately identified therefore we simply assign it the values for Maryland. Heating and cooling degree days are indicators of the demand for heating and cooling, respectively, where heating days are those days where the average temperature is below 65°F and cooling days are days where the average temperature is above 65°F. For example, if the day average temperature is 75°F then that day is given a value of 10 cooling degree days. The monthly figures are monthly averages (for precipitation and temperature) and sums (for heating and cooling degree days). We compute six annual climatic measures: (1) average temperature; (2) max-min temperature (that is, the difference in the average temperature between the months with the highest and lowest temperature); (3) average monthly precipitation; (4) max-min precipitation (that is, the difference in the average monthly precipitation levels between the months with the highest and lowest precipitation); (5) heating degree days; and (6) cooling degree days. We calculate (moving) past 30-year averages to remove year-to-year variations.[63] For example, average cooling degree days in 1990 is computed by taking the average of the 30 values of yearly-summed cooling degree days from 1961 to 1990.

[63]The World Meteorological Organization (WMO) recommends the use of 30-year climate averages.

4.2 Descriptive Statistics

Table 1 displays summary statistics for all the variables used in the analysis. The statistics are unweighted means and standard deviations. The columns split the sample into those observations where an individual migrates interstate (that is, 'Movers') and those observations where an individual does not move interstate (that is, 'Stayers'). To be clear, the reported statistics refer to the year immediately preceding the migration decision. Just over three percent of the total 117,019 individual-year observations are when an individual moves interstate. The sample used to construct the summary statistics is that for which we observe all the variables used in the analysis. By using a subset of the variables one can increase the sample size to a maximum of 128,231 observations – of which 3,851 are 'Movers'.[64] From the 'Total' column we see that the mean income in our PSID sample is 29,927 dollars in 1999 prices.[65] The mean relative income in our sample is 1.14, which implies state mean income is on average 14 percent higher in our unweighted PSID sample than the CPS weighted sample.[66] Now compare the columns for stayers and movers. The mean earnings of movers is 438 dollars less than that for stayers. Similarly, mean relative income is lower and mean relative deprivation is higher for movers. Movers are more likely to be unemployed and less likely to own their own home. Continuing down the table we see that movers are on average 7 years younger than stayers, more likely to hold a college degree, less likely to be married, and tend to have fewer children than stayers. This is consistent with the vast empirical literature that finds migrants

[64]The variables that are constructed using CPS data (relative income and relative deprivation) have fewer observations since, as already mentioned, pre-1976 the smaller U.S. states are not individually recorded in the CPS. Also, the climatic conditions variables are not available for Alaska and Hawaii.

[65]The median income is 24,165 dollars.

[66]Recall that relative income for a PSID individual is the ratio of his or her income to the CPS mean income in his or her state. Therefore, the mean of relative income does not have to equal one because the denominator is not from the PSID sample. Further, PSID labour income is a broader measure of labour income than that of the CPS wages and salaries series. Also, the summary statistics presented in **Table 1** do not use the PSID sampling weights whereas state mean income is calculated using the CPS sampling weights. The mean income in the CPS is $27,543 in 1999 prices.

tend to be young, educated, and single with few or no dependants.[67] The remaining rows of the table summarise the aggregate conditions in the state of residence immediately prior to the migration decision. There is no difference between the average price level in the mover and stayer subsamples. On average movers leave states with a slightly lower unemployment rate than stayers reside in. There is little discernible difference in climatic conditions between the mover and stayer subsamples, although one may say that movers tend to leave states where the climate requires more heating days. Finally, movers tend to leave states with more borders and less land area than stayers reside, although the differences are tiny.

Table 2 displays the frequency distribution for the number of observations per individual in our sample. The second column ('Stayers') shows the frequency distribution for the subsample of individuals that (in-sample) do not move interstate; the third column ('Movers') shows the corresponding distribution for those individuals that move interstate at least once in-sample. In total there are 14,332 individuals in our sample and 2,222 of these are in-sample movers. A number of these movers move multiple times which explains why the number of moves in **Table 1** is higher than the number of movers. To control for unobserved individual heterogeneity we need at least two observations per individual. Around 15 percent of individuals are only observed once – these will be dropped in the fixed effects estimations.[68,69] The average number of observations per individual is 9, and for the subsamples of stayers and movers the average is 8 and 14 observations, respectively.

[67]See, for example, Greenwood (1975) for a survey of U.S. interstate migration.

[68]To check whether this introduces selection bias we compared the means of the explanatory variables in the full sample with the corresponding means from the sample that drops those observations where an individual is only observed once. We found no significant difference.

[69]Note that the sample selection rules were implemented after we determined who was an in-sample mover, which explains why 45 movers are only observed once after our sample selection criteria have been met.

TABLE 1

Descriptive Statistics: Means and Standard Deviations

Variable	Stayers		Movers		Total	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Income	29,940	33,861	29,502	28,217	29,927	33,705
Relative income	1.14	1.21	1.11	1.03	1.14	1.20
Relative deprivation	10,955	7,656	11,819	8,001	10,981	7,668
Unemployed (d)	0.08	0.27	0.12	0.33	0.08	0.27
Own home (d)	0.53	0.50	0.26	0.44	0.52	0.50
Age	39.5	12.5	32.7	11.0	39.3	12.5
College degree (d)	0.15	0.35	0.24	0.43	0.15	0.36
Married (d)	0.55	0.50	0.44	0.50	0.55	0.50
Children #	1.08	1.35	0.75	1.13	1.07	1.35
State-level variables:						
Price level	1.03	0.10	1.03	0.10	1.03	0.10
Unemployment rate	6.39	1.98	6.23	1.92	6.38	1.98
Temp. average (°F)	55.2	7.34	54.7	7.27	55.2	7.34
Temp. max-min (°F)	43.0	7.93	43.7	8.07	43.0	7.94
Precip. month average (")	3.24	0.96	3.16	1.00	3.24	0.96
Precip. max-min (")	5.31	1.25	5.24	1.31	5.31	1.25
Heating deg. days	4,365	1,878	4,523	1,891	4,370	1,879
Cooling deg. days	1,271	796	1,246	805	1,270	797
Borders #	4.33	1.53	4.45	1.57	4.34	1.53
Land area (km^2/1000)	191	167	187	160	191	167
Observations	113,493		3,526		117,019	

Notes: The reported statistics are unweighted means and standard deviations in our pooled sample. The sample is PSID household heads that are non-institutionalised, of working age, and in the labour force. 'Movers' refer to individual-year observations in the year immediately preceding interstate migration; all other observations are 'Stayers'. *Income* refers to individual pre-tax labour income in 1999 U.S. dollars. *Relative income* is the ratio of individual income to average income in the state of residence. *Relative deprivation* for an individual is the fraction of people with higher incomes than that individual multiplied by their average excess income. (d) indicates that the variable is a dummy that takes the value one if the variable label applies to the individual and zero otherwise (hence, the mean is simply the fraction of observations with this characteristic). The state-level variables refer to the conditions in the state that the individual is resident in the year preceding migration. The *price level* is normalised such that the average across all states is one in each year.

TABLE 2

Distribution of Observations per Individual

Observations per individual	Frequency Stayers	Movers	Total	Percent	Cumulative percent
1	2,083	45	2,128	14.8	14.8
2	1,394	138	1,532	10.7	25.5
3	1,804	155	1,959	13.7	39.2
4	742	137	879	6.1	45.3
5	610	126	736	5.1	50.5
6	594	89	683	4.8	55.2
7	454	93	547	3.8	59.1
8	389	91	480	3.3	62.4
9	359	75	434	3.0	65.4
10	322	85	407	2.8	68.3
11	262	80	342	2.4	70.7
12	253	64	317	2.2	72.9
13	243	67	310	2.2	75.0
14	227	64	291	2.0	77.1
15	178	69	247	1.7	78.8
16	215	57	272	1.9	80.7
17	184	56	240	1.7	82.4
18	167	52	219	1.5	83.9
19	166	46	212	1.5	85.4
20	148	66	214	1.5	86.9
21	143	53	196	1.4	88.2
22	127	48	175	1.2	89.5
23	117	41	158	1.1	90.6
24	111	45	156	1.1	91.6
25	126	36	162	1.1	92.8
26	96	51	147	1.0	93.8
27	87	37	124	0.9	94.7
28	142	35	177	1.2	95.9
29	66	40	106	0.7	96.6
30	62	32	94	0.7	97.3
31	65	34	99	0.7	98.0
32	38	36	74	0.5	98.5
33	44	30	74	0.5	99.0
34	47	15	62	0.4	99.4
35	45	34	79	0.6	100
Total	12,110	2,222	14,332	100	

Notes: The table displays the frequency distribution for the total number of times (or surveys) an individual is observed after our sample selection criteria are met. 'Stayers' and 'Movers' refer to individuals who did and did not undertake (in-sample) interstate migration, respectively.

TABLE 3

Distribution of Observations per Spell for Movers: Pre- and Post-Migration

Observations in spell	Frequency of spells: Pre/post move: Pre	Post	Total	Percent	Cumulative percent
1	609	1,268	1,877	32.2	32.2
2	361	577	938	16.1	48.2
3	218	355	573	9.8	58.0
4	136	266	402	6.9	64.9
5	109	194	303	5.2	70.1
6	101	150	251	4.3	74.4
7	71	144	215	3.7	78.1
8	43	98	141	2.4	80.5
9	66	99	165	2.8	83.3
10	46	83	129	2.2	85.6
11	31	59	90	1.5	87.1
12	25	55	80	1.4	88.5
13	23	58	81	1.4	89.9
14	20	59	79	1.4	91.2
15	20	50	70	1.2	92.4
16	15	42	57	1.0	93.4
17	16	31	47	0.8	94.2
18	12	35	47	0.8	95.0
19	7	31	38	0.7	95.6
20	4	22	26	0.4	96.1
21	3	19	22	0.4	96.5
22	5	23	28	0.5	97.0
23	6	24	30	0.5	97.5
24	7	24	31	0.5	98.0
25	2	17	19	0.3	98.3
26	5	20	25	0.4	98.7
27	2	14	16	0.3	99.0
28	0	10	10	0.2	99.2
29	3	9	12	0.2	99.4
30	2	12	14	0.2	99.6
31	0	9	9	0.2	99.8
32	1	3	4	0.1	99.9
33	1	3	4	0.1	99.9
34	1	3	4	0.1	100
Total	1,971	3,866	5,837	100	

Notes: The table displays the frequency distribution for the total number of times an (in-sample) interstate mover is observed continuously in the same US state (or spell). Columns 2 and 3 split these spells into those that occur pre- and post-interstate migration, respectively.

For movers, we can further divide their observations into those that occur pre- and post-migration. To do this, we define a 'spell' as the length of time during which an individual continuously resides in the same U.S. state. Movers – by definition – have more than one spell. **Table 3** presents the frequency distribution for the number of observations per spell for movers, divided into pre- and post-migration spells.[70] Almost a third of all spells for movers have just one observation. This is partly driven by the fact that a number of individuals move multiple times and, hence, have three or more spells. The average number of observations per spell for movers is 4.6 pre-migration and 5.5 post-migration.

TABLE 4

Distribution of the Number of Migrations per Individual

Migrations	All migrations		Return migrations	
per individual	Frequency	Percent	Frequency	Percent
0	12,110	84.5	13,147	91.7
1	1,172	8.2	893	6.2
2	611	4.3	193	1.3
3	208	1.5	64	0.4
4	109	0.8	24	0.2
5	64	0.4	7	0.0
6	37	0.3	4	0.0
7	16	0.1		
8	2	0.0		
9	1	0.0		
10	1	0.0		
11	1	0.0		
Total	14,332	100	14,332	100

Notes: The table displays the frequency distribution for the number of (in-sample) interstate migrations per individual in our sample. 'Return migrations' refer to those migrations where an individual returns either to his or her state of birth or to a state he or she has (in-sample) previously resided in.

[70]The number of pre-migration spells is 1,971, which is 251 less than the number of movers in **Table 2** because these 251 only have post-migration observations once our sample selection criteria are met.

Table 4 displays the frequency distribution for the number of migrations per individual in our sample. The frequency in the second column is for all migrations. We see that about 84.5 percent never migrate. Of the 15.5 percent that migrate, 47 percent migrate more than once. The fourth column contains the frequency of return migrations – that is, where an individual returns either to his or her state of birth or to a state he or she has previously resided in. The numbers of return migrants are huge: 8.3 percent of all individuals return to a state they have previously resided in, which is over half of all migrants. Further, 25 percent of return migrants return more than once. In terms of the total number of migrations (the product of the first and second columns), 39 percent are where an individual is returning.

In the forthcoming estimations we will look separately at the subsample of returning migrants to see whether the results are driven by this group. One may think that the motives of returning migrants are different from migrants who are leaving a state for the first time. If migrants leave a low-income state for a high-income state then – given the persistence in average earnings – one may expect the reverse to be true for those migrants who subsequently return. If so – and holding individual income constant – returning migrants will improve their relative income and relative deprivation. Also, migrants who intend to subsequently return may be less likely to substitute their reference group towards the host state upon migration from the source.

4.3 Results

We divide the results into three subsections. First, we simply use the subsample of migrants to document what happens to their observed income, relative income and relative deprivation around the time of migration. We make no suggestion of causality here – we merely present correlations. Second, we consistently estimate the counterfactual migrant earnings of non-migrants, correcting for the selection of migrants and endogeneity. Third, we estimate various models for the probability of interstate migration, whilst controlling for the estimated income gain from migration. Fourth and finally, we conduct a number of robustness checks.

4.3.1 On the Outcomes of Migrants

For migrants, we observe their absolute income, relative income and relative deprivation both before and after migration. Therefore, a useful first step in assessing the merit of the migration theories is simply to 'describe' the change in income, relative income and relative deprivation around the time of migration for those individuals that actually migrate. This is the subject of this subsection.

Under the absolute income, relative income and relative deprivation theories of migration one would expect to see an improvement in income, relative income and relative deprivation at the time of migration, respectively. If we were to find that migrants take a pay cut after migration, then this would lead us to doubt the absolute income hypothesis. Such a finding would also constitute evidence against relative income and relative deprivation under an assumption of no reference substitution, since a fall in income whilst holding the reference income distribution constant necessarily reduces relative income and increases relative deprivation. Alternatively, if we were to find that relative income and relative deprivation do not improve post-migration, then this would be strong evidence against reference group substitution.[71]

To be clear, here we merely present a regression 'line of best fit' between either income, relative income or relative deprivation, and migration choice, whilst controlling for other covariates. In other words, it is not a structural equation and, hence, we make no claim

[71] It should be clear that such a finding would not by itself lead us to reject the relative income and relative deprivation hypotheses, since they could still hold under no reference substitution.

of causality – it is in no way a test of the migration theories. Nonetheless, it is a useful descriptive exercise to simply document the regression slope (or correlation) between migration choice and our income-based well-being measures. We will address causality in the next subsection.

To study this, we further restrict our sample to those individuals that have at least one observation either side of (in-sample) migration. The value for the (natural) logarithm of the outcome of interest – either income, relative income or relative deprivation – for individual i at time t is assumed to be given by the unobserved effects model

$$\begin{aligned}\log(outcome_{it}) = \gamma_1 M_{it} + \gamma_2 YSM_{it} + \gamma_3 YSM_{it}^2 + \gamma_4 R_{it} + \gamma_5 RG_{it} \\ + x_{it}'\beta + f_i + \varepsilon_{it}; \quad i = 1, ..., N; \quad t = 1, ..., T_i; \qquad (13)\end{aligned}$$

where $outcome_{it}$ is either income, relative income or relative deprivation; M_{it} (or *migration count*) is the cumulative sum of (in-sample) migrations (for example, during an individual's third spell M_{it} takes the value two); YSM_{it} is years-since-migration (which is zero in the year of migration); R_{it} (or *returned count*) is the cumulative sum of times the individual has returned to a state that he or she has previously resided in; RG_{it} (or *returned-to-grewup count*) is the cumulative sum of times the individual has returned to the state he or she grew-up in; x_{it} is a vector of individual time-varying socio-economic characteristics; f_i is an unobserved individual fixed effect; and ε_{it} is an idiosyncratic disturbance. The control vector x_{it} consists of age, age squared, a dummy variable indicating whether individual i has a college degree at time t, and a full set of year dummies.[72] We include a quadratic in years-since-migration (YSM_{it}) to allow for a post-

[72]Therefore, we have assumed a simple Mincer human capital earnings function (Mincer, 1974). We use the same log-linear specification for relative income since its variation it mostly due to the variation in income. The (natural) log-linear functional form is also useful for interpreting the coefficient estimates – in particular, a change in the level of relative deprivation is difficult to interpret, whereas a change in the (natural) log is easily interpreted as the approximate percentage change in relative deprivation. We only include a dummy for college degree rather than a full set of categorical education dummies because

migration assimilation effect on earnings. For example, it may be that migrants incur some downgrading immediately after migration due to imperfect transferability of skills (or simply the disruption of moving causes a loss of earnings), but over time this downgrading effect is eliminated through assimilation. Therefore, the total effect of migration on the outcome variable – and assuming no return – after YSM years-since-migration is: $\gamma_1 + \gamma_2 YSM + \gamma_3 YSM^2$. The immediate marginal effect of return migration is $\gamma_1 + \gamma_4$ if the individual returns to a state other than the state he grew-up, and $\gamma_1 + \gamma_4 + \gamma_5$ if the individual returns to the state he grew-up.

It is well-known that the unobserved individual fixed effect f_i (which includes innate ability and motivation) is correlated with the regressors. Therefore, we will use fixed effects estimation. Also, although we have placed the migration indicators $(M_{it}, R_{it}, RG_{it})$ on the right-hand-side of equation (13), this is not our premise for the direction of causality. On the contrary, later we will argue that causality runs the other way; that is, from income, relative income and relative deprivation to migration.[73] Again, we merely present correlations.

Before taking the logarithm, we need to do something with the zeros for income, relative income and relative deprivation. There is little lost in recoding relative deprivation from zero to one. Regarding income, one approach is to recode zeros to ones (and, hence, relative income is 1/mean). As expected, this gives a poor fit for both the income and relative income regression – intuitively, we expect migration to be associated with a smaller percent change in income for an employed person than an unemployed person. Therefore, based on goodness-of-fit measures we choose to drop all observations with income of 1,000 dollars or less.

we control for individual fixed effects.

[73]Indeed, the migration theories imply that the post-migration indicator fails the strict exogeneity assumption required for causal inference from the fixed effects estimates of (13). For example, under the absolute income hypothesis, past adverse shocks to individual income should make that individual more likely to migrate in the future, implying $E(\varepsilon_{it}|M_{i1}, ..., M_{iT_i}, f_i) \neq 0$, $t = 1, 2, ..., T_i$.

Table 5 displays the coefficient estimates from fixed effects estimation of equation (13) for when the sample is restricted to the first in-sample migration; that is, we drop those observations that occur after a second (in-sample) migration. Therefore, M_{it} is simply a post-migration dummy that takes the value one post-migration and zero otherwise. The first column shows the estimates when log absolute income is the dependent variable. The coefficient estimate on the post-migration dummy is positive and significant; more specifically, on average migration is associated with a rise in absolute income of about 8 percent.[74,75] Of course this result only applies to the self-selected group of migrants (later we will look at the outcomes of non-migrants as well as migrants). There is also tenuous evidence of an assimilation effect on earnings since the coefficient on years-since-migration is positive and significant at the five percent level. In restricting the sample to (in-sample) first-time migrants, it is impossible for an individual to return to a state she previously resided in unless she enters the sample in a state other than the state she grew-up and returns to that state. Therefore, since all returns are to the state one grew-up, R_{it} and RG_{it} are perfectly collinear and we drop R_{it}. The net effect of returning to the state the individual grew-up in is to reduce income by about 3 percent. The remaining estimates in the first column are as expected – income is an increasing and concave function of age. The coefficient estimate on the college degree dummy is positive but statistically insignificant, which is perhaps not unsurprising given we include fixed effects.

The second column of **Table 5** contains the estimates for the log of relative income as the outcome variable. The coefficient on the post-migration dummy is significant and implies that migration coincides with an increase in relative income of about 9.3 percent.[76]

[74] The exact percentage change in income from migration is equal to $100 * [\exp(\gamma_1) - 1]$ in the first year – assuming no return.

[75] The corresponding regression without sampling weights yields a coefficient estimate on the post-migration dummy of .063, which is statistically significant at the one percent level. We speculate that, since the unweighted data oversample the poor (from the SEO sample), it is high-income migrants who experience the largest percentage increase in their income post-migration.

[76] The corresponding unweighted regression yields a coefficient estimate on the post-migration dummy

TABLE 5

FIXED EFFECTS ESTIMATES FOR LOG INCOME, RELATIVE INCOME AND RELATIVE DEPRIVATION FOR FIRST-TIME MIGRANTS

	log Dependent variable:					
	Absolute income		Relative income		Rel. deprivation	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Post-migration dummy	0.077***	0.022	0.089***	0.022	–0.161***	0.046
YSM	0.010**	0.005	0.010**	0.005	–0.026***	0.010
YSM^2/100	–0.000	0.018	–0.007	0.018	0.062*	0.033
Returned-to-grewup	–0.109**	0.051	–0.098*	0.051	0.141*	0.082
Age	0.105***	0.009	0.097***	0.008	–0.129***	0.016
Age^2/100	–0.125***	0.010	–0.122***	0.010	0.172***	0.018
College degree	0.048	0.030	0.049	0.030	–0.234***	0.066
R-sq within	0.20		0.18		0.17	
Number of observations	16,996					
Number of groups	1,910					

Notes: Significance levels: * 10 percent, ** 5 percent, and *** 1 percent. Standard errors are robust and clustered at the individual level. The dependent variable is indicated by the column heading. The sample is those individuals who (in-sample) migrate interstate for the first time – that is, observations from second and higher migrations by the same individual are dropped. Further, only observations with income in excess of 1,000 dollars (in 1999 prices) are included. *Post-migration dummy* takes the value one after migration and zero before. *YSM* is years-since-migration. *Returned-to-grewup* is a dummy that takes the value one if the individual has returned to the state he grew-up and zero otherwise. *College degree* takes the value one if the individual has a college degree and zero otherwise. A full set of year dummies are included but not reported. The fixed effects estimation uses the individual longitudinal sampling weights supplied by the PSID.

A test of the null hypothesis that the coefficient estimates on the post-migration dummy from the absolute income and relative income regressions are equal is rejected at the three percent level.[77] Therefore, since the percentage rise in relative income is 1.3 percent larger than the percentage rise in absolute income, it must be that migrants tend to choose destination states with a lower mean income (about 1.3 percent lower) than their source states. This is suggestive – albeit tentative – that relative income as well as absolute income considerations may matter for migration choice. However, economically the 1.3 percent additional boost to relative income is small compared to the 8 percent rise in absolute income. Again there is evidence of a delayed effect coming through the coefficient estimate on years-since-migration, however this seems to be entirely driven by variation in absolute income. The net effect on relative income associated with returning to the state one grew-up is to reduce relative income by about one percent. Therefore, the fall in relative income from return migration is two percent less than the fall in absolute income from returning (in the first column). This difference is statistically significant at the five percent level, which supports the commonly-held view that return migration is more prevalent to areas of lower average income.

The third column of **Table 5** displays the estimates when the log of relative deprivation is the outcome variable. The coefficient estimate on the post-migration dummy indicates that migration is associated with an initial fall in relative deprivation of about 15 percent, which is statistically significant.[78] Also, the coefficient estimates on the quadratic in years-since-migration are significant and suggest that the relative deprivation of migrants further declines (but at a decreasing rate) as time passes. The effect is

of .066, which is significant at the one percent level.

[77]To test this in Stata, we first survey set the data to account for the PSID sampling weights and clustering at the individual level. Then in turn we group-demean all variables, run OLS separately – and store the results – for the group-demeaned absolute and relative income equations and, finally, calling the 'suest' command to test for equality in the cross-equation coefficients on the post-migration dummy.

[78]The estimate of γ_1 in the corresponding unweighted regression is -.11, which is significant at the one percent level.

large. For example, five years after migration relative deprivation in total falls by around 24 percent compared to its pre-migration value. Returning to the state the individual grew-up in is associated with a 2 percent fall in relative deprivation.

As an additional check for the absolute income hypothesis, we estimate equation (13) for when the dependent variable is the state price level. Recall that we have normalised our state price level variable such that in any given year the state average is one. We would like to know whether migrants choose destination states that have lower prices than their pre-migration (source) state. If true, it would represent additional evidence in support of the absolute income theory. The control vector x_{it} only includes a full set of year dummies for this regression. The fixed effects coefficient estimates (not reported) suggest that there is no evidence to support the claim that migrants choose a destination state with a lower price level than the source state. The coefficient estimate on the post-migration dummy is -.0023 with a standard error of .0049. There is no evidence of a delayed effect operating through the coefficient estimates on the quadratic in years-since-migration. There is, however, weak evidence that returning migrants face lower prices upon return – the coefficient estimate on the return migration dummy is negative and significant at the 8 percent level.

We now turn our attention to the full sample of migrants; that is, we include the individual-year observations from multiple migrations by the same individual – and do not simply use the first in-sample migration. **Table 6** displays the fixed effects estimates. Recall the explanatory variable of interest M_{it} (or *migration count*) in equation (13) is equal to the number of past in-sample migrations. Compared to the earlier estimates in **Table 5**, we notice a couple of differences. Most importantly, the coefficient estimate on M_{it} from the absolute income model (column one) and relative income model (column two) are not statistically different. That is, the rise in relative income associated with

TABLE 6

Fixed Effects Estimates for Log Income, Relative Income and Relative Deprivation for All Migrants

	log Dependent variable:					
	Absolute income		Relative income		Rel. deprivation	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Migration count	0.110***	0.016	0.109***	0.016	–0.188***	0.032
YSM	0.014***	0.004	0.014***	0.004	–0.025***	0.007
$YSM^2/100$	–0.007	0.015	–0.012	0.014	0.044	0.027
Returned count	–0.147***	0.033	–0.139***	0.034	0.270***	0.063
Returned-to-grewup count	–0.012	0.038	–0.016	0.038	–0.025	0.070
Age	0.108***	0.007	0.101***	0.006	–0.137***	0.012
$Age^2/100$	–0.130***	0.008	–0.128***	0.007	0.183***	0.014
College degree	0.022	0.024	0.018	0.024	–0.259***	0.053
R-sq within	0.22		0.19		0.17	
Number of observations	25,180					
Number of groups	1,954					

Notes: Significance levels: * 10 percent, ** 5 percent, and *** 1 percent. Standard errors are robust and clustered at the individual level. The dependent variable is indicated by the column heading. The sample is all individual-year observations of in-sample interstate migrants. Further, only observations with income in excess of 1,000 dollars (in 1999 prices) are included. *Migration count* is the cumulative sum of (in-sample) migrations for an individual. *Returned count* is the cumulative sum of return migrations to a state the individual has previously resided in. *YSM* is years-since-migration. *Returned-to-grewup count* is the cumulative sum of return migrations to the state the individual grew-up. *College degree* takes the value one if the individual has a college degree and zero otherwise. A full set of year dummies are included but not reported. The fixed effects estimation uses the individual longitudinal sampling weights supplied by the PSID.

migration is entirely due to the rise in absolute income. We also note that the magnitude of this coefficient is greater than that in **Table 5**. This suggests there is no evidence of decreasing returns to multiple migrations by the same individual. This is useful to know because, if there was decreasing returns to migration then one may have argued that the first (in-sample) migration has a special status over any other subsequent migration.

A possible explanation for why we find no evidence of a greater improvement in relative income in the full sample of migrations is the following. From **Table 5** we saw that returning migrants tend to return to states with a lower average income. Given the substantial persistence in state average income, it is likely that returning migrants initially left a low income source for a high income host (and possibly did not change their reference group). Therefore, the initial migration observations of eventual return migrants may contaminate the coefficient estimate on the *migration count* variable in **Table 6**.

In summary, we have found tentative evidence that is consistent with all three theories (absolute income, relative income and relative deprivation) as well as both reference and no reference substitution. However, the evidence that relative income increases by more than absolute income around the time of migration is at best statistically weak and any pure relative effect is small economically. Nonetheless, this does not imply that the relative income hypothesis fails, it can still hold under the assumption of no reference substitution. However, the analysis so far is rather unsatisfactory for the following reasons. First, we want to say something about causality rather than mere correlations; more specifically, we want to measure the causal effect of each income-based well-being measure on migration propensity. Second, we want to estimate the relative importance (or the partial effects) of the three theories for migration choice, which requires controlling for all three stories simultaneously. Indeed, in the above analysis it is unclear whether the improvement in relative deprivation around the time of migration is solely due to the increase in absolute income or, whether it is due to a change in the reference income distribution. Third, the fact that stayers – by definition – choose not to migrate is useful information that we want to exploit.

4.3.2 On Estimating Counterfactual Migrant Earnings

The migration theories dictate – by definition – that causality runs from either income, relative income or relative deprivation to migration, and not the other way around – that is, not from migration to income, relative income or relative deprivation. In deciding whether to migrate, individuals compare their expected well-being from moving with that from staying. Clearly we need to account for the opportunities that exist in the potential destination states. For example, it may be that those on higher incomes in the source have even better opportunities available in the destination states. If so, then failure to account for this will bias the effect of income on migration upwards. Therefore, a necessary first step in estimating migration propensity is to estimate expected income conditional on migration, which is the objective of this subsection.

Naturally, we only observe migrant earnings for those individuals who migrate and, albeit, after migration has taken place. The (counterfactual) migrant earnings of non-migrants must be estimated. To predict the migrant earnings of non-migrants from a particular source state, we will use the observed earnings of actual migrants from that source state. In this way, we are assuming that it is migrants from the same source – rather than natives in the destination – that are the best yardstick for what non-migrants could have earned if counterfactually they had migrated.[79] Recall that we defined the source state as the state of residence pre-migration. Therefore, for migrants, their source

[79] The empirical evidence that migrants and natives are imperfect substitutes (the so-called downgrading of migrants) supports our approach (see, for example, Ottaviano and Peri (2005, 2006, 2007) for evidence of imperfect substitutability between international immigrants and natives in the U.S.).

state gets updated such that, when considering a second migration, their source is the destination of their last migration. In other words, when considering migration, the source is always the current state of residence.

The numbers of in-sample migrants from any one source state are far too small to disaggregate them by the 50 potential destination states; hence, we simply combine all migrants from the same source. Therefore, our focus is on explaining the decision whether to migrate and, not the joint decision of whether to migrate and which destination to choose. The estimation proceeds in two stages: (1) in this subsection we consistently estimate counterfactual income for non-migrants and, (2) in the next subsection we use these counterfactual income estimates as an additional explanatory variable in a probit/logit model for the probability of migration.[80]

The first stage is to predict counterfactual earnings of non-migrants using the earnings of migrants from the same source. To be clear, we estimate (or predict) *contemporaneous* migrant earnings for every individual-year observation in our sample. The reason is because, even once an individual has migrated, he or she can of course migrate again, and we want to estimate the income he or she would get if they were to do so from the updated source. The point is that migration and non-migration are mutually exclusive events; therefore, even when we observe migrant earnings for an individual, it is necessarily at a different time to any period in which he or she chose not to migrate.

We assume that the migrant log earnings of individual i at time t is given by the

[80]Since we lump all destination states together, we cannot estimate migrant relative income and migrant relative deprivation for non-migrants because these measures are inherently destination-specific. An alternative procedure would be to estimate migrant earnings of non-migrants using the observed earnings of those (natives) in each potential destination. Then one could estimate destination-specific migrant relative income and migrant relative deprivation using the observed destination-specific income distribution. The dependent variable of the second stage estimation will then be the location choice among 51 states (where non-migrants choose their current state), which could be estimated using the conditional logit for example. Among other things, one would want to control for the distances between destinations which is well-known to be a substantial deterrent to migration. This is beyond the scope of this paper but represents a promising area for future research.

following linear form

$$\log y_{it}^m = x_{it}'\beta^m + f_i^m + \xi_{it}^m; \quad i = 1, ..., N; \quad t = 1, ..., T_i \tag{14}$$

where the superscript m indicates conditionality on migration from the source; y_{it}^m is income; x_{it} is a vector of observable explanatory variables; β^m is the parameter vector of interest; f_i^m is an unobserved individual fixed effect; and ξ_{it}^m is an unobserved idiosyncratic error with $E(\xi_{it}^m) = 0$. Let $M_{it} \in \{0, 1\}$ be a post-migration indicator that takes the value one if individual i migrated prior to time t and zero otherwise. We observe y_{it}^m if $M_{it} = 1$ and not otherwise. Estimation of equation (14) is carried out separately for each source state (all 50 U.S. states plus the District of Columbia) using the subsample of migrants from that source. The question is, under what conditions will our estimate of β^m be consistent (for the whole source population) when we condition on $M_{it} = 1$?

A sufficient condition for consistency of pooled OLS (or random effects) on (14) is the conditional mean-independence of the unobserved term: $E(f_i^m + \xi_{it}^m | x_{it}, M_{it}) = E(f_i^m)$; for all i, t. This may not hold for one or more of the following reasons: (1) correlation between the individual fixed effect f_i^m and x_{it}; (2) correlation between ξ_{it}^m and x_{it}; (3) correlation between the post-migration (or selection) indicator M_{it} and f_i^m; and (4) correlation between M_{it} and ξ_{it}^m. Points (1) and (2) result in bias due to endogeneity, whereas (3) and (4) result in bias due to selection.[81] It is well-known that f_i^m is correlated with x_{it} (that is, point (1) is true). For example, unobserved innate ability has a direct effect on earnings (and hence contained in f_i^m) and is correlated with education (contained in x_{it}).

Semykina and Wooldridge (2010) derive an expression for the conditional expectation

[81] Of all these potential biases, only (2) and (4) can be corrected for in cross-sectional data (following Heckman's procedure and a set of instruments $\tilde{x}_{it}$ such that $E[x_{it}|\tilde{x}_{it}] \neq 0$ and $E[\xi_{it}^m|\tilde{x}_{it}] = 0$).

TABLE 7

Two Stage Least Squares Estimates from First Stage

	AL	AZ	AR	CA	CO	CT	DE	DC	FL
Experience	.12***	.17	.11***	.045***	.033	.038	.15**	.074**	.049*
Experience-sq	–.0023**	–.0027	–.0027***	–.0012***	–.0013	–.0016	–.0033	–.0017**	–.00098
Degree	.24	–1.8	.95	.15	–2	.77	1.2***	–1	.25
Lambda	–17848206	.28	–.16	–.04	.063	–.16	0	.55	.042
Age bar	–.04	.18	–.12*	–.026	.21	.19	–.24	.051	–.12
Age-sq bar	.00073	–.0029	.0017**	.00048	–.0021	–.002	.0024	–.00033	.0014
Married bar	1.1*	1	.44*	.95***	1.2**	.53	1.9***	.66**	.42*
State price bar	3.3	1.6	–.45	1.1	.73	1.4	0	.21	2.6***
No. of obs	244	261	427	1,917	454	192	42	548	737
	GA	ID	IL	IN	IA	KS	KY	LA	ME
Experience	.076***	.017	.059	.12	.077**	.086	.059	.17**	.0066
Experience-sq	–.0012**	–.00026	–.0013*	–.0042	–.0024***	–.0028	–.0025**	–.0039**	–.0013
Degree	.89	–1.6	–3.3	8.1	–1.1*	3.3	–2.8	–2.6	–.011
Lambda	.072	0	–.28	–.088	.047	.54	–1.2*	.36	–158240238*
Age bar	.0083	–1.3	.37	–.57	–.17	–.48	.14	–.025	–.049
Age-sq bar	–.00031	.02	–.0042	.0073	.0025*	.0066	–.00096	.00049	.00089
Married bar	.71***	.51	1.3*	–.79	.78***	1.6	.29	.93*	–.91
State price bar	2.1**	–15	4.1	–9.7	1.1	–2.1	–2	5.3	–9.4***
No. of obs	472	55	1,115	680	385	238	262	393	105
	MD	MA	MI	MN	MS	MO	MT	NE	NV
Experience	.046**	.06**	.12*	.067	.11***	.054	.29***	.13	–.074
Experience-sq	–.0009**	–.0019***	–.0016*	–.0013	–.0014*	–.00076	–.0019***	–.0019	.00061
Degree	.2	–1.1	–2.8	.64	.7	–1.3	0	3.9	.25
Lambda	.017	–.0034	–.087	.22*	.043	–.15	.64**	–21644098	–3139809***
Age bar	.078	.1	–.0056	.16	.068	.12	–.78**	–.6	.17
Age-sq bar	–.0009	–.0014	–.00054	–.0019	–.0012*	–.0013	.0062**	.0067	–.0016
Married bar	.84***	.66	.82	.49	.56*	1.1***	–.54**	–.15	.98
State price bar	.024	3.1	1.5	2.7**	–1.6	1.3	0	–3.9	.12
No. of obs	665	375	582	347	365	771	35	266	167
	NH	NJ	NM	NY	NC	ND	OH	OK	OR
Experience	–.023	–.0026	.026	.071***	.086	14	.032	.045	–.013
Experience-sq	–.0015	.00021	–.0033	–.0015**	–.0017	–.21	–.0017***	–.00069	.0011
Degree	1.7***	–.32	–1.8	.24	4.1	11	–3.5	–4.5*	–.69
Lambda	0	.055	.4	–.1	–.048	0	–.34	327803896	–.23
Age bar	.13	.11	–.19	.027	–.023	15	.14	.73	.28
Age-sq bar	–.00092	–.0013	.0039	–.00031	.00024	–.27	–.0013	–.0087	–.004
Married bar	1.3	.88***	.82	.66**	.82	0	1.5	4.1**	.86***
State price bar	–4.9	2.9**	.39	2.3*	2.5	0	–.95	–.95	–.087
No. of obs	84	605	126	1,088	608	23	1,040	205	175
	PA	RI	SC	SD	TN	TX	UT	VT	VA
Experience	.069***	–.32***	–.00026	.14	.15***	.068**	.13*	0	.057*
Experience-sq	–.0016***	.0063***	.0005	–.0023*	–.0033***	–.0014***	–.0017	.15	–.0022***
Degree	–1.1	.62**	5.3	.41	–.82	–.6	–.15	0	–2.3
Lambda	.048	0	–.43	0	.29	–.061	.0022	–.2	–.26
Age bar	.15*	1.6***	.087	.0046	–.072	.046	–.091	0	.049
Age-sq bar	–.0017*	–.019***	–.00092	–.00055	.00076	–.00053	.0003	.074	–.00031
Married bar	.35*	–2.3***	.25	–.2	.68	.68***	1.2***	0	1**
State price bar	1.9*	–2.1*	.5	.74	–1.3	1.7**	2.8	0	.41
No. of obs	636	53	376	126	407	1,267	212	4	825
	WA	WV	WI	WY	AK	HI			
Experience	.12**	–.075***	.077	.25	.25*	.2			
Experience-sq	–.0026*	.00073	–.0017	–.0022	–.0046*	–.0031			
Degree	.36	2.8	2.1*	0	–.9	–1.7			
Lambda	–.051	0	14112328	0	36173494	0			
Age bar	–.083	.099***	–.04	0	–.3	.28			
Age-sq bar	.0011	–.0025	.00038	–.002	.0034	–.0042			
Married bar	.62***	1.9***	.61*	–.43	.39	1.3			
State price bar	.65	–56***	–.53	0	–.67	–9.9			
No. of obs	288	35	221	17	166	91			

Notes: Significance levels: * $p < .1$, ** $p < .05$, and *** $p < .01$. Standard errors (not reported) are bootstrapped.

$E(f_i^m + \xi_{it}^m | x_{it}, M_{it})$ (that is, the bias) and then include this as an additional explanatory variable in equation (14) to correct for the bias.[82] The selection process is assumed to be

$$M_{it}^{\star} = z_{it}'\gamma_t + a_i + u_{it}; \qquad M_{it} = 1[M_{it}^{\star} > 0]; \tag{15}$$

where $M_{it}^{\star}$ is the latent propensity to migrate; z_{it} is a vector of instruments that both explain selection and are strictly exogenous to the unobserved idiosyncratic disturbance in the income equation: $E(\xi_{it}^m | z_{i1}, ..., z_{iT_i}, f_i) = 0$; a_i is a fixed effect and, u_{it} is an unobserved idiosyncratic disturbance. Then, under some fairly weak assumptions[83], Semykina and Wooldridge (2010) show that (using only the subsample of migrant earnings) consistent estimates of β^m result from running pooled Two Stage Least Squares (2SLS) on

$$\log y_{it}^m = x_{it}'\beta^m + \overline{z}_i'b + g\hat{\lambda}_{it} + \text{error}_{it}; \quad i = 1, ..., N; \quad t = 1, ..., T_i; \tag{16}$$

where $\hat{\lambda}_{it}$ is the Inverse Mills Ratio from a probit estimation – for each t – on equation (15); and $\overline{z}_i \equiv T_i^{-1} \sum_t z_{it}$ is the within-individual time mean of the regressors in the selection equation. It only remains for us to specify x_{it} and z_{it}. The vector x_{it} consists of experience, experience squared, a college degree dummy, unemployment status, state-level average income, price level, unemployment rate and a full set of year dummies. In z_{it} we include age, age squared, marital status, number of children, a full set of year dummies, and state-level variables for average income, the price level, unemployment rate, climatic conditions, number of bordering states and land area. Our choice of variables was chosen to meet two criteria: (1) z_{it} includes all those variables in x_{it} that are strictly exogenous to the idiosyncratic error in the income equation and, (2) z_{it} needs to be of strictly higher

[82] Semykina and Wooldridge (2010) extends Wooldridge (1995) to correct for correlation between the idiosyncratic error ξ_{it}^m and the regressors x_i. See Dustmann and Rochina-Barrachina (2007) for a survey of correction procedures for panel data estimation in the presence of unobserved fixed effects and selection.

[83] The three key assumptions are: (1) Mundlak's (1978) specification that $f_i^m = \overline{z}_i'b + c_i$ and $a_i = \overline{z}_i'd + v_i$ where $\overline{z}_i \equiv T_i^{-1} \sum_t z_{it}$; (2) u_{it} in the selection equation is Normally distributed; and (3) a valid set of instruments z_{it} exist.

rank than x_{it}.[84] In particular, we feel that the college degree dummy and unemployment status are unlikely to be strictly exogenous and therefore these are omitted from z_{it}.

The coefficient estimates from pooled 2SLS on equation (16) are displayed in **Table 7**. Each column presents the estimates for a particular source state (the column headings are the USPS state abbreviations). A number of the coefficient estimates on the within-individual time means of the instruments are statistically significant, implying evidence of fixed effects. There is little evidence for (contemporaneous) selection on the unobserved idiosyncratic error since the coefficient estimate on $\hat{\lambda}_{it}$ is mostly insignificant.

4.3.3 On the Propensity to Migrate

In this subsection we jointly estimate the effects of individual income, relative income and relative deprivation on the individual propensity to migrate from the source state (where the source state is defined as the state the individual resides in prior to migration). We will control for the predicted counterfactual migrant income that we estimated in the previous section. The dependent variable is the end-of-year binary migration decision.

It is important to remember that individual income enters directly into the calculation for relative income and relative deprivation. Therefore, we would expect the three measures to be correlated. To identify their separate effects on migration they must, of course, be imperfectly correlated and the lower the correlation the higher the precision of the estimates. **Table 8** presents the pairwise correlation coefficients between the three measures (as well as the state price level, state mean income and the state unemployment rate) in our pooled sample. First, we see that individual income and relative income are almost perfectly correlated, this is despite variation in the reference both across individuals (individuals living in different states) and within-individuals across-time (from

[84]The higher the difference in rank the better to reduce collinearity between the selection bias term and x_{it} in equation (16).

migration). This implies that there is little variation in state mean income both across states and time and, what little variation exists is dwarfed by the variation in individual income. This collinearity will make it difficult to identify the separate effects from individual and relative income. Therefore, in the upcoming regressions we will control for individual income and state average income – the effect of relative income can be inferred from these two components. Hereafter we use 'average income' to refer to mean income in an individual's state of residence. Second, and this is important, from **Table 8** we see that relative deprivation is far from perfectly correlated with individual income. The negative sign of the correlation coefficient is to be expected since higher income lowers relative deprivation, holding the reference income distribution constant. It is this moderate (rather than strong) correlation that will allow us to distinguish the relative deprivation motive from absolute and relative income motives. As expected, the aggregate (state-level) variables – the price level, average income and the unemployment rate – are weakly correlated with the individual-level variables (individual income, relative income and relative deprivation). Among the state-level variables, the price level is positively correlated with average income, but it is far from perfectly correlated.

TABLE 8

PAIRWISE CORRELATIONS

	Individual income	*Relative income*	*Relative deprivation*	*Price level*	*Average income*	*Unemployment rate*
Individual income	1					
Relative income	0.979***	1				
Relative deprivation	–0.543***	–0.599***	1			
Price level	0.108***	0.045***	0.057***	1		
Average income	0.122***	–0.012*	0.260***	0.489***	1	
Unemployment rate	–0.001	0.043***	–0.118***	0.138***	–0.326***	1

Notes: Significance levels: * 5 percent, ** 1 percent, and *** 0.1 percent. The table displays the pairwise correlation coefficients for selected variables in our dataset. The sample is all PSID individual-year observations of household heads that are in the labour force, non-institutionalised and of working age. The PSID sampling weights are applied.

The structural model for the propensity to migrate is assumed to be

$$m_{it}^{*} = \psi_1 \log y_{it} + \psi_2 \log y_{it}^{m} + \psi_3 \log Y_{it} + \psi_4 \log RD_{it} + \theta' z_{it} + \alpha_i + \nu_{it};$$
$$m_{it} = 1[m_{it}^{\star} > 0]; \quad i = 1, ..., N; \quad t = 1, ..., T_i; \tag{17}$$

where m_{it}^{*} is the latent propensity to migrate for individual i at the end-of-year t; y_{it} is individual income in year t; y_{it}^{m} is counterfactual migrant income in year t; Y_{it} is average income in individual i's state of residence in year t; RD_{it} is relative deprivation; z_{it} is a vector of controls; α_i is an unobserved individual fixed effect; ν_{it} is an independent disturbance; m_{it} is the observed binary migration decision that takes the value one if individual i migrates at the end-of-year t and zero otherwise; and $1[.]$ is the indicator function.[85] The control vector z_{it} includes personal characteristics, state-level variables and a full set of year dummies. The personal characteristics we control for are age, age squared, a dummy for college degree, marital status, number of children, and whether the individual is unemployed or not at the time of the survey. The state-level variables we control for are the price level, the unemployment rate, climatic conditions, number of bordering states and land area. The parameters of primary interest are $\{\psi_1, \psi_3, \psi_4\}$, which represent the *causal effect* of individual income, average income and relative deprivation on migration propensity, respectively. The question is, under what conditions can we consistently estimate these parameters?

Causal inference relies on the regressors being exogenous; that is, statistically inde-

[85] We enter individual income in logs since we expect income to have a multiplicative effect on migration propensity; that is, a *percentage* change (rather than a *level* change) in income is likely to have a similarly-sized effect on migration propensity, irrespective of the income level. In other words, we would expect a 1,000 dollar increase in income to have a bigger effect on the migration decision of a low-income person than a high-income person. We enter average state income in logs so that we can infer the effect of relative income by a simple comparison of the marginal effects of individual income and average income. We choose to enter relative deprivation in logs since, although relative deprivation is far less skewed than income, taking its log helps with interpretation and, if we were to enter it in levels then it may be seen as capturing a level-effect on migration propensity due to income.

pendent of the error term $\alpha_i + \nu_{it}$ (or *as if* the regressors were randomly assigned to people). The error term represents all omitted variables that determine migration choice. For the (non-linear) panel random and fixed effects models that we will estimate, the parameter estimates are consistent only under the assumption that the regressors are strictly exogenous (see Wooldridge (2002)). Strict exogeneity requires the regressors to be uncorrelated with past, current and future values of the error term. The problem is that income (and hence relative income and relative deprivation) is highly likely to be endogenous – that is, there is feedback from migration choice to future income (and, hence, from the error term to future income). Indeed, migrants would surely hope that migration has a positive effect on future income and the decision to migrate is in large part in anticipation that migration will lead to higher income. There may be other reasons why migration choice will affect income. Endogeneity is therefore a problem that arises from not being able to observe all the factors that determine migration choice. If we could control for all those variables that influence migration – including expected income conditional on migration – then there would be no endogeneity concern because these variables will not be omitted from the model. Indeed, the estimates are consistent under arbitrary dependence among the regressors.

We take a number of steps towards consistent estimation. First, the regressors are determined prior to the end-of-year migration decision; hence the regressors are predetermined – that is, the unobserved disturbance is uncorrelated with current and past values of the regressors, but may still be correlated with future values.[86] Second, we control for individual fixed effects, time effects and a large vector of observable time-varying variables that have been suggested to influence migration. Therefore, our estimates are consistent

[86]For example, if we observe that individual i resides in California in 1989, New York in 1990 and New York in 1991, then $m_{i1989} = 1$ and $m_{i1990} = 0$. Personal characteristics (for example, college degree, marital status, unemployment status) refer to their values at the time of the PSID survey within year t, whilst income refers to earnings in year t. There is no way that the individual can retrospectively change the values of the regressors in response to their migration choice.

under arbitrary dependence between the regressors and any unobserved time-invariant individual heterogeneity. The remaining source of endogeneity bias is if the regressors are correlated with past values of the unobserved idiosyncratic (time-varying) disturbance. We have already suggested that a 'shock' to migration choice is likely to affect future income. Third and to reduce these 'shocks', we control for predicted counterfactual migrant income that we estimated in the previous section.[87] Finally we present a series of robustness checks, including Wooldridge's (1997) estimator for the consistent estimation of non-linear fixed effects panel data models without strict exogeneity.

Table 9 displays estimates for the average partial effects from a probit model of equation (17), without controlling for relative deprivation (RD_{it}). The average partial effect for a regressor tells us the change in the probability of migration for a one unit change in that regressor – for the typical individual in our sample.[88] Adjacent to the point estimate for the partial effect, the table reports the corresponding standard error, which are bootstrapped, robust and clustered at the individual level.[89]

The first column of **Table 9** controls for individual income, estimated migrant income, average income and personal circumstances – whether unemployed, age, age squared, whether college degree, whether married and number of children. The partial effects are random effects estimates, that is, they assume the individual unobserved effect α_i is uncorrelated with the regressors. The estimated partial effect for individual income is insignificant (and has the wrong sign). A priori, we would expect the partial effect of individual income to be negative – under both the absolute and relative income hypotheses

[87]Clearly we would not want to control for actual future income post-migration (which we observe for migrants) because then it would be unclear what – if any – economic meaning we could derive from this – the individual can never know for sure what his future income will be, individuals may make 'mistakes' when estimating their post-migration earnings.

[88]In computing the average partial effects, we set the individual-specific intercepts to zero – which is the mean of the random effects.

[89]Bootstrapping should adjust the standard errors for the fact that counterfactual migrant earnings are estimated.

and holding average income constant, an increase in absolute income (and, hence, relative income too) reduces migration propensity. The effect of (counterfactual) migrant income is also insignificant. In contrast, average income is positive and significant – a one percent rise in average income increases the probability of migration by 1.2 percent, holding individual income constant. We also see that those who are unemployed at the time of the survey are more likely to migrate; more specifically, an unemployed person is .55 percent more likely to migrate than an employed person. The estimates on the remaining controls are as expected. The probability of migration is higher for people that are younger, with a college degree and fewer children. The partial effect of being married is not statistically different from zero. The estimated partial effect for age accounts for a quadratic in age.

We argued in section 3 that the random effects assumption – which is implicit in the cross-sectional studies that dominate the related literature – is unlikely to hold. The estimates in the second column of **Table 9** display estimates of the average partial effects from a probit fixed effects estimation using Mundlak's (1978) correction procedure (see Mundlak (1978) and Wooldridge (2002)).[90] Mundlak (1978) assumes that the individual fixed effect, α_i, can be written as a linear function of the within-individual time mean of the regressors and a disturbance term that is uncorrelated with the regressors. That is, $\alpha_i = a_1 \log y_i + a_2 \log y_i^m + a_3 \log Y_i + a_4' z_i + c_i$ where the subscript i for the regressor indicates the time mean for individual i, for example $z_{1i} \equiv T_i^{-1} \sum_{t=1}^{T_i} z_{1it}$, and c_i is a disturbance that is uncorrelated with the regressors $(\log y_{it}, \log y_i^m, \log Y_{it}, z_{it})$. To estimate the model we simply run random effects estimation on the transformed model that appends the within-individual time mean of the regressors to the set of regressors.[91]

[90]The standard fixed effects methods for linear regression models (either differencing or group-demeaning) will not eliminate the fixed effect α_i for the probit model. Further, directly estimating the group-specific intercepts using Maximum Likelihood estimation leads to inconsistent estimates of the group intercepts – and consequently the slope coefficients too – since T_i is fixed and small (this is the 'incidental parameters problem', see Greene (2008)).

[91]In theory, z_i should include the individual time means of the year dummies since, for our unbalanced

TABLE 9

Average Partial Effects for Migration Propensity

Dependent variable: *Migration dummy*	(1) Random effects		(2) Fixed effects		(3) Compensating aggregates	
	A.P.E.	S.E.	A.P.E.	S.E.	A.P.E.	S.E.
log *Individual income*	1.4e-4	1.3e-4	–3.4e-4**	1.5e-4	–3.0e-4*	1.6e-4
log *Migrant income*	–1.6e-4	1.4e-4	–2.8e-4	1.7e-4	–6.8e-4***	2.1e-4
log *Average income*	1.2e-2***	2.6e-3	1.6e-2***	3.7e-3	6.8e-3	5.1e-3
Unemployed (d)	5.5e-3***	1.0e-3	5.9e-3***	1.1e-3	5.2e-3***	1.2e-3
Age	–9.7e-4***	6.2e-5	–1.0e-3***	8.7e-5	–7.8e-4***	9.5e-5
College degree (d)	8.5e-3***	9.2e-4	2.4e-3**	1.2e-3	2.2e-3*	1.3e-3
Married (d)	–4.8e-4	6.6e-4	–2.7e-3***	9.6e-4	–2.7e-3***	1.0e-3
Children	–2.5e-3***	2.9e-4	–1.5e-3***	3.5e-4	–1.4e-3***	3.6e-4
Price level					3.3e-2***	7.6e-3
Unemployment rate					5.3e-4**	2.6e-4
Temperature, ave.					1.7e-3***	3.9e-4
Temp, max-min					2.7e-4	2.7e-4
Pecipitation, ave.					–4.5e-3***	1.4e-3
Precip, max-min					–1.2e-3	8.2e-4
Heating deg. days					3.6e-6*	1.9e-6
Cooling deg. days					–1.2e-6	3.0e-6
Borders					6.6e-4	4.4e-4
Land area					–2.9e-8***	7.4e-9
Fixed effects	NO		YES		YES	
LogL	–14,089		–14,036		–13,845	
Number of obs	117,192		117,192		117,019	
Number of groups	13,862		13,862		13,851	

Notes: Significance levels: * 10 percent, ** 5 percent, and *** 1 percent. The table displays estimated average partial effects (A.P.E.) from a probit model for the probability of migration. Standard errors (S.E.) are bootstrapped, robust and clustered at the individual level. The dependent variable is a dummy variable that takes the value one if the individual migrates interstate at the end of the year, and zero otherwise. The suffix (d) denotes a discrete change in a dummy variable from zero to one. The reported partial effects for *age*, *temperature average* and *precipitation average* account for a quadratic in these variables. The models in columns two and three control for the within-individual time averages (or fixed effects) of the regressors – following Mundlak (1978) – although these are not reported. A full set of year dummies are also included but not reported.

The fixed effects estimates in column two of **Table 9** are very different from the

panel, the time means of the year dummies vary across individuals. However, doing so leads to convergence problems of the maximum likelihood solver; hence, the regression only includes a full set of year dummies and not their individual time means.

random effects estimates in column one. The partial effect on income is now negative and significant, which conforms to prior expectations. A one percent increase in individual income decreases the probability of migration by .034 percent, which is small. The partial effect of average income is positive and even higher than the estimate in column one – a one percent increase in average income increases the probability of migration by 1.6 percent, holding individual income constant. Migrant earnings remains insignificant. The partial effect of a college degree in column two is lower than that in column one, which is not surprising given that a college degree is likely correlated with unobserved innate ability. The partial effect of marriage is now negative and significant.

If the random effects assumption that the fixed effect α_i is uncorrelated with the regressors is correct, then the coefficient estimates on the within-individual time means of the regressors should not be significantly different from zero. The vast majority of the individual time means of the regressors (not reported) are individually significantly different from zero. Moreover, a likelihood ratio test between the nested models in columns one and two overwhelmingly rejects the null hypothesis that the coefficients on the within-individual time means are jointly insignificant. The likelihood ratio test statistic of 104 is far larger than the critical value at the one percent level, 21.67.[92] This suggests the fixed effects model is the appropriate one. Therefore, this is evidence in support of our earlier critique of cross-sectional studies; that is, unobservables (innate ability, motivation, willingness to move) play an important role in determining migration propensity and are correlated with the regressors.

There is at least one obvious concern with the estimates in column two. If the absolute income hypothesis has at least some relevance, then the (negative) partial effect of individual income should be greater in absolute value than the (positive) partial ef-

[92]The critical value of 21.67 is from the chi-squared distribution with 9 degrees of freedom. Recall that *age* is entered as a quadratic so there are 9 within-individual time means.

fect of average income – and if the pure relative income hypothesis is correct then the partial effects on income and average income should be equal in absolute value, which is overwhelmingly rejected. Clearly we need to control for confounding state-level variables.

The estimates in column three of **Table 9** control for various state-level variables. These are the state price level, unemployment rate, climatic conditions (including a quadratic in average temperature and precipitation), number of bordering states (a proxy for distance) and land area. Importantly, these state-level controls render the partial effect of average income on migration choice insignificant. The partial effect on individual income remains negative and significant. Estimated migrant income is now significant but negative, which is the opposite of what we would expect. The state price level has a positive and statistically significant effect on the probability of migration. Recall that the price level is normalised such that the average across states is one. Therefore, residing in a state with a price level one percent higher than the average increases the probability of migration by .033 percent compared to residing in a state with the average price level. Therefore, the partial effect of the price level is comparable in magnitude to that of individual income, which is what we would expect. Nonetheless, the effect is small. Considering the remaining covariates, we see that the unemployment rate in the state of residence has a positive effect on migration propensity. For the typical person a rise in average temperature increases the probability of migration, whilst an increase in precipitation lowers migration propensity. Heating degree days – an indicator of the demand for heating – has a positive effect on the probability of migration. The number of bordering states does not have a significant effect; state land area reduces the probability of migration.

Table 10 contains the estimated average partial effects when we control for relative deprivation. Holding individual income and average income constant, it is possible

TABLE 10

Average Partial Effects for Migration Propensity:
Controlling for Relative Deprivation

Dependent variable: *Migration dummy*	(1) Relative deprivation		(2) RD + Average income		(3) Compensating aggregates	
	A.P.E.	S.E.	A.P.E.	S.E.	A.P.E.	S.E.
log *Individual income*	–5.8e-5	1.6e-4	–1.0e-4	1.6e-4	–5.5e-5	1.7e-4
log *Migrant income*	–2.7e-4	1.7e-4	–2.8e-4	1.8e-4	–6.8e-4***	2.1e-4
log *Average income*			1.3e-2***	3.8e-3	3.8e-3	5.2e-3
log *Relative deprivation*	1.8e-3***	4.0e-4	1.5e-3***	4.0e-4	1.6e-3***	4.2e-4
Unemployed (d)	5.8e-3***	1.1e-3	5.8e-3***	1.2e-3	5.1e-3***	1.2e-3
Age	–9.6e-4***	8.1e-5	–9.9e-4***	8.8e-5	–7.7e-4***	9.6e-5
College degree (d)	3.0e-3**	1.2e-3	2.9e-3**	1.2e-3	2.6e-3**	1.3e-3
Married (d)	–2.5e-3***	9.7e-4	–2.5e-3***	9.7e-4	–2.6e-3**	1.0e-3
Children	–1.4e-3***	3.5e-4	–1.4e-3***	3.6e-4	–1.4e-3***	3.7e-4
Price level					3.4e-2***	7.6e-3
Unemployment rate					5.0e-4*	2.6e-4
Temperature, ave.					1.7e-3***	3.9e-4
Temp, max-min					2.8e-4	2.7e-4
Pecipitation, ave.					–4.4e-3***	1.4e-3
Precip, max-min					–1.2e-3	8.2e-4
Heating deg. days					3.4e-6*	1.9e-6
Cooling deg. days					–1.3e-6	3.0e-6
Borders					6.3e-4	4.4e-4
Land area					–2.9e-8***	7.4e-9
Fixed effects	YES		YES		YES	
LogL	–14,031		–14,021		–13,831	
Number of obs	117,192		117,192		117,019	
Number of groups	13,862		13,862		13,851	

Notes: Significance levels: * 10 percent, ** 5 percent, and *** 1 percent. The table displays average partial effects (A.P.E.) from a Mundlak (1978) fixed effects probit model. Standard errors (S.E.) are bootstrapped, robust and clustered at the individual level. The dependent variable is a dummy variable that takes the value one if the individual migrates interstate at the end of the year, and zero otherwise. The suffix (d) denotes a discrete change in a dummy variable from zero to one. The reported partial effects for *age*, *temperature average* and *precipitation average* account for a quadratic in these variables. All models include the individual-specific time averages for each regressor as well as a full set of year dummies, but these are not reported.

to change relative deprivation. In section 3.2 we saw that a mean-preserving spread of the income distribution can achieve this. Consider the estimates in column one, which does not control for average income. We see that relative deprivation increases migration propensity – a one percent increase in relative deprivation increases the probability of migration by about .18 percent. This is economically significant given that only three percent of our observations are when an individual migrates. The partial effects of individual income and estimated migrant income are statistically insignificant.

The second column controls for relative deprivation and average income. Both have a positive and significant effect on migration propensity, as predicted by the relative income and relative deprivation hypotheses, respectively. The coefficients on individual income and migrant income remain insignificant. In the final column we control for the various state-level variables. As before, this renders the effect of average income insignificant. However, the effect of relative deprivation is still positive and highly significant.

4.3.4 Robustness checks

Table 11 displays the results from various robustness checks. The columns contain the average partial effects and corresponding standard errors for the two regressors of primary interest: individual income and relative deprivation. The baseline model is that in the third column of table 10 and – for ease of comparison – we reproduce these estimates in the first row of table 11. The remaining rows indicate a variant of the baseline model. Unless specified otherwise, all estimations include the same controls as the baseline model.

The estimations in rows (2)–(4) add extra controls to the baseline model. As in the baseline model, the figures are probit fixed-effects estimates using Mundlak's (1978) specification for the unobserved fixed effect. The second row controls for whether the individual owns his or her own home. One may think that home ownership has a direct

TABLE 11

Robustness Checks

Dependent variable: *Migration dummy*		Individual income		Relative deprivation			
	Specification:	A.P.E.	S.E.	A.P.E.	S.E.	LogL	N
(1)	Baseline	–5.5e-5	1.7e-4	1.6e-3***	4.2e-4	–13,831	117,019
Variable addition:							
(2)	Control for whether own home	–7.6e-5	1.8e-4	1.1e-3**	4.3e-4	–13,638	117,019
(3)	State fixed effects	–4.2e-5	1.8e-4	1.6e-3***	4.3e-4	–13,748	117,019
(4)	Control for quartic in log income and RD	9.2e-4	2.3e-3	7.5e-3***	2.7e-3	–13,790	117,019
(5)	Enter everything in levels, not logs	–4.1e-8**	1.6e-8	2.8e-7***	7.4e-8	–13,807	117,019
Sample selection:							
(6)	Drop biennial obs.	2.6e-4	2.0e-4	1.6e-3***	4.3e-4	–10,989	98,621
(7)	Movers	–6.1e-4	1.3e-3	1.1e-2***	2.9e-3	–9,690	27,411
(8)	Non-returning movers	–6.9e-3***	1.7e-3	1.0e-2**	4.1e-3	–4,058	12,852
(9)	Keep if income >\$1,000	–2.4e-3***	7.4e-4	4.9e-4	5.3e-4	–13,057	110,061
(10)	Keep if income >\$1,000 and enter everything in levels	–3.7e-8**	1.6e-8	3.6e-7***	8.8e-8	–13,036	110,061
(11)	Keep if income >\$1,000 and control for quartic	1.2e-2***	3.4e-3	1.9e-2***	3.9e-3	–13,020	110,061
(12)	Keep if income <\$100,000	1.6e-4	1.8e-4	2.7e-3***	6.2e-4	–13,482	114,543
(13)	Keep the 25 most populous US states	2.8e-5	2.1e-4	1.9e-3***	5.1e-4	–11,123	97,827
(14)	Drop self-employed	–4.1e-5	2.1e-4	2.3e-3***	5.2e-4	–12,853	104,986
Endogeneity tests:							
(15)	Conditional FE logit	–1.2e-7	4.4e-7	3.3e-6***	1.0e-6	–6,469	25,718
(16)	Wooldridge (1997) transform + GMM	–2.6e-2	2.7e-2	3.2e-1***	7.7e-2		82,908

Notes: Significance levels: * 10 percent, ** 5 percent, and *** 1 percent. The table displays average partial effects (A.P.E.) and corresponding standard errors (S.E.) for individual income and relative deprivation. Standard errors are bootstrapped, robust and clustered at the individual level. The dependent variable is a dummy variable that takes the value one if the individual migrates interstate at the end of the year, and zero otherwise. Unless specified otherwise, all estimations include the same controls as the baseline model in table 10, column 3. The final two columns report the log likelihood and the number of observations (N). In row (15) the partial effect at the means is reported instead of the A.P.E., and row (16) reports the coefficient estimates and not the partial effect.

effect on migration propensity since it increases the cost of moving and, those that own their home tend not to be relatively deprived. From the table we see that – compared to the baseline model – the partial effect of relative deprivation is weakened slightly but it is still positive and significant. A one percent rise in relative deprivation increases the probability of migration by .11 percent for the typical individual. Home ownership has a strong negative effect on migration propensity.

The estimates in the third row control for a full set of state dummies, which take the value one if the individual is resident in that state and zero otherwise. If there are state-level time-invariant factors that affect the attractiveness of a state (such as amenities), then the state dummies will capture these. From the table we see that this has no discernible effect on the estimates. A likelihood ratio test firmly rejects the null hypothesis that the state fixed effects are jointly zero.

The estimation in the fourth row controls for a fourth-order polynomial in log individual income and log relative deprivation. One may think that average income and relative deprivation are capturing non-linearities in the effect of individual income on migration propensity. The estimates suggest this is not the case.

In the fifth row we control for the *levels* of individual income, estimated migrant income, average income and relative deprivation instead of their logarithms. The partial effect of relative deprivation is positive and highly significant. There is also evidence that absolute income matters too since the effect of individual income is negative and significant at the five percent level.

In rows (6)–(14) we estimate the baseline model for selected subsamples to see whether the results are driven by certain observations. In row six we drop all the biennial observations. Recall, since 1997 the PSID has surveyed sample members once every two years. This is problematic because the PSID asks respondents for their labour income in the year prior to the survey. Our estimates suggest that dropping the biennial observations does not affect the estimated partial effect of relative deprivation.

The estimates in row seven use only the observations of individuals who in-sample migrate interstate one or more times. If relative deprivation truly does affect migration propensity then it should hold for the self-selected group of migrants. Our estimates suggest this is the case and, moreover, the effect is economically stronger for movers than for the full sample. A one percent increase in relative deprivation increases the probability of migration by 1.1 percent, which is substantial.

A possible concern is that the results may be driven by returning migrants since it is typically thought that migrants return from high-income host states. Row eight of table 11 presents estimates for when the sample is restricted to in-sample interstate migrants who (in-sample) never return to either a state they have previously resided in or to the state they grew-up. The partial effect of relative deprivation remains positive and significant at the five percent level. One notable difference is that the effect of individual income is now statistically significant and negative – as predicted by the absolute income theory. This would seem to imply that whilst non-return migration is driven by bad income shocks, return migration is not.

An interesting question is whether the unemployed are driving the results. In row nine we restrict the sample to those observations where an individual reports earnings in excess of 1,000 dollars (in 1999 prices). This renders the partial effect of relative deprivation statistically insignificant, although the point estimate is positive. The effect of individual income is negative and significant. Clearly the idea that relative deprivation matters for migration choice is less convincing if its empirical relevance relies on the unemployed. It appears that the functional form assumption may be important. More specifically, whilst we expect the logarithm of income to better capture the effect of income on migration for the employed – and not the unemployed for whom a small increase in income equates to a large percentage increase – differently we expect the logarithm of relative deprivation to do a good job at capturing its effect for those with high relative deprivation – and not the high earners. For a high earner, relative deprivation is low and an additional small fall in the level of relative deprivation equates to a large percentage decrease.

To assess this, row ten again restricts the sample to those earning over 1,000 dollars but this time enters everything in levels and not logarithms. The effect of relative deprivation is positive and significant. In row eleven we control for a fourth-order polynomial in log income and log relative deprivation – again for the sample with income greater than a thousand dollars. The partial effect of relative deprivation is strongly positive and statistically significant – a one percent increase in relative deprivation increases the probability of migration by almost two percent. This point estimate is the highest of all the models considered. The partial effect of income is significant but has the 'wrong' sign.

Another concern is income outliers at the top-end of the distribution. Some of these extreme values may be due to typing errors – possibly adding one too many digits. The estimates in row twelve use the subsample that drops all observations where income is in excess of 100,000 dollars (in 1999 prices). The result is that, compared to the baseline model, the partial effect of relative deprivation on the probability of migration is higher.

Recall, when we compute relative deprivation for an individual, we use CPS data on the earnings of individuals in the same state and year as that individual. For smaller states the CPS sample size is small and possibly too small for reliable estimation of relative deprivation. In row thirteen we restrict the sample to those observations where the individual resides in one of the 25 most populous U.S. states. Again the effect of relative deprivation is positive and significant. The effect of average income is now significant but has the 'wrong' sign.

In row fourteen we exclude the self-employed from the sample. One may think that the self-employed have different behavioural characteristics to those who work for someone else – and, consequently, may have different slope coefficients. Dropping the self-employed increases the estimated average partial effect of relative deprivation. The partial effect of individual income remains statistically insignificant.

The remaining two rows of table 11 present estimates that attempt to do more in terms of achieving consistent estimation of causal effects. Row fifteen presents estimates from the logit conditional fixed-effects model of equation (17) (see Chamberlain (1980)). The reason we do this is because, for the logit model, Chamberlain (1980) showed that a sufficient statistic for the fixed effect (α_i) exists. Indeed, conditioning the likelihood of observing our data on $\sum_t m_{it}$ eliminates α_i from the conditional likelihood function (see Greene and Hensher (2010)). Therefore, we do not have to rely on Mundlak's (1978) specification assumption for α_i. Unfortunately this comes at a cost since the resulting fixed effects model can only be estimated for the subsample of movers.[93] Therefore, in order to use the conditional fixed effects estimates to say something about the whole population, one needs to take a leap of faith and assume that the in-sample movers are not that different from the stayers. The estimates displayed in row fourteen are partial effects evaluated at the mean of the regressors and not the average partial effect.[94] The estimated partial effect for relative deprivation is positive and significant.

Of course we should still be concerned that the regressors are correlated with past

[93]The reason is that, for those who do not migrate in-sample, the value of the dependent variable is equal to zero in every period, which is perfectly explained by conditioning on $\sum_t m_{it}$.

[94]The conditional logit model will not give us estimates of the fixed effects that we require to calculate the partial effects of the regressors. We follow the method proposed in Greene and Hensher (2010) to estimate the average fixed effect for the estimation sample and use this to calculate the partial effect at the means. For our sample the estimated partial effect at the means is less than the average partial effect.

unobserved idiosyncratic disturbances. For example, this would occur if a shock to the migration decision today affects future income. More generally, any variable that is directly or indirectly chosen by an individual may not be strictly exogenous. The literature on the estimation of non-linear fixed effects panel data models *without strict exogeneity* is tiny. For consistent estimation when the regressors are predetermined, Wooldridge (1997) extends the work of Chamberlain (1992) and proposes a quasi-differencing transformation and then Generalised Method of Moments (GMM) estimation of the resulting orthogonality conditions.[95]

We assume that the predetermined regressors are individual income, predicted migrant income, relative deprivation, and the unemployed and college degree dummies. The GMM estimation uses all the available lags of the predetermined regressors as instruments in a given year. We treat the remaining regressors as strictly exogenous since these either can be considered deterministic (such as age) or they are state-level.[96] We assume a logistic distribution for the idiosyncratic error term. The estimates in row sixteen of table 11 display the coefficient (not the partial effect) estimates from Wooldridge's (1997) estimator. The coefficient estimate on relative deprivation is positive and statistically significant. The coefficient estimate on individual income is negative but statistically insignificant.

In summary, from studying interstate migration in the U.S., we have amassed evidence in favour of Stark's relative deprivation theory of migration. We find little support for the absolute income theory that dominates the migration literature and the thinking of

[95] To remove the fixed effect for a binary model, we need to assume a particular variant of equation (17) such that the probability of migration can be factored into the product of a term that depends only on the fixed effect and a term that depends on the regressors. More specifically, rather than including the fixed effect α_i in the expression for $m^{\star}_{it}$, we instead add the condition that migration occurs if the fixed effect is greater than some threshold (see Wooldridge (1997)). This may not be too restrictive since only certain types of people would even consider migration. There are some people that will never consider moving, no matter how large the income gain from migration.

[96] An individual-level shock has a negligible effect on a state-level variable.

policy makers.

5 Concluding Remarks

This paper has examined whether absolute income or relative income (to others in some comparison group) provides the main motivation for migration. Almost all models of migration – both theoretical and empirical – assume that absolute income determines migration. Indeed, the most popular model of migration, George Borjas' (1987) selection theory, is built on the assumption that absolute income differentials between the source and destination provide the incentive for migration. The model is so popular that a whole literature is devoted to testing the migrant quality (or selection-on-skills) predictions of Borjas' model, and none of these papers control for relative income. All this is at odds with the mounting evidence that suggests utility is driven by relative income (or relative deprivation) as well as absolute income, particularly after a threshold level of income – needed for the essentials in life – is exceeded.

We show that, under some conditions, the two main theories (absolute income and relative deprivation) predict the same aggregate relationship between income inequality and the quality (or selection-on-skills) of migrants. We argue that in order to distinguish between the two theories, one needs individual-level data. Moreover, one needs individual-level panel data on before and after migration outcomes. The reason is that, since migration and non-migration are mutually exclusive, one has to estimate the (counterfactual) migrant earnings of non-migrants using the subsample of migrant earnings. If migrants are selected on unobservables, then cross-sectional estimates will systematically bias the predicted migrant earnings of non-migrants. Importantly, we show that the estimates are biased in favour of the finding that relative deprivation is important precisely when migrants are positively selected-on-unobservables, which is difficult to reconcile with the relative deprivation theory. Hence the need for individual-level panel data to correct for selection-on-unobservables. Since the current literature either fails to control for relative deprivation or fails to control for selection-on-unobservables (or both), we undertake some empirical analysis of our own.

The paper estimates the relative importance of the two main theories in explaining interstate migration in the United States. The data is a panel of individuals from the Panel Study of Income Dynamics. We assume that the reference group to which income comparisons are made is the population of the U.S. state of residence. First we find that for the subsample of migrants, their income and relative deprivation both improve post-migration. Second, we jointly estimate the effects of individual income and relative deprivation on the propensity to migrate out of the source state. We find strong and robust evidence that an increase in relative deprivation increases the probability of migration. In contrast, our estimates suggest individual income has no significant effect on migration propensity. This is true even after controlling for the estimated gain in income from migration.

In studying U.S. interstate migration, we are looking at the migratory behaviour of people that – generally speaking – have enough income to buy the 'essentials in life', and hence are more likely to care about relative income than those on very low incomes. Therefore, whether our findings have wider applicability to regional migration in low-income countries, or international migration (particularly between low- and high-income countries), should be the subject of future research. Since in many cases of international migration the 'essentials in life' are not satisfied, we would expect absolute income to be more important for international migration.

On the one hand, our results are surprising given that the migration literature (and migration policy) is dominated by considerations of absolute income differentials between the source and destination. On the other hand, our results support the recent survey evidence that happiness is determined by relative income (or deprivation), particularly when the average level of income is high.

There are several other promising avenues for future research. If, as we suggest, relative deprivation is the correct theory of migration, then a big question concerns how the reference group is chosen. Here we briefly discuss two aspects: (1) what is the correct size (persons) of the reference group; and (2) how does the reference group change in response to actions, including migration. Regarding (1), we assume the reference group coincides with the population of a U.S. state, but one may think that the true reference group is much narrower than this, particularly for the larger states.[97] If the true reference group is narrower than the state, then our estimate of the effect of relative deprivation on migration can be seen as an underestimate. To see this, consider a state that contains a rich and a poor neighbourhood. Assume that the inhabitants follow the relative deprivation hypothesis. If the true reference group is the neighbourhood, then the relatively deprived within each neighbourhood are more likely to migrate – either to another neighbourhood within the same state or to another state. If, however, the true reference group is the state, then the inhabitants of the poor neighbourhood are more likely to migrate. Therefore, if the reference group is wrongly taken to be the state rather than the neighbourhood, it works against our finding that relative deprivation matters – the relatively deprived in the rich neighbourhood are not deprived at the state level.

The second aspect concerns the endogeneity of the reference group. If relative deprivation matters, as we suggest, then an individual that migrates from a poor to a rich region will surely do all he can to prevent reference substitution. For example, this may require the migrant to avoid mixing with destination natives and instead form social ties

[97]Using population figures from the 2000 Census, one may be sceptical that a Californian compares himself to around 34 million other California residents, whereas a resident of Wyoming compares himself to half a million.

with earlier migrants from the same source. If so, then one can expect enclaves and segregation. An interesting question is whether leaving some family members behind in the source helps to prevent reference group substitution? If so, it may provide a new explanation for remittances, since it is a mechanism through which migrants avoid reference substitution. Another question of interest is how the mere passing of time spent in the destination affects the likelihood of reference substitution? If the probability of reference substitution increases with time spent in the destination, then migrants may circle from source-to-destination-to-source and so on. Conversely, a migrant from a rich source region will want to encourage reference substitution to that of a lower-income destination. Migrants may then bring their families with them and set up ties with natives in the destination. Furthermore, there are interesting equilibrium aspects to be thought through. Clearly, the location decision of one person changes the well-being of all persons in the source and destination reference groups.

It is natural to question the unconventional. Many readers will ask, if relative income is so important for migration choice, why do we not see an exodus from (high-income) New York to, say, (low-income) Louisiana? In response we would say that our question is why we do not see the reverse flow. A Louisiana janitor will probably earn more doing the same job in New York, but he or she will likely be relatively more deprived in comparison to the high-income New Yorkers. People trade-off the change in relative deprivation with the change in income from migration and, on average, they tend to counter-balance each other.

One final comment. The existing evidence that finds well-being is determined by relative income (or deprivation) – as well as absolute income – is from self-reported happiness and life satisfaction. In contrast, we have revealed preferences that support the relative deprivation theory using the actual migration actions of individuals and, not subjective survey responses.[98] Indeed, migration provides excellent natural variation to assess relative deprivation. This is because relative deprivation can change substantially upon migration, particularly when reference substitution occurs.[99] More research needs to be done to assess the wider applicability of our result and, if our findings are confirmed, then an evaluation of current migration and redistributive policy may be in order.

[98]Naturally one may be sceptical as to whether survey respondents report their true feelings when the question is subjective. Indeed, often a reordering of questions or a slight change in question wording can lead to a different answer (Bertrand and Mullainathan, 2001). Nonetheless, Frey and Stutzer (2002) present evidence to suggest that self-reported happiness is a reliable indicator of well-being.

[99]Furthermore, the criticism that using migration data in this way is problematic since one cannot know the true reference group and how it changes after migration can equally be directed at the happiness literature.

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Appendices

A Sample and Variable Construction for Empirical Analysis

This Appendix describes the construction of the sample and each variable used in the empirical analysis of section 4.

The Sample

The sample is from the Panel Study of Income Dynamics (PSID). The PSID contains two separate data sets: an individual file with longitudinal data on every individual that has ever appeared in the PSID; and a family file for each cross-sectional year that contains information on the head, “wife" and family unit for all family units sampled in that year. The family files contain the vast majority of survey information, while the individual file is needed to keep track of a specific individual because of moves into and out of different family units. Importantly, both the individual file and family file contain the year, family-unit, and relationship-to-head identifiers, which combined permit us to link the two data sets.[100]

We merge the files in the following way. First, we reshape the individual file into long format; that is, each row now contains a unique individual-year identifier. Second,

[100]Note that when we say relationship-to-head we do not mean that we directly use the variable “Relationship to head" as given in the PSID individual file. Indeed, the PSID variable “Relationship to head" is not sufficient to identify the current head because any last year’s head (or wife) that moved out is also recorded as the head (wife) in this variable. As the PSID documentation explains, the current head is identified by yearly values for "Sequence Number" 1-20 *and* "Relationship to head" 1 or 10. The current wife "wife" is identified by yearly values for "Sequence Number" 1-20 *and* "Relationship to head" 2, 20, or 22. In 1968 we can safely identify head with "Relationship to head"=1 and wife with "Relationship to head"=2 because, trivially, there are no movers in the first year of the sample.

we download the family files for all survey years into a single file and reshape it into long-long format so that each row is either a head-year or wife-year or family-year observation. We then merge the family file with the individual file in three steps. Firstly, we merge based on year-family-head and year-family-wife for all head and wife observations, respectively. Secondly, we merge based on year-family for all current period family unit variables. Finally, we merge based on year–family–non-split-off–non-mover for all family unit variables that are lagged one period; for example, the survey question on family income asks retrospectively what family income was in the prior period. A problem arises when the head, for example, moved families between the prior and current period, since merging based on year-family-head will incorrectly allocate family income to the wrong head. Hence, we merge the lagged family-level data only for those family members who did not change families between the prior and current period. These are non-split-off families and non-mover individuals.

Our working sample includes people that meet all of the following four criteria:

1. The ***head of household*** is typically the adult male head (the husband if married) unless an adult male is not present or is severely disabled. The current head is identified jointly by yearly values for “Sequence Number" in the range 1-20 (PSID variable name ER30021 in 1969) *and* a “Relationship to head" value of 1 or 10 (PSID variable name ER30003 in 1968). The sequence number is used to ensure that only the current head is included and not the head in the previous wave in the event that the previous head moved out of the household. In 1968 we can safely identify the head with a “Relationship to head" value 1 because there are no movers in the first period.

2. Of ***working age*** is defined as those persons aged between 16 and 64. See the entry for ***Age*** below.

3. In the ***labour force*** is determined by looking at the employment status of the head

from the family files. Prior to 1976, employment status was coded using six values (PSID variable V196 in 1968), where the labour force are those with values 1 or 2. Between 1976 and 1996 there were eight values, where the labour force are those with values 1-3. Since 1996 respondents were offered more than one mention to describe their employment status. We use the first mention (variable ER10081 in 1997).

4. ***Non-institutionalised*** individuals are people that are not in the armed forces, prison, a health care or educational facility. We drop those in the armed forces using occupation. Members of the armed forces have occupation code 55 in the 2-digit classification (variable V4459 in 1976), code 600 in the 1970 Census Occupation Codes (COC) (PSID variable V7712 in 1981), and code 984 in the 2000 COC (PSID variable ER21145 in 2003). We also use type of institution for the family unit (variable V11124 in 1985) to determine when a family is institutionalised, which includes those in the armed forces living off base.

The Dependent Variables

End-of-period migration is a dummy variable that takes the value one if the individual changes state between the current and next survey, and takes a zero value otherwise. The state of residence is recorded in the PSID family file. Prior to 1985, states were coded according to the GSA classification (variable V93 in 1968) and from 1985 classified using the FIPS system (variable V12380 in 1985). We converted the FIPS codes to the GSA classification. There are instances where an individual has a gap between records because of non-response or missing values. When the gap is more than two years we set the end-of-year migration decision to missing (1.5 percent of observations).

End-of-period return migration is a dummy that takes the value one if the individual returns to a state he previously resided in between the current and next survey, and takes a zero value otherwise. We keep track of all states an individual has previously resided in within sample and, in addition, the PSID records the state the individual grew-up (defined as where the individual spent most of his years between the ages of 6 and 16). Prior to 1994 the grew-up state was coded using the GSA classification (variable V311 in 1968) and since 1997 used the FIPS code (variable ER11842 in 1997).

The Regressors

Individual income includes wages, bonuses, overtime, commissions and the labour part of business and farm income (PSID variable V74 in 1968) and refers to total annual income before tax in the previous year to the survey. In the years 1994-1996 and 2001, labour income was reported excluding the labour part of business and farm income. For these years we construct total labour income by summing labour income excluding business and farm income (variable name ER4140 in 1994), farm income (ER4117 in 1994) and the labour portion of business income (ER4119 in 1994). Labour income is expressed in constant 1999 dollars using the CPI-U. Survey respondents are asked about their labour income in the previous year. We forward labour income by one survey wave to account for this although it is, of course, imperfect for the biennial surveys post-1997.

Average income is the sample mean income from the Current Population Survey in a given state-year, where the sample is restricted to those in the labour force. The income series includes wages and salaries and is expressed in 1999 dollars.

Unemployed is a dummy variable that takes the value one if the individual is unemployed in the current period and zero otherwise. From the employment status variable (PSID variable V196 in 1968), the unemployed have code 2 for years prior to 1976 and code 3 since 1976.

Own home is a dummy variable that takes the value one if the individual owns their home and zero otherwise. This is determined by looking at the value of the house (PSID variable V5 in 1968), which is coded zero if the individual is not a home owner.

Age is reported in the PSID for an individual in each survey (PSID variable name ER30004 in 1968). We take the first recorded age of the individual and apply the gap in survey years to fill in age over time. We do this to avoid the sporadic two-year jumps or no change in reported age between surveys that sometimes occur due to changes in the date of the survey within a year.

College degree is a dummy variable that takes the value one if the individual has a Bachelor's degree (1968-1974 we use PSID variable with name V313 in 1968; 1975-2009 we use PSID variable with name V4099 in 1975).

Married is a dummy variable that takes the value one if the individual is married at the time of the PSID survey and zero otherwise. We use the married pairs indicator from the individual file (PSID variable ER30005 in 1968).

Children is the number of children under 18 living in the family unit at the time of the PSID survey (PSID variable V398 in 1968).

Borders is the number of contiguous U.S. states for the state that the individual resides.

Land area of a state is obtained from the U.S. Census Bureau and is measured in square kilometres.

Sampling weights

Sampling weights are inverse (ex-ante) sampling probability weights supplied by the PSID. From 1968 to 1989 we use the "Core Individual Weight" (variable name ER30019 in 1968); 1990-1992 the "Combined Core-Latino Weight" (ER30688 in 1990); 1993-1995 the “Combined Core-Latino Sample Longitudinal Weight" (ER30866 in 1993); 1996 we use the “Core Sample Individual Longitudinal Weight" (ER33318) and post-1996 we use the “Combined Core-Immigrant Sample Individual Longitudinal Weight" (ER33430 in 1997).