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Dynamics of Labor Market Earnings in Urban Mexico, 1987-2002

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Abstract

This paper studies short-run individual earnings mobility in urban Mexico from 1987 to 2002. It analyzes whether initially advantaged individuals experience more positive earnings mobility than the initially disadvantaged ones. It also studies whether earnings converge over time to their conditional mean, and what is the impact of socioeconomic characteristics on earnings mobility. The results show that while there is a great amount of convergence in the earnings of rich and poor over a year, this convergence is mostly due to transitory adjustments in earnings, i.e. it is due to earnings converging to their own conditional mean. Individuals with characteristics that give them a more permanent advantage in the labor markets (like high levels of education, being a male, etc.) usually keep their high earnings over a year. The main exception to this finding occurred in the aftermath of the 1994 Peso crisis when everybody experienced proportional earnings losses, and hence the permanently advantaged individuals experienced greater losses in absolute terms. Holding everything else constant, having high levels of education, being a male, becoming a formal sector self-employed, and living in cities in the US Border and in the North of the country is usually associated positive mobility. On the contrary, transitions into informal wage work and living in the Center and South of the country brings more negative conditional mobility.

Resumen

Este artículo estudia la movilidad de corto plazo en las ganancias salariales individuales en el México urbano desde 1987 hasta 2002. En particular, estudia si los individuos que muestran una ventaja inicial en un periodo base tienen una movilidad más positiva que aquellos que empezaron desde una posición desventajosa. También estudia si las ganancias salariales individuales convergen a su media condicional, así como el impacto de características socioeconómicas en la movilidad salarial. Los resultados muestran que, aunque hay un alto grado de convergencia entre las ganancias de pobres y ricos a lo largo de un año, dicha convergencia se debe a ajustes transitorios en las ganancias salariales, es decir debido a convergencia condicional. Individuos con características socioeconómicas que les dan una ventaja más permanente en los mercados laborales (como un alto nivel educativo, ser hombre, etc.) en general mantienen sus altas ganancias salariales a lo largo de un año. La principal excepción a esta regla ocurre durante el periodo que le sigue a la crisis económica de fines de 1994, cuando la mayoría de los trabajadores experimentaron pérdidas

salariales proporcionales, y por lo tanto, los individuos aventajados perdieron más en términos absolutos.

Ceteris Paribus*, el tener altos niveles de educación, ser hombre, volverse un autoempleado formal y vivir en ciudades del norte del país está asociado a movilidad salarial positiva. Por el contrario, transiciones hacia el sector informal asalariado, así como vivir en el centro y sur del país trae movilidad condicional negativa.*

Dynamics of Labor Market Earnings in Urban Mexico, 1987-2002

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This paper studies short-run individual earnings mobility in urban Mexico from 1987 to 2002. It analyzes whether initially advantaged individuals experience more positive earnings mobility than the initially disadvantaged ones. It also studies whether earnings converge over time to their conditional mean, and what is the impact of socioeconomic characteristics on earnings mobility. The results show that while there is a great amount of convergence in the earnings of rich and poor over a year, this convergence is mostly due to transitory adjustments in earnings, i.e. it is due to earnings converging to their own conditional mean. Individuals with characteristics that give them a more permanent advantage in the labor markets (like high levels of education, being a male, etc.) usually keep their high earnings over a year. The main exception to this finding occurred in the aftermath of the 1994 Peso crisis when everybody experienced proportional earnings losses, and hence the permanently advantaged individuals experienced greater losses in absolute terms. Holding everything else constant, having high levels of education, being a male, becoming a formal sector self-employed, and living in cities in the US Border and in the North of the country is usually associated positive mobility. On the contrary, transitions into informal wage work and living in the Center and South of the country brings more negative conditional mobility.

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Keywords: Earnings Dynamics, Mexico.

1 Motivation

This paper studies earnings dynamics in urban Mexico from 1987 to 2002. In particular, it focuses on the impact of initial earnings and other socioeconomic characteristics on earnings mobility.

The specific questions this study tries to answer are 1) "Are the most advantaged individuals gaining more (losing less) in terms of earnings changes?", 2) "What is the *ceteris paribus* impact of socioeconomic characteristics of the individual on earnings mobility?", and 3) "How do these socioeconomic factors affect the impact of initial earnings on mobility?".

The first question is concerned with whether the mobility process benefits (hurts) the rich more (less) than the poor, or is it that this process benefits more individuals at the bottom of the earnings distribution, allowing them to catch-up as time goes by? This question is closely related to the study of poverty traps and cumulative advantage. The main difference between those studies and this paper is that such studies are usually concerned with mobility in the long-run, while the present paper, due to data limitations, focuses on short-run mobility.

With respect to the second question, i.e., the impact of socioeconomic characteristics on earnings mobility, this paper tries to specify which variables, in addition to initial earnings, explain earnings mobility. In particular, it starts by specifying an earnings equation in levels and derives a mobility model that captures the impact of factors like gender, education, age, sector of employment (informal vs. formal), and geographical region on earnings changes.

Finally, the third question deals with how controlling for the aforementioned socioeconomic factors changes the impact of initial earnings on earnings mobility. This question provides a test of whether individual earnings are converging to their *conditional* mean.

The research on economic mobility issues in developing countries is fairly recent. While mobility studies were performed in the developed world since the second-half of the twentieth century, such topics started to be addressed in developing nations only towards the end of last century, mostly due to the lack of suitable longitudinal data, following the same unit of analysis over time. The importance of mobility studies comes from their ability to follow the destinies of individuals or households over time. This advantage over cross-sectional studies helps in tackling new questions that are inherently dynamic in nature.

This study uses a series of short overlapping panels with quarterly information tracking individuals for at most 1 year. The period covered by these short-lived panels goes from January 1987 to December 2002.[1] The earnings mobility analyzed is yearly earnings mobility. This allows capturing the longest period of time possible for each individual, and avoids having to worry about issues of seasonality in the data. Having many short-lived panels, covering over 15 years, is a unique opportunity for analyzing an economy like the Mexican, which underwent radical transformations during this period, including a long process of economic liberalization, and a severe financial crisis in December 1994. Hence, this study attempts to identify the differential impact of macroeconomic shocks on earnings mobility in the short-run.

Two methodological issues that are of particular concern in the mobility literature are the potential effects of measurement error in the earnings variable and the attrition of individuals from the panel. The robustness of the results to these problems is explored.

The data used in this paper is presented in section 2. Section 3 summarizes previous findings in the literature together with the contribution of this paper. The methodology is introduced in section 4, and the main results are presented in section 5. Robustness tests are presented in section 6. Finally, section 7 concludes.

2 Data

This paper uses data coming from the National Survey of Urban Employment (in Spanish "Encuesta Nacional de Empleo Urbano") from now on abbreviated as ENEU. This is a survey conducted on Mexican urban households with the purpose of inquiring about the conditions that prevail in urban labor markets. The database is a rotating panel with quarterly data. It tracks individuals for at most 5 quarters in the most important urban areas of the country. The sampling is done in three stages, based on a sampling frame of dwellings.

The survey gathers information about socioeconomic characteristics such as age, gender, education, marital status, labor force participation, labor market earnings, sector of employment, occupation, type of fringe benefits, hours worked in the market, as well as hours devoted to other activities (e.g.,

[1] Although it would be desirable to have panel data following individuals for more than a year, such data does not exist yet for Mexico.

housework), type of employment contract, firm size, employment search activity, dwelling characteristics, etc. The survey is designed to be geographically and socioeconomically representative of urban Mexico.[2] Furthermore, it is one of the surveys used by the government to create employment statistics.

Although the geographic coverage of the ENEU has expanded substantially over time, the analysis presented in this paper restricts the sample to the 16 cities that originally appeared in the sample of 1987. Doing otherwise might confound the "true" evolution of earnings mobility with the effects caused by the expansion of the geographical coverage.

As previously mentioned, this is a study on earnings mobility in the short-run. The short temporal coverage of each panel makes it impossible to draw conclusions on what happens with earnings mobility in the long-run. However, a long period of time, that runs from 1987 to 2002, is covered by using many of these short-lived overlapping panels. These years include several periods of growth and the major recession following the 1994 Peso crisis. This period also coincides with the years of trade liberalization in Mexico.[3]

In the 3rd quarter of 1994 a new questionnaire was applied in the survey, and although there were minor changes with respect to the previous questionnaire, this paper avoids using the panels in which the individuals experienced a change in questionnaire.

The unit of analysis is the individual worker. Throughout the panel individuals are matched according to their household and personal identification numbers, and also by their age, gender and years of education in order to minimize the probability of spurious matching.

This paper studies one-year earnings mobility (from the initial interview quarter to the same quarter next year). Since the survey follows individuals for at most 5 quarters, then at most one observation of yearly earnings changes exists per individual. This precludes using any panel data econometric technique. Although there is information available on earnings at other quarters, studying earnings changes over shorter periods of time (e.g., quarterly mobility) is not pursued here. The reason for not doing this, is that a year is already a short period over which to study mobility. Furthermore, restricting the analysis to yearly mobility helps in avoiding having to deal

[2]The survey stratifies the population according to wealth.

[3]Unfortunately, there are no comparable datasets that cover the periods previous to the beginning of the trade liberalization process. Also, since this process was a slow one, it is hard to find a break point which would identify the impact of new trade conditions, separately from other factors.

with the potential effects of seasonality in the data. Further extensions to the present work can include modeling the covariance structure of earnings using all the 5 periods available for each individual.

The subpopulation of study is restricted to individuals between 25 and 60 years of age. Also, all the estimations are restricted to individuals who are double-labor-force participants (i.e., that are in the labor force both in the first interview and one year later). The reason for applying these restrictions is to avoid having to analyze the mobility associated with first time entries into the labor force by young people who recently graduated from school, retirement decisions, and transitions-in-and-out of the labor force in general. Although this might hide some interesting effects, like the entrance into the labor force of family members during times of recessions, it helps focusing the study on the earnings mobility experienced by workers who are more permanently attached to the labor market. Note however, that unemployed individuals *are* included in the analysis. This is done because finding and/or losing a job is an important event *per se*, that affects the welfare of an individual and it involves a significant mobility in earnings.

The earnings variable is real earnings measured in 2002 Mexican pesos. The advantage for using such year as base period, is that back then the nominal exchange rate between the US Dollar and the Mexican peso was about 10 pesos per dollar, something which facilitates the interpretation of the results to the international reader.

Finally, it is important to remark that all the calculations here presented are weighted estimations using the survey factor weights. This is done in order to obtain estimates that are more representative at the national urban level. The author has performed most of these estimations with unweighted data, and the conclusions reached do not change significantly.[4] Also, whenever possible the standard error estimates and statistics calculated are adjusted for the characteristics of the survey design, in particular for clustering and stratification.

The next section presents some descriptive statistics for the main sample under study in the ENEU, and some data on the macroeconomic evolution of the Mexican economy. All the calculations presented were performed by the author using the ENEU surveys, unless explicitly noted.

[4] Some of these results are included in Fields *et al.* (2005). As it will be seen below, due to problems of attrition in the panel sample the weighted mobility estimators might not necessarily representative.

2.1 Descriptive Statistics

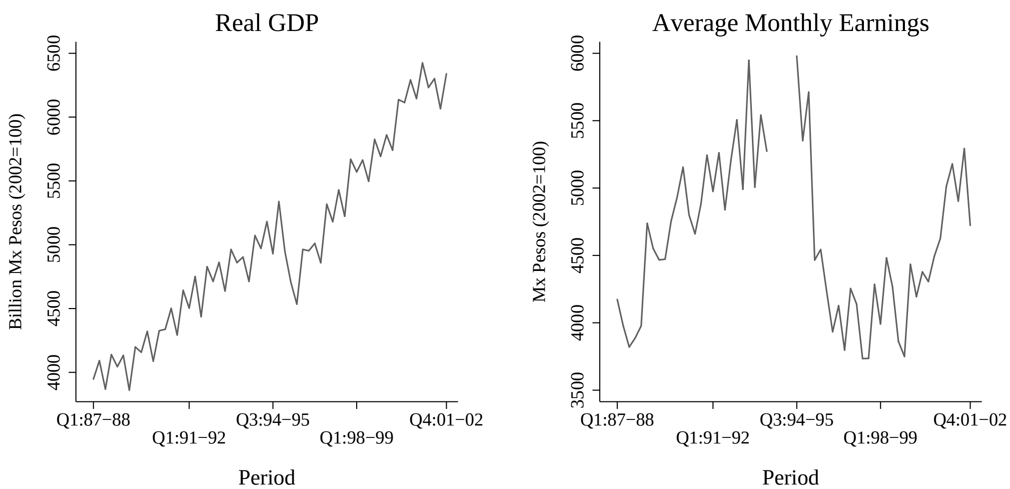
The first graph introduced in this section presents the evolution of GDP in Mexico together with the evolution of average earnings for the sample used in this paper. Figure 1 shows an upward trend in real GDP during the years going from 1987 to 1994, when the December Peso crisis hit the economy. After this crisis, output suffered a sharp downturn, out of which it started rapidly recovering. Nevertheless, the pre-1994 aggregate output levels were not recovered until 1999. From 1999 onwards, the Mexican economy continued its growth, but by 2001-2002 a new recession had started again.

Average earnings followed the steady growth of the economy during the period going from 1987 to 1994. After the 1994 Peso crisis, earnings fell dramatically. However, unlike GDP, they did not start their recovery but until much later. It was only after 1999 that average earnings started growing again, and by 2002 they hadn't reached yet their pre-1994 level. These graphs serve to illustrate that earnings did not exactly match the evolution of aggregate output. Instead, three clear periods can be distinguished in the evolution of earnings. The first period goes from the 1st quarter of 1987 to the 2nd quarter 1993, the second going from the 3rd quarter 1994 to the 1st quarter 1999, and the last one going from the 2nd quarter 1999 to the 4th quarter of 2002. This classification of the evolution of earnings will become useful for presenting results in a more compact way, by pooling panels for these broad periods.[5]

The evolution of earnings inequality in the sample is presented in Figure 2, which depicts the Gini coefficient estimated at the initial interview of the panels. This picture shows that inequality grew for the first half of the period under study. After 1994 it remained fairly constant at this higher level.

The characteristics of the sample are displayed in Table 1. This table shows that the average age of the individuals in the sample was around 37 years of age for the first half of the period, and then increased afterwards, raising this average by 1 year. On the other hand, individuals had on average 9 years of education during the first half of the sample, and by the end this

[5] The reader will notice that (with the exception of the GDP graph which comes from National Accounts statistics), all the other graphs have a break in the middle, where no data is reported. This will happen with all the figures generated with the ENEU survey. The break comes from omitting those panels where the questionnaire changed, and for which no analysis was performed.



Source: GDP−INEGI National Accounts, Earnings−author's calculation based on ENEU's surveys

Figure 1: GDP and Average Earnings

Figure 2: Gini Coefficient for Individuals in the Sample

number had risen by one extra year.[6] It is important to recall that these numbers pertain to individuals from 25 to 60 years of age who are in the labor force both at the first and the last interview. Hence, these numbers do not represent the whole labor force. Including all the individuals in the labor force would have the effect of reducing the average age and bringing in less educated workers. The fraction of males in the sample is around 70% and declining over time, due to the steady rise in the labor force participation of women.

[6]However the median education remained 9 years over time.

Table 1: Descriptive Statistics for the Individuals in the Sample.

	Q1:87-Q2:93			*Q3:94-Q1:99*			*Q2:99-Q4:01*		
	Mean	Std. Dev	Median	Mean	Std. Dev	Median	Mean	Std. Dev	Median
Earnings	4826.1	5896.7	3336.2	4324.6	5651.2	2879.3	4703.7	5604.5	3199.5
Age	37.7	9.0	36	38.0	8.9	37	38.3	9.0	37
Years of Education	8.9	4.8	9	9.6	4.8	9	9.9	4.7	9
Male	0.72	0.45	1	0.70	0.46	1	0.68	0.47	1
Sector of Employment									
Unemployed	0.015	0.12	0	0.029	0.17	0	0.015	0.12	0
Informal Wage Workers	0.087	0.28	0	0.115	0.32	0	0.116	0.32	0
Informal Self-employed	0.219	0.41	0	0.236	0.42	0	0.235	0.42	0
Formal Self-employed	0.015	0.12	0	0.012	0.11	0	0.011	0.10	0
Formal Wage Workers	0.665	0.47	1	0.608	0.49	1	0.623	0.48	1
Sector Transitions									
Unemployment to:									
Unemployment	0.002	0.05	0	0.006	0.08	0	0.002	0.05	0
Informal Wage Work	0.002	0.04	0	0.004	0.07	0	0.002	0.05	0
Informal Self-employment	0.003	0.05	0	0.006	0.08	0	0.003	0.05	0
Formal Self-employment	0.000	0.01	0	0.000	0.01	0	0.000	0.01	0
Formal Wage Work	0.008	0.09	0	0.012	0.11	0	0.008	0.09	0
Informal Wage Work to:									
Unemployment	0.002	0.04	0	0.003	0.06	0	0.001	0.04	0
Informal Wage Work	0.040	0.20	0	0.061	0.24	0	0.063	0.24	0
Informal Self-employment	0.020	0.14	0	0.024	0.15	0	0.023	0.15	0
Formal Self-employment	0.000	0.02	0	0.000	0.01	0	0.000	0.01	0
Formal Wage Work	0.025	0.16	0	0.026	0.16	0	0.029	0.17	0
Informal Self-employment to:									
Unemployment	0.003	0.05	0	0.004	0.07	0	0.002	0.04	0
Informal Wage Work	0.022	0.15	0	0.028	0.17	0	0.028	0.16	0
Informal Self-employment	0.162	0.37	0	0.175	0.38	0	0.172	0.38	0
Formal Self-employment	0.004	0.07	0	0.003	0.06	0	0.004	0.06	0
Formal Wage Work	0.028	0.16	0	0.025	0.16	0	0.029	0.17	0

Table 1 (Cont.)

	Q1:87-Q2:93			*Q3:94-Q1:99*			*Q2:99-Q4:01*		
	Mean	Std. Dev	Median	Mean	Std. Dev	Median	Mean	Std. Dev	Median
Formal Self-employment to:									
Unemployment	0.000	0.01	0	0.000	0.02	0	0.000	0.01	0
Informal Wage Work	0.000	0.02	0	0.000	0.01	0	0.000	0.01	0
Informal Self-employment	0.006	0.08	0	0.004	0.06	0	0.004	0.06	0
Formal Self-employment	0.005	0.07	0	0.004	0.07	0	0.004	0.06	0
Formal Wage Work	0.003	0.06	0	0.003	0.05	0	0.002	0.05	0
Formal Wage Work to:									
Unemployment	0.009	0.09	0	0.012	0.11	0	0.009	0.10	0
Informal Wage Work	0.027	0.16	0	0.028	0.17	0	0.030	0.17	0
Informal Self-employment	0.032	0.18	0	0.028	0.16	0	0.032	0.18	0
Formal Self-employment	0.003	0.05	0	0.002	0.04	0	0.003	0.05	0
Formal Wage Work	0.594	0.49	1	0.539	0.50	1	0.549	0.50	1
Region									
Mexico City	0.57	0.50	1	0.52	0.50	1	0.50	0.50	0
US Border	0.05	0.23	0	0.07	0.26	0	0.08	0.27	0
North	0.18	0.39	0	0.21	0.41	0	0.22	0.41	0
Center	0.18	0.38	0	0.17	0.37	0	0.18	0.38	0
South	0.02	0.14	0	0.03	0.16	0	0.03	0.17	0

The sectoral composition as well as the transitions between sectors are also presented in Table 1. Before analyzing these numbers, it is important to clarify how the sector variables are constructed. In order to classify an individual as being in the formal or the informal sector, a question in the survey that asks whether the firm at which the individual worked last week had a name, or whether it was registered with the authorities, was used. An individual is considered to work in the informal sector if he reports that such firm did not have a name, nor it was registered with the authorities. To cross-check this statement, the individual must have not received any fringe benefit from this job, like health coverage, housing credit, social security, etc. In addition to that, if an individual was a self-declared informal street vendor, or worked at a firm with less than 6 employees that provided no fringe benefits at all, then he was also considered to be in the informal sector. The reason for this last classification choice is that some people might work at an informal firm that has a name, but no official registration. This is likely to be the case of individuals working at micro scale firms that provide no coverage to their workers. The classification into formal and informal sectors is further broken down by whether the individual is self-employed or a wage worker. There is evidence that wage workers and self-employed can differ dramatically in their characteristics (see for instance Maloney, 1999).

Table 1, shows that approximately 60% of the individuals in the sample are formal wage workers, slightly more than 20% are informal self-employed, around 10% are informal wage workers, and around 1.5% are unemployed and formal self-employed. The low fraction of unemployed individuals in the sample is a known feature of the Mexican labor markets. In general, unemployment is low in Mexico, because there are no institutions that can cover a long search period for an individual who suddenly loses a job. Furthermore, the unemployment rate in this sub-sample of middle-aged workers seems to be lower than the unemployment rate of the overall urban population which is around 3.5%.

Regarding sector transitions, it can be noticed that the majority of initially unemployed individuals become Formal Wage workers after one year. For the individuals initially working in other sectors the majority of them tend to remain in the sector they started in.

Finally, Regarding the regional composition of the sample, most of the individuals included come from Mexico City (less than 60%), while the North and the Center regions in the country represent approximately 20% each. Finally, the US Border cities and the South region represent a small fraction of the sample. As previously mentioned, the ENEU expanded its geographic coverage over time, but this study focuses only on the cities present in the 1987 survey, in order to avoid confounding effects from expanding such coverage. The list of cities included under each region appears on the appendix A.1, together with the number of observations contained in each panel.

3 Previous Research and Contribution

The literature on the relationship between mobility and initial earnings focuses on two different questions. The first one is "What is the relationship between earnings changes and initial earnings?", and the second "What is the relation between earnings changes and initial earnings, after one has controlled for the effects of individual characteristics like age, education, gender, etc.?".

These two questions are different in nature. The unconditional question deals with the common concern of whether "the richer are getting richer (and the poor poorer)", while the conditional one is concerned with the determinants of mobility and the existence of state dependence in the conditional earnings dynamics.[7]

For developed countries, the study of earnings dynamics has been pursued at a great level of detail. In the US alone, earnings mobility studies have addressed issues like the role of on-the-job-training on earnings (Hause, 1977), poverty dynamics (Lillard and Willis, 1978), wage dynamics and job turnover (Lillard, 1999), and the covariance structure of earnings *per se* (Lillard and Weiss, 1979; MaCurdy, 1982; Abowd and Card, 1989). The high quality of the data in these countries has allowed mobility researchers to even explore the dynamics of income variance (Meghir and Pistaferri, 2001), and the effects of measurement error on the estimated earnings mobility by means of validation data (Pischke, 1995).

For the case of developing countries the panorama is less positive. Most of the panel data for these countries have few observations over time, and hence many mobility studies are performed using two temporal observations per unit of analysis (see for instance Grootaert *et al.*, 1997; Fields *et al.*, 2003a,b).[8] In addition, the lack of validation studies in these countries makes

[7]In particular this question implies studying whether an individual converges to his own permanent level of earnings, as it will be seen below.

[8]Needless to say, this clearly limits the type of dynamic structures that can be esti-

hard to assess the extent of measurement error on the earnings variable, and its potential impact on the estimated mobility parameters. Finally, another issue that makes the mobility research harder to perform in these countries is the existence of high levels of attrition in the surveys collected. In spite of these difficulties, research on earnings mobility has continued to grow in the developing world.[9]

For the case of Mexico, the number of mobility studies addressing earnings dynamics is limited. Previous studies focusing on the evolution of incomes (or earnings) were made by means of comparable cross-sections and not with longitudinal data (see for instance Lustig and Székely (1999), and Cortés (2000)). Many of these studies focused on the evolution of poverty and inequality over time, but since they did not follow the same individuals they do not constitute mobility studies as such.[10]

It is important to mention that there is a set of studies on economic mobility in Mexico that analyzes other types of variables like education (Binder and Woodruff (2002), Dahan and Gaviria (1999), Behrman *et al.* (2001)), occupation (Latapí, 1992), industry of economic activity (Ibarlucea, 2003), and regional convergence in earnings (Aguayo-Tellez, 2005).

For the Mexican economy two recent studies have appeared dealing with issues very close in spirit to the present paper. The papers by Antman and Mckenzie (2005a) and Antman and Mckenzie (2005b) study earnings dynamics with the same data over similar periods of time. Since the ENEU consists of 1-year panels, and the authors are interested in studying mobility in the long-run, they create pseudo-panels in which specific age-education cohort groups are tracked over long periods of time. This method has advantages in extending the temporal coverage for mobility studies over many years, and it potentially helps to mitigate the problems of measurement error and attrition bias. However, this methodology makes strong assumptions that are problematic in practice. First, by tracking the mobility of a cohort they

mated.

[9]The expansion of this literature in the developing world can be witnessed by the fact that two major journals specializing in Development Economics have devoted entire issues to the topic (August 2000 issue of the *Journal of Development Studies*, and March 2003 issue of *World Development*). Further references are reviewed in Fields (2001) and Baulch and Hoddinott (2000).

[10]An exception to this claim are the papers on aggregate time dependence in economic positions by Wodon (2001) and Yitzhaki and Wodon (2002), the first comparing urban Mexico and Argentina, the second only in rural Mexico. Since these papers study aggregate mobility issues they are not reviewed in this paper.

miss the study of any intra-cohort mobility that might take place over time. Second, one cannot be sure that the mobility experienced by a cohort group actually represents the true mobility experienced by a given group of individuals. Issues like migration, deaths and household dissolution and creation might lead to incorrect inferences when this method is applied. As rightly pointed out by Deaton when discussing this methodology (otherwise strongly advocated by him) "(...) time series of cross sections can tell us about average earnings for the cohort over time, and it can tell us about inequality of earnings within the cohort and how it is changing over time, but it cannot tell us how long individuals are poor, or whether the people who are rich now were rich or poor at some earlier date" (Deaton, 1997, p.120).

In Antman and Mckenzie (2005a) the authors focus on mobility in household labor income. The authors are interested in studying whether there is unconditional convergence between the earnings of rich and poor households (what the authors call absolute convergence), and whether there is conditional convergence of the household's earnings to its own average level. Since they work with cohort average incomes, it is important to keep in mind that all their results correspond to such average variables. The authors find very little absolute convergence between rich and poor households, i.e., in general households keep their income levels over time. However, there is rapid conditional convergence, and it increases as time goes by.[11]

According to the authors, analyzing mobility over cohort averages gives them the advantage of solving the problems of measurement error and attrition bias commonly encountered in this type of studies. Regarding the measurement error problem, although it seems plausible that averaging the incomes of several households in a given cohort will tend to diminish the household idiosyncratic measurement error, this may not solve the overall measurement error problem if the households in a given cohort systematically misreport their earnings, e.g., if households with highly educated middle-aged heads systematically underreport their income. As a solution to the attrition problem, the authors use the first interview for each household (when there is no attrition) in the construction of their pseudo-panels. Their overall finding

[11]It is interesting to note that the authors also compare pseudo-panel quarterly mobility estimates to the ones stemming from true panels (following individuals instead of cohorts) and they find that their pseudo-panel results are surprisingly similar to the ones obtained through instrumental variable estimations (attempting to correct for measurement error) in the true panel. These estimates show much slower convergence than the ones obtained through Ordinary Least Squares.

is that there is no substantial difference in their convergence estimates for subsamples of attritors and non-attritors.

In a companion paper (Antman and Mckenzie, 2005b) the authors use the same pseudo-panels to study whether there are poverty traps in Mexico. More specifically they study the possibility of nonlinearities in household labor-income dynamics. Their finding is that there are no poverty traps for Mexican urban households.

Other studies analyzing the determinants of earnings mobility in Mexico are Maloney and Cunningham (2000), Maloney *et al.* (2004), and World Bank (2004). The main aim of this literature is studying vulnerability and the distribution of income shocks in Mexico. In particular, they ask which subgroups of the population are more "vulnerable" to income falls. They study what happens at different points of the *conditional* earnings mobility distribution, where the conditioning factors are a set of socioeconomic variables.

The periods covered by Maloney and Cunningham (2000) and Maloney *et al.* (2004) include before, during and after the 1994 Peso crisis, as well as 2000-2002. The data set used by these studies is again the ENEU. Among their main findings are that, holding everything else constant, the least educated and poor suffered slightly less in terms of earnings changes during the 1994 Peso crisis, but at the cost of having to put other members of the household in the labor market. They also find that if higher weights are attached to the income changes of poor households, the households with a less educated head present large losses, something interpreted by the authors as higher vulnerability. Finally, they find that the structure of the determinants of earnings changes is quite stable regardless of whether the economy is in recession or not. The main difference being that, during recessions, more educated households experience larger earnings losses, holding everything else constant.

It is important to stress that none of the conditional mobility estimations in these papers included the initial income level as an explanatory variable. Nevertheless, some evidence is provided for the relationship between household income change and a proxy for permanent income. The relationship they find between these two variables is negative during the 1994-95 recession and stronger than the one observed during the recovery period that followed afterwards.

One interesting analysis conducted in World Bank (2004) is the inclusion of rural households. This study incorporates results based on a recently created rural panel survey that complements the ENEU to form the new National Survey of Quarterly Employment (ENET). The period of analysis goes from 2000 to 2002. The results obtained with the ENET are compared to the ones from another rural panel generated to evaluate the PROGRESA poverty alleviation program. This last dataset covers the 1998-2000 period and, in contrast with the ENET, it contains information on consumption of the households. While the authors obtain similar results when comparing the urban and rural sub-samples of the ENET, they reach very different conclusions when analyzing consumption changes in the PROGRESA panel. In particular, the PROGRESA survey indicates that more disadvantaged households fared worse in terms of consumption mobility. Although these results contradict the findings for the rural part of the ENET, it is hard to know whether this is because income do not appraise the welfare of a group of individuals as fully as consumptions does, because of the different years compared in the two samples, or because of the way the sample is selected in the PROGRESA surveys.[12]

In the light of these previous studies, the contribution of the present paper to the previous mobility literature is to focus in the short-run earnings dynamics experienced by individuals in urban Mexico, with an emphasis on the role played by initial advantage on mobility. This paper also provides further evidence on the role played by socioeconomic factors determining mobility, and interprets the results within the framework of a structural model of earnings. The results obtained are analyzed over a long period of time, with varying macroeconomic conditions. Finally, the robustness of the findings to different measures of initial advantage, to measurement error and to attrition bias are explored. Some results similar to the ones here reported also appear in Fields *et al.* (2005).

4 Methodology

4.1 Unconditional Mobility

This section introduces the methodology used to analyze the relationship between mobility and initial advantage. Denote by y_{it} the earnings of individual i at period t, and its change by Δy_{it}, then in order to answer the

[12]These surveys only contain treatment and control communities associated with the PROGRESA program.

question: "Are the most advantaged individuals gaining more (losing less) in terms of earnings changes?", one natural place to start is by estimating the expected earnings changes given initial earnings, i.e., $E(\Delta y_{it}|y_{it-1})$. The simplest assumption to make about this conditional expectation is that it is linear, i.e.,

$$\Delta y_{it} = \beta_0 + \beta_1 y_{it-1} + u_{it} \tag{1}$$

Under these assumptions the answer to the previous question will depend on the sign of the β_1 parameter. More specifically, a positive β_1 means that the individuals at the top of the initial earnings distribution have more positive (or less negative) earnings changes, i.e., the rich got richer. On the contrary, if β_1 is negative then there is convergence between the individuals at the bottom and the ones at the top of such distribution, i.e., the least advantaged gain the most (lose the less). Finally, if β_1 equals zero then earnings mobility is not affected by initial earnings, and mobility depends only on the constant β_0 and the random factors captured by u_{it}. Since these random factors average to zero, a $\beta_1 = 0$ means that on average everybody experiences the same mobility β_0. In other words, there will be divergence in earnings if $\beta_1 > 0$, there will be convergence if $\beta_1 < 0$, and the earnings changes patterns will be parallel if $\beta_1 = 0$.

The relationship stated in equation (1) can be estimated by Least Squares (LS) for earnings both in levels and logarithms. The estimation in levels gives a measure of the convergence in pesos, while the logarithmic specification estimates the amount of log-convergence, which gives a larger weight to the mobility of poorer individuals and approximates the proportional mobility by level of initial earnings.

Since there are many overlapping short-run panels over which to estimate this relationship, there will be several LS estimates of the β_1 parameter, one for each period (i.e., there will be many β_{1t}'s). With these many β_{1t}'s it is possible to track the evolution of convergence over time, and across varying macroeconomic conditions.[13]

The interpretation of the previous β_1 parameters is an issue that deserves further discussion. Even if it were observed that earnings converge (i.e., $\beta_1 < 0$) it is not evident what meaning should be attached to this finding. A negative β_1 could be the product of reversion to the mean resulting from

[13] A median regression was also estimated but the results were similar to the ones of the LS analysis. For this reason those results are not included in this paper.

adjustments in earnings to a temporary shock. For instance, it is possible that individuals who reported having low (high) earnings in the base period were temporarily unlucky (lucky) and that the positive (negative) mobility observed for them is just an adjustment back to their permanent earnings level.[14] A negative β_1 could also mean that individuals at the bottom are truly faring better by experiencing gains that will continue in the future. Without panel data that extends its coverage for several years these two scenarios cannot be distinguished by just analyzing the sign of β_1.

As previously mentioned, issues of mobility in the long-run cannot be analyzed with the data at hand. However, measures of permanent advantage can be generated and used to analyze their relationship with mobility in the short-run. In particular, a regression similar to (1) can be estimated using a proxy of permanent advantage as the independent variable. In this paper, this predicted permanent advantage measure $\hat{y}_{it-1}$ is formed in two ways: first, by averaging the earnings of the individual using all the quarters of information available (instead of using earnings from the first interview only), and second, by instrumenting the permanent component of initial earnings in eq. (1) with variables that would be good predictors of the permanent advantage of an individual. The instruments used include human capital variables like age, education, gender, and wealth proxies like cluster average earnings, and dwelling characteristics. Under these two methods new β_{1t}^{P}'s are obtained.

4.2 Conditional Mobility and the Socioeconomic Determinants of Mobility

This section presents the methods used to analyze the second set of questions: "What is the *ceteris paribus* impact of socioeconomic characteristics of the individual on earnings mobility?" and "How do these factors affect the impact of initial earnings on mobility?".

Broadly speaking, the socioeconomic characteristics of an individual can be grouped in time-invariant characteristics Z_i and time-variant characteristics X_{it-1} and X_{it}. The time-invariant characteristics include gender, age (linear and squared), education (linear and squared), and regional dichotomous variables. The time-variant variables refer to sector of employment, meaning formal wage work, formal self-employment, informal wage work,

[14]Further empirical evidence of whether this is happening will be provided below.

informal self-employment, and unemployment.

In order to be able to interpret the results of the conditional mobility estimations within a structural framework, it is useful to start by specifying an earnings equation and from there derive a mobility equation. A natural starting point is to allow earnings at time t to be affected by all the factors listed under Z_i and X_{it}. That means that earnings are determined by age, gender, education level, region, and sector of employment.

Whether this constitutes truly exogenous "determination" or not, is a matter of debate. Variables like sector of employment and region are potentially endogenous to the earnings determination process, since an individual could choose which sector to work in, or which region to migrate to depending on his earnings mobility. Keeping this caveat in mind, this section will proceed by treating these variables as pre-determined, and will ignore the potential complications that arise due to these issues.[15] Finally, since no attempt will be made to correct for the potential self-selection of individuals into the labor force, all the results should be considered to apply only for the subpopulation of individuals participating in the labor force both in the initial and the final periods.

The basic specification of the earnings equation is[16]

$$y_{it} = Z_i\gamma_t + X_{it}\kappa_t + \varepsilon_{it} \tag{2}$$

where the error term ε_{it} is independent of Z_i and X_{it} and autocorrelated, i.e.,

$$\varepsilon_{it} = \rho_t\varepsilon_{it-1} + \eta_{it}$$

$$\eta_{it} \perp X_{it}, Z_i \qquad\qquad \eta_{it} \sim (0, \sigma_\eta^2) \qquad \forall i, t$$

This equation states that earnings at time t depend on the time-invariant characteristics Z, time-variant characteristics X at time t, and an error term ε that captures shocks to earnings. The effects of past values of the time-variant characteristics and of the shocks are assumed to enter only through the current values of these factors. This equation provides a rationale for why initial earnings would affect earnings changes, even after conditioning for socioeconomic characteristics. In particular, the AR(1) structure assumed

[15]In a companion paper Duval-Hernández (2006) I study more closely the issue of sector selection, in particular whether individuals are free to choose among sectors and the implications for earnings mobility.

[16]A similar model was used in Fields *et al.* (2003a)

for the ε term ensures that y_{it-1} has an impact on earnings mobility. To see why note that the earnings changes implied by (2) are

$$\Delta y_{it} = Z_i(\Delta\gamma_t) + (\Delta X_{it})\kappa_t + X_{it-1}(\Delta\kappa_t) + ((\rho_t - 1)\varepsilon_{it-1} + \eta_{it})$$

hence substituting $\varepsilon_{it-1} = y_{it-1} - Z_i\gamma_{t-1} - X_{it-1}\kappa_{t-1}$ into this expression leads to

$$\Delta y_{it} = (\rho_t - 1)y_{it-1} + Z_i\tilde{\gamma}_t + (\Delta X_{it})\kappa_t + X_{it-1}\tilde{\kappa}_t + \eta_{it} \tag{3}$$

where $\tilde{\gamma}_t = \gamma_t - \rho_t\gamma_{t-1}$ and $\tilde{\kappa}_t = \kappa_t - \rho_t\kappa_{t-1}$. Therefore, under this model, the effect of initial earnings y_{it-1} on earnings mobility after conditioning a on a set of socioeconomic variables comes from the autocorrelation of the unobserved error component.[17]

In the present application of the model described by eq. (3), the only time-varying variables will be dichotomous variables indicating the sector of employment, as a result, a slightly modified version of this equation is estimated. In particular, denote by $st(l, m)$ a dichotomous variable that takes value 1 if the individual transited from sector l to sector m, and zero otherwise, and let $\pi_{lm} = \kappa_t(m) - \rho_t\kappa_{t-1}(l)$, where $\kappa_t(j)$ is the *j-th* element of the vector κ_t, i.e., is the parameter for the sector j in the earnings equation (2). Under this notation, the term $(\Delta X_{it})\kappa_t + X_{it-1}\tilde{\kappa}_t$ equals $\sum_l \sum_m st(l, m)\pi_{lm}$, hence the conditional mobility equation (3) can be rewritten as

$$\Delta y_{it} = (\rho_t - 1)y_{it-1} + Z_i\tilde{\gamma}_t + \sum_l \sum_m st(l, m)\pi_{lm} + \eta_{it}. \tag{4}$$

This is the equation that will be estimated.

This model subsumes the partial adjustment model as a particular case. If the steady state earnings of an individual are defined as

$$y_i^s = Z_i\gamma + X_i\kappa$$

and if $X_{it} = X_i, \kappa_t = \kappa, \gamma_t = \gamma, \rho_t = \rho$, then equation (4) can be rewritten as

$$\Delta y_{it} = (1 - \rho)(y_i^s - y_{it-1}) + \eta_{it}$$

[17]A richer version of the model expressed by (2) and (3) would allow for the presence of individual unobserved time-invariant effects, δ_i. As previously explained, the present paper focuses on yearly mobility, of which only one observation per individual is available. Hence, it is not possible to estimate the parameters in eq. (3) conditional on this δ_i. The reader should keep this limitation in mind when analyzing the estimates of this equation.

where the parameter ρ is adjustment coefficient of earnings to its steady-state. In particular, if the variables grouped under X and Z constitute the determinants of steady-state earnings, the influence of y_{it-1} on earnings mobility comes from the adjustment of earnings to its steady-state level.

Equation (4) is estimated by (robust) LS for changes in earnings and log-earnings. Other specifications estimated included a model with time-invariant regressors only, and a model with varying ρ_t's for different groups of the population. These specifications together with the base specification were also estimated by a median regression. Since similar conclusions were reached under the alternative specifications, only the results pertaining to the base specification are included in this paper. The interested reader can refer to Duval Hernández (2006) for more details.

Since the methods described in this section involve the comparison of large amounts of results, the presentation of such results will be done by graphing the coefficient of y_{it-1}, i.e., $(\rho_t - 1)$ for each period, and the full set of regression results will be presented only for the data grouped under 3 pooled periods. The pooled periods are Q1:87-Q2:93, Q3:94-Q1:99, and Q2:99-Q4:02.

5 Results

5.1 Unconditional Mobility

This section presents the results that pertain to the unconditional mobility analysis as described in section 4.1. In particular, it presents the estimates for the parameter β_1 from equation (1), i.e.,

$$\Delta y_{it} = \beta_0 + \beta_1 y_{it-1} + u_{it}.$$

Figure 3 plots β_1 obtained by Least Squares, both for earnings in levels and logarithms. The graphs show unanimously that there is convergence between the earnings of rich and poor. That is, over a calendar year the initially poor got richer, and the initially rich got poorer. The graphs of the parameters do not show a specific trend or pattern of this convergence over time.[18]

[18] If anything, the graph of log-earnings has an increasing concave shape.

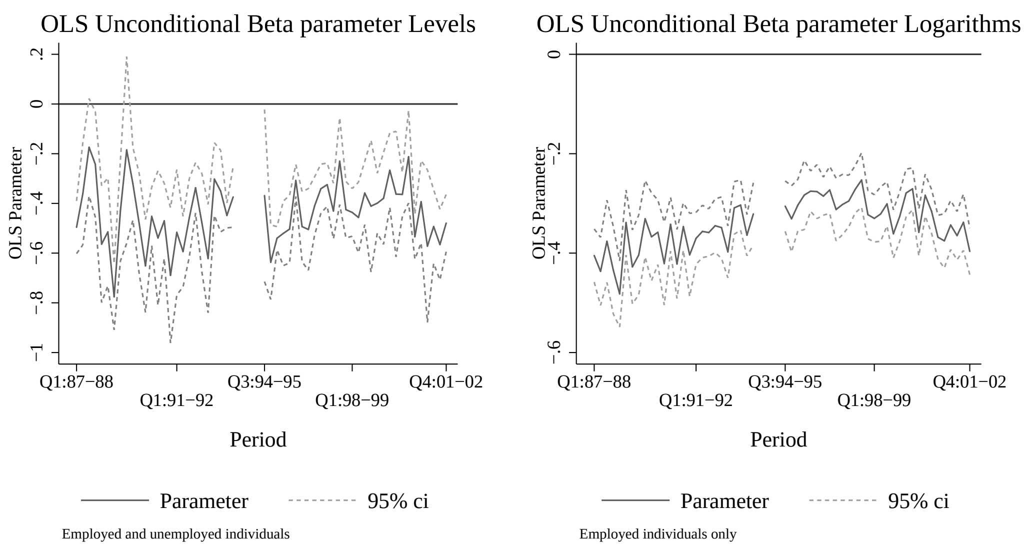


Figure 3: OLS Unconditional Mobility Parameters

As previously mentioned, other specifications of this relationship were estimated. In particular, equation 1 was estimated by OLS excluding individuals unemployed at the initial or final period. Also using the full sample, median regressions were estimated to obtain estimates that are not affected by the presence of outliers in the data. All these exercises led to the same result here reported, i.e. convergence between the earnings of initially rich and poor.[19]

Although the finding of convergence in earnings between rich and poor over a calendar year seems clear (leaving aside issues of measurement error), it is not evident how to interpret this result. On one side it could mean that low-earners are catching up with high-earners; but, as previously mentioned, this could also be the product of an adjustment to a temporary shock in earnings, without any permanent approaching between rich and poor. Evidence supporting this second interpretation is presented in Figure 4.

The graphs in Figure 4 plot the average earnings profiles for individuals classified at different points in time into quintiles of the earnings distribution. They show that in the quarter in which the quintile classification takes place, the earnings of the individuals in the lowest quintile are considerably lower than at any other period. Similarly, in this period the average earnings of the individuals in the top quintile appear to be considerably larger than what they usually are. In other words, classifying individuals as rich and poor based on the earnings of a single period exacerbates their apparent advantage or deprivation (depending on the case). The implication of this finding for the present unconditional mobility estimations is that when regressing Δy_{it} on initial earnings, part of the convergence obtained reflects the adjustment of earnings back to their "regular" level, and not necessarily convergence between these earnings profiles.[20]

In order to capture the relationship between mobility and "permanent" advantage, regressions like (1) are estimated now using measures of permanent advantage as a regressor. In particular, two measures of permanent advantage are considered. The first one is average earnings over time, for each individual. The second one is constructed by instrumenting the permanent component of initial earnings using variables that are related to the permanent advantage of an individual like age, education, gender, wealth proxies

[19]For more details refer to Duval Hernández (2006).

[20]Although this graph corresponds to one of the last panels in the sample, similar plots for other years lead to the same conclusion.

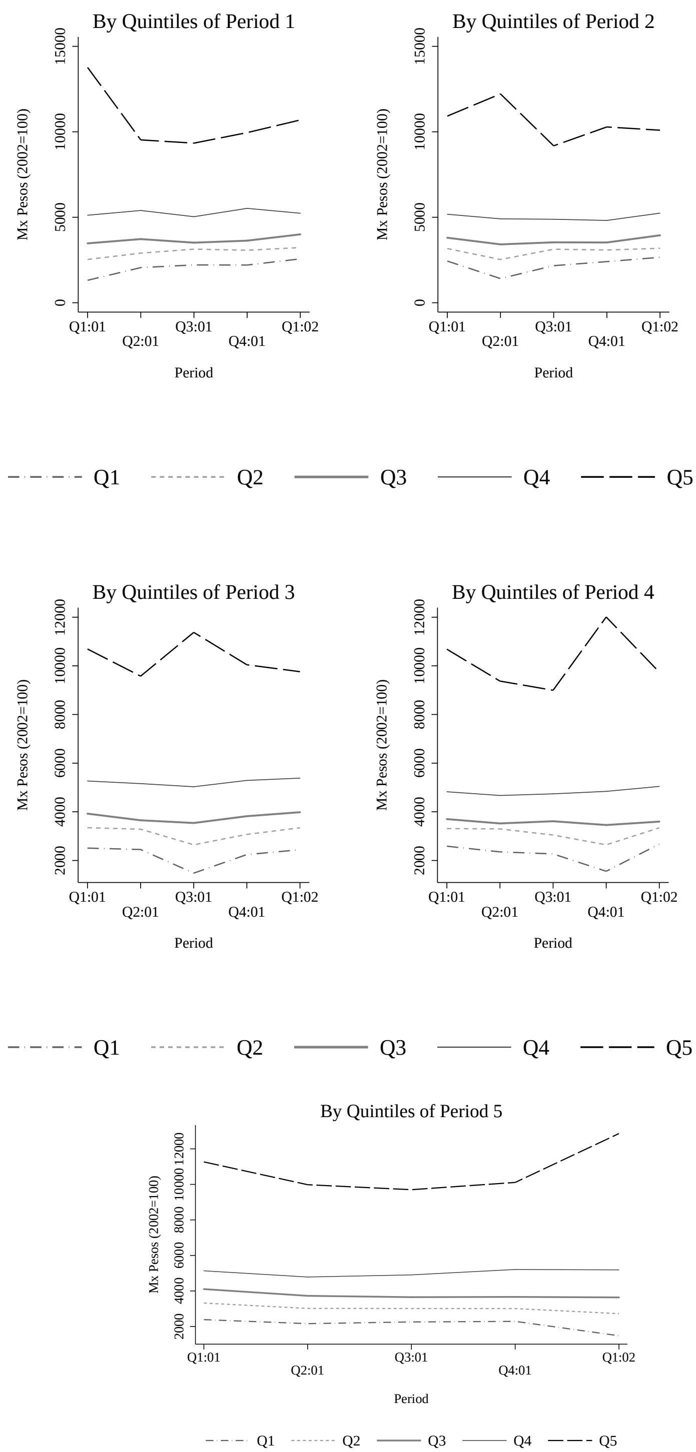


Figure 4: Earnings Profiles by Quintile Groups Classified at Different Periods

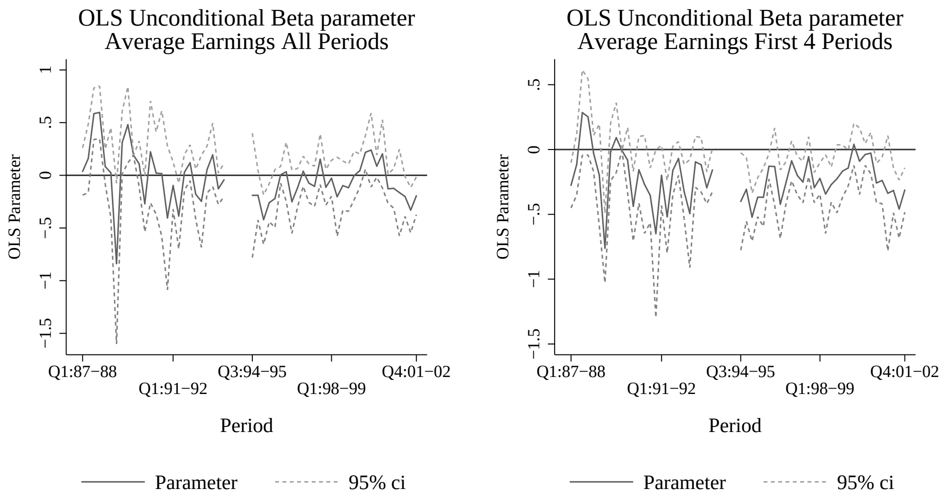


Figure 5: Unconditional Mobility. Average Earnings as Permanent Advantage.

and sometimes sector of employment. The results of these estimations are plotted in Figures 5 to 7.

Figure 5 contains the graphs where average earnings is used as a measure of permanent advantage. Here two exercises are performed, one averaging earnings over the full 5 quarters of observations and the second averaging earnings over the first 4 quarters only. Both estimations are performed only for earnings in levels. Figure 6 contains the estimates where permanent advantage is instrumented with human capital variables and wealth proxies. Finally, Figure 7 plots the parameter estimates when permanent advantage is instrumented with the aforementioned regressors plus sector dichotomous variables. These predictions are made both for earnings in levels and log-earnings.

All these figures show that yearly mobility is unrelated to the generated measures of permanent advantage for most of the years in the sample. The only exception to this finding occurs during the second half of the nineties, especially right after the 1994 Peso crisis in the IV regressions with earnings in levels. In this case there is convergence in earnings. One interesting point

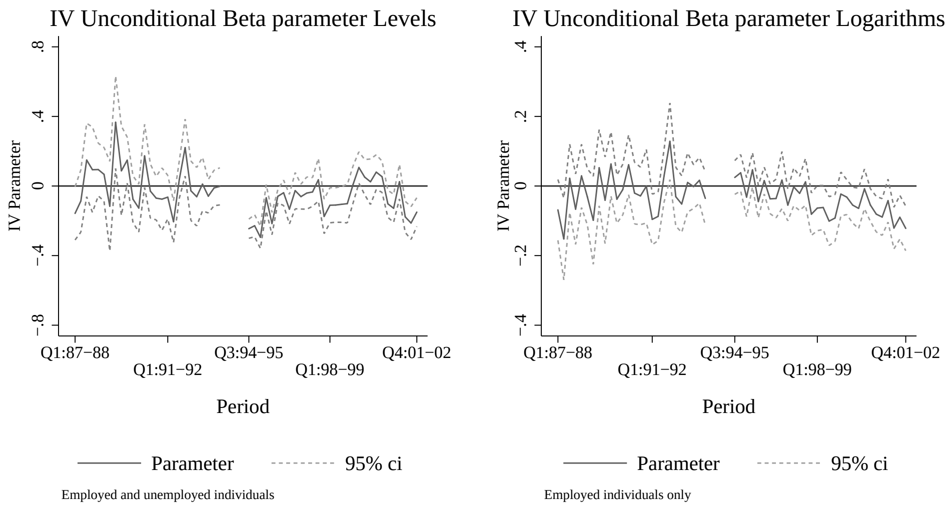


Figure 6: IV estimates with Human Capital and Wealth Proxies as instruments of Permanent Advantage.

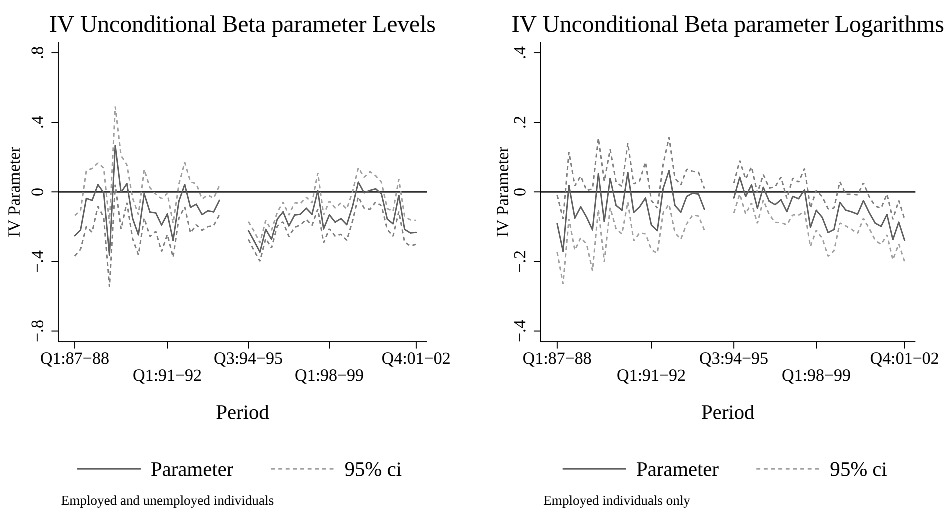


Figure 7: IV estimates with Human Capital, Wealth Proxies and Sector as instruments of Permanent Advantage.

is that the lack of *log*-convergence after the Peso crisis implies that in this episode the "permanent" rich lost more than the "permanent" poor, but their losses were proportional to their higher levels of earnings.

Summarizing, these findings confirm that the strong convergence obtained when using reported earnings as a measure of initial advantage was mostly due to a short-run adjustment of earnings back to their permanent level. In other words, the mobility over a year did not alter the permanent advantage of the individuals in the economy. The only exception to this occurred in the aftermath of the 1994 Peso crisis. This crisis brought proportional losses to everybody in the economy, making the richer individuals lose more than anybody else in absolute terms.

5.2 Conditional Mobility and the Determinants of Earnings Changes

To start the presentation of the results on conditional mobility the parameter $\rho_t - 1$ from eq. (4), i.e.,

$$\Delta y_{it} = (\rho_t - 1)y_{it-1} + Z_i\tilde{\gamma}_t + \sum_l \sum_m st(l,m)\pi_{lm} + \eta_{it}$$

is plotted for several specifications.

Figure 8 shows this parameter when the included controls are only time-invariant variables Z_i that capture human capital characteristics like age, education and gender, plus regional control dummies. As it can be appreciated, there is always convergence to the conditional mean, and this convergence is slightly stronger than the unconditional convergence presented in the previous section. This means that the overall effect of the human capital and regional controls is to generate divergence in earnings, so that once these socioeconomic variables are explicitly accounted for, the convergence in earnings is stronger. Also it can be seen that this parameter, which is around -0.6 would imply a value for ρ of about 0.4, i.e., the auto-regression parameter in transitory earnings is about 0.4.

These estimates are complemented by estimating the same conditional convergence parameter now including sector transition dummies as additional controls. As it can be seen from Figure 9 the conditional mobility parameters are very similar to the ones of Figure 8, hence, sector transition controls do

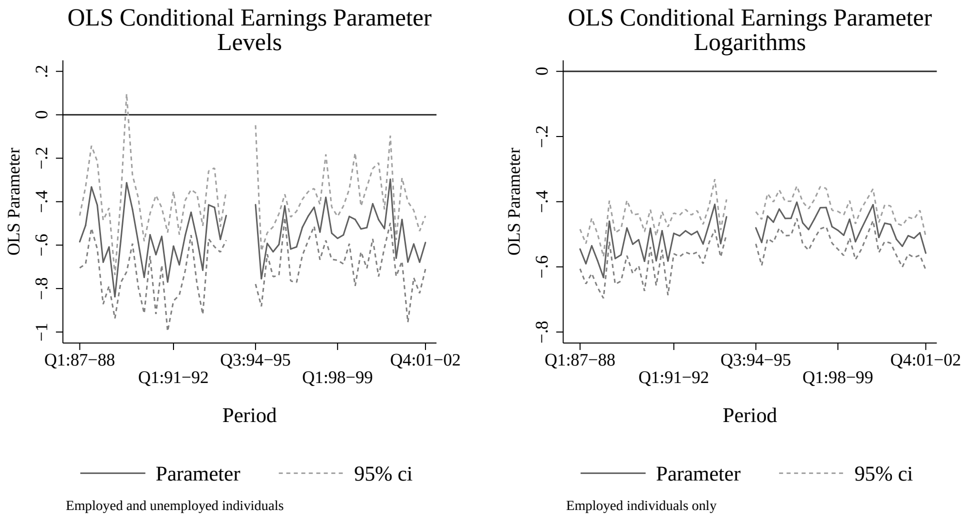


Figure 8: Conditional Mobility Parameter. Human Capital and Regional Controls.

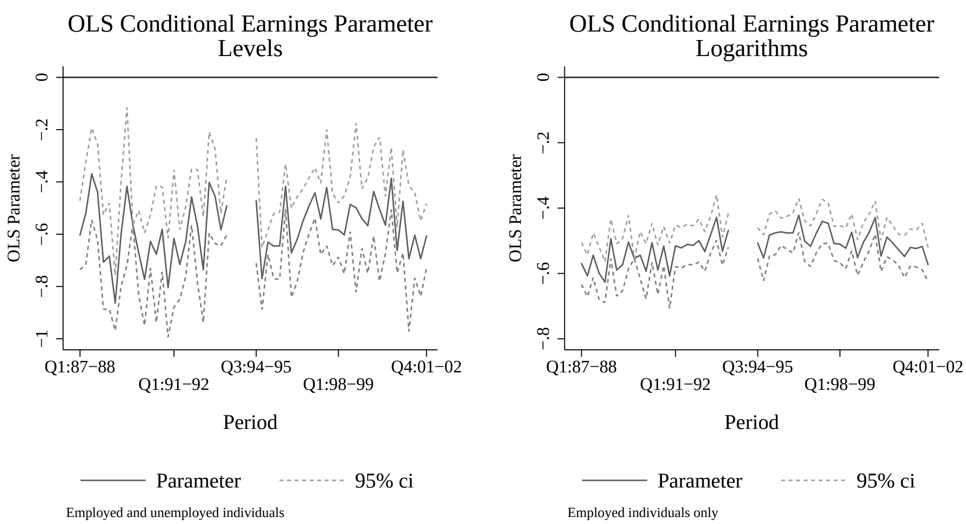


Figure 9: Conditional Mobility Parameter. Human Capital, Regional and Sector Transition Controls.

not seem to affect much the conditional convergence rates.[21]

In order to present the direct effects of socioeconomic variables on earnings changes, the data is again pooled by subperiods in the sample. The results from these regressions are shown in Tables 2 and 3.

The results for the regressions in levels show that age increases mobility, but at a decreasing rate; however, the positive effect of age has diminished over time. Education on the contrary has a convex pattern. From 0 to about 5 years of education an extra year of education reduces earnings mobility, but after that point it increases it. However, as time goes by the inflection point at which the positive effect on mobility kicks in is located at higher levels of education, i.e., the negative effect of education on mobility for individuals with low education has become more pervasive.[22]

Being a male has a large positive impact on mobility in all periods, and under all the specifications. However, while in the specification in levels the effect appears to follow the cycle of average earnings (highest in the 87-93 period, lowest in the 95-99, otherwise in the middle); in the specification in logarithms this positive effect has become stronger over the years.

Out of all the sector transitions, the one into formal self-employment is always associated with the largest gains, after controlling for everything else. This could be generated by the potential inclusion of capital gains in the earnings reported by the self-employed. Aside from movements into unemployment (which trivially involve losses), the most negative conditional mobility is associated with transitions into informal wage work. The other destination sectors (Formal wage work and informal self-employment) are between the two previous extremes. In general, the transitions into informal self-employment bring more upward conditional mobility than the ones into formal wage work, but this effect is sometimes reversed during the aftermath of the 1994 Peso crisis.[23]

[21]Equation (4) was estimated allowing ρ to vary for different subgroups of the population. Out of all the interactions estimated, only education and sector groups seem to have some noticeable differences in their convergence rates. In the case of education groups, the higher the education the smaller the convergence rates. In the case of sector groups, the formal wage workers present the lowest convergence rates, while the formal self-employed exhibit the highest ones, although their convergence rates also fluctuate more.

[22]A similar conclusion applies to the regression in logarithms, while in the first period going from 1987 to 1993 education had an increasing convex effect on log-mobility, after 1999 a negative effect for low educated people appeared.

[23]It is important to stress again that these parameter estimates of sector transitions just reflect the conditional earnings changes experienced by movers and stayers, and they

Finally, in what concerns the regional analysis the cities along the US Border and in the North experience higher conditional mobility, while the Center and the South exhibit less positive conditional mobility. The omitted region is Mexico City. It is worth mentioning that the positive effect on mobility of living in a US Border city has become stronger over time. Whether this is related to the increasing activity of "maquiladoras" (American assembly factories that benefit from the comparatively cheap labor across the border) is something that requires further study. The results for the regression in logarithms show a similar pattern to the one above described.

need not reflect the counterfactual gains a randomly selected individual would experience by moving from sector x into sector y.

Table 2: OLS Regression. Levels. Dep. Var.: Change in Reported Earnings

	Q1:87-Q2:94		Q3:94-Q1:99		Q2:99-Q4:02	
	Coef./(s.e.)		Coef./(s.e.)		Coef./(s.e.)	
Initial Earnings	-0.621	***	-0.571	***	-0.587	***
	(0.03)		(0.03)		(0.04)	
Age						
Linear	194.290	***	92.974	***	66.527	**
	(29.20)		(25.14)		(32.39)	
Squared	-2.099	***	-0.928	***	-0.613	
	(0.36)		(0.32)		(0.41)	
Education						
Linear	-91.764	***	-148.619	***	-181.971	***
	(21.02)		(22.76)		(28.00)	
Squared	19.135	***	18.146	***	21.001	***
	(1.38)		(1.52)		(1.87)	
Male	968.224	***	597.598	***	780.774	***
	(54.73)		(43.76)		(71.22)	
Sector Transitions						
Unemployed to Informal Worker	4111.114	***	2499.281	***	3030.385	***
	(272.40)		(143.01)		(280.08)	
Unemployed to Informal Self-employed	5060.229	***	3045.929	***	5299.060	***
	(393.15)		(240.27)		(767.00)	
Unemployed to Formal Self-employed	14615.738	*	22656.096	***	7423.921	***
	(7786.13)		(3891.63)		(394.66)	
Unemployed to Formal Worker	4237.511	***	3695.184	***	4785.051	***
	(268.05)		(207.29)		(364.79)	
Informal Worker to Unemployed	-8.605		-379.147	*	265.666	
	(288.87)		(203.14)		(282.29)	
Informal Worker to Informal Worker	3104.463	***	1957.628	***	2824.087	***
	(257.70)		(143.53)		(265.59)	
Informal Worker to Informal Self-employed	4017.568	***	2533.454	***	3384.354	***
	(281.09)		(164.19)		(277.84)	
Informal Worker to Formal Self-employed	7408.207	***	7122.204	***	8136.863	***
	(1112.99)		(2171.75)		(2585.46)	
Informal Worker to Formal Worker	3484.269	***	2199.401	***	3143.540	***
	(262.05)		(146.14)		(275.63)	
Informal Self-employed to Unemployed	-1213.697	***	-1206.367	***	-939.647	**
	(369.46)		(260.78)		(424.68)	
Informal Self-employed to Informal Worker	2607.156	***	1474.242	***	2260.800	***
	(257.33)		(165.95)		(292.77)	
Informal Self-employed to Informal Self-employed	3905.512	***	2368.119	***	3319.076	***
	(272.77)		(175.49)		(297.95)	
Informal Self-employed to Formal Self-employed	9982.576	***	6168.406	***	8933.459	***
	(1436.25)		(1189.88)		(1020.36)	
Informal Self-employed to Formal Worker	2969.937	***	1977.717	***	3138.862	***
	(289.38)		(195.10)		(316.64)	

Table 2 (Continued)

Formal Self-employed to Unemployed	-7776.634	***	-4720.922	***	-3698.514	***
	(855.26)		(736.62)		(507.12)	
Formal Self-employed to Informal Worker	2590.661	*	448.713		807.239	
	(1404.30)		(675.60)		(1852.53)	
Formal Self-employed to Informal Self-employed	5802.505	***	2658.578	***	4711.537	***
	(848.51)		(660.96)		(1575.82)	
Formal Self-employed to Formal Self-employed	12097.630	***	10537.072	***	12434.895	***
	(1192.00)		(1703.91)		(2141.84)	
Formal Self-employed to Formal Worker	5827.793	***	5278.981	***	2437.215	**
	(1076.09)		(1523.93)		(1051.56)	
Formal Worker to Unemployed	-1576.436	***	-1940.113	***	-2161.025	***
	(328.03)		(237.81)		(427.38)	
Formal Worker to Informal Worker	3037.783	***	1848.114	***	2779.085	***
	(257.88)		(152.20)		(275.47)	
Formal Worker to Informal Self-employed	4406.599	***	2244.568	***	3726.094	***
	(313.88)		(192.18)		(340.96)	
Formal Worker to Formal Self-employed	8718.413	***	7992.312	***	9208.727	***
	(1282.34)		(1268.39)		(1670.31)	
Formal Worker to Formal Worker	3558.109	***	2541.005	***	3418.881	***
	(253.50)		(160.34)		(287.57)	
Region						
Mexico City (omitted)						
US Border	548.806	***	608.806	***	727.502	***
	(84.88)		(79.31)		(117.82)	
North	85.365		109.581		276.262	***
	(76.47)		(67.14)		(91.18)	
Center	-79.721		-191.689	***	-18.502	
	(57.41)		(48.92)		(73.89)	
South	-464.985	***	-361.778	***	-467.909	***
	(85.22)		(67.79)		(109.58)	
Constant	-6545.706	***	-3419.910	***	-3433.571	***
	(645.21)		(516.08)		(687.17)	
R-squared	0.333		0.376		0.331	
N	98327		83112		55415	

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 3: OLS Regression. Dep. Var.: Change in Reported Log-Earnings

	Q1:87-Q2:94 Coef./(s.e.)		Q3:94-Q1:99 Coef./(s.e.)		Q2:99-Q4:02 Coef./(s.e.)	
Initial Log-earnings	-0.532	***	-0.510	***	-0.512	***
	(0.01)		(0.01)		(0.01)	
Age						
Linear	0.025	***	0.023	***	0.015	***
	(0.00)		(0.00)		(0.00)	
Squared	-0.000	***	-0.000	***	-0.000	***
	(0.00)		(0.00)		(0.00)	
Education						
Linear	0.007	***	0.003		-0.009	***
	(0.00)		(0.00)		(0.00)	
Squared	0.001	***	0.002	***	0.002	***
	(0.00)		(0.00)		(0.00)	
Male	0.148	***	0.152	***	0.156	***
	(0.01)		(0.01)		(0.01)	
Sector Transitions						
Informal Worker to Informal Self-employed	0.205	***	0.173	***	0.148	***
	(0.03)		(0.03)		(0.03)	
Informal Worker to Formal Self-employed	0.818	***	0.985	***	0.847	***
	(0.20)		(0.22)		(0.13)	
Informal Worker to Formal Worker	0.140	***	0.163	***	0.155	***
	(0.02)		(0.02)		(0.02)	
Informal Self-employed to Informal Worker	-0.064	**	-0.125	***	-0.148	***
	(0.03)		(0.03)		(0.03)	
Informal Self-employed to Informal Self-employed	0.130	***	0.105	***	0.075	***
	(0.02)		(0.02)		(0.02)	
Informal Self-employed to Formal Self-employed	0.583	***	0.667	***	0.628	***
	(0.06)		(0.15)		(0.06)	
Informal Self-employed to Formal Worker	0.090	***	0.145	***	0.133	***
	(0.03)		(0.02)		(0.03)	
Formal Self-employed to Informal Worker	-0.010		-0.222		-0.617	
	(0.13)		(0.17)		(0.44)	
Formal Self-employed to Informal Self-employed	0.259	***	0.231	***	0.222	**
	(0.05)		(0.05)		(0.10)	
Formal Self-employed to Formal Self-employed	0.665	***	0.739	***	0.631	***
	(0.05)		(0.06)		(0.08)	
Formal Self-employed to Formal Worker	0.281	***	0.342	***	0.054	
	(0.06)		(0.09)		(0.09)	
Formal Worker to Informal Worker	-0.001		-0.016		-0.015	
	(0.02)		(0.02)		(0.03)	
Formal Worker to Informal Self-employed	0.192	***	0.065	**	0.140	***
	(0.03)		(0.03)		(0.03)	
Formal Worker to Formal Self-employed	0.532	***	0.674	***	0.649	***
	(0.08)		(0.09)		(0.12)	
Formal Worker to Formal Worker	0.140	***	0.219	***	0.166	***
	(0.02)		(0.01)		(0.01)	

Table 3 (Continued)

Region						
Mexico City (omitted)						
US Border	0.119	***	0.167	***	0.146	***
	(0.01)		(0.01)		(0.01)	
North	0.018	**	0.020	**	0.064	***
	(0.01)		(0.01)		(0.01)	
Center	0.014	*	-0.018	**	0.016	*
	(0.01)		(0.01)		(0.01)	
South	-0.060	***	-0.084	***	-0.104	***
	(0.01)		(0.01)		(0.01)	
Constant	3.397	***	3.068	***	3.470	***
	(0.08)		(0.07)		(0.09)	
R-squared	0.269		0.286		0.279	
N	95607		79650		53854	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

So far the results presented have assumed that the earnings variable is measured without error, or more precisely that earnings are correctly reported. It also assumed that the individuals that disappear from the sample or that do not report their earnings are doing so at random. Both assumptions are unrealistic and require further scrutiny. These issues are tackled in the next sections.

6 Robustness checks

Two issues that concern many researchers studying mobility are the presence of measurement error in the earnings variable and attrition bias. Errors or misreports of earnings can lead to serious biases in the estimation of the coefficients in equations (1) and (4). It could even be the case that initial earnings have no effect on mobility, and still a relationship is found due to the correlations of the measurement error terms. On the other hand, the existence of attrition (and non-reporting) leads to problems in identifying the conditional expectations of interest, since the dependent variable will not be observed for a fraction of the population. This section presents methods to assess the possible impacts of these two problems, one at a time.

6.1 Measurement Error

6.1.1 Theory

Until recently, it was usually assumed that measurement error of economic variables was always of the classical variety, i.e., the measurement error was assumed to be an iid term, uncorrelated with the true value of the variable of interest, with any other variable in the model, with the error term in the equation of interest, and with any other measurement error in any other variable. Although this model is analytically convenient, enough evidence has accumulated over the past decade showing that this assumption does not hold in general for the earnings variable used in labor studies (see Bound and Krueger, 1991; Bound *et al.*, 1994; Pischke, 1995; Bound *et al.*, 2001). Individuals tend to report what they "usually" earn, and not necessarily the exact earnings they had in a specific period. Also, rich individuals might underreport their earnings out of fear that the survey will be used for tax purposes.[24] Similarly, individuals at the bottom of the earnings distribution might overstate their incomes out of embarrassment. Unfortunately, for the case of Mexico there are no validation studies that allow testing the nature of this potential problem.

Given this data limitation, the approach taken here is to follow the measurement error model proposed in Bound *et al.* (1994) and extended by Pischke (1995), in order to show some implications of this model for the mobility estimations performed in this paper.[25] The main caveat of proceeding this way is that the measurement error evidence on which this section relies, comes from a validation study performed on a single Detroit firm in the mid-eighties.[26] The earnings measure in that study is annual earnings coming both from employer records and the answers to a PSID questionnaire applied to a sample of workers in that firm.

Clearly, using a validation study for a single US firm is far from satisfactory (although a similar model described well the measurement error process in the more representative CPS sample when compared to Social Security records, see Bound and Krueger (1991)). Also, in the case of Mexico the ENEU reports monthly earnings, instead of yearly earnings. This earnings variable is constructed by allowing the respondent to choose a preferred time framework (day, week, month, etc.) and to report their earnings during that period. After that, the interviewer performs whatever conversion is necessary to transform that report into monthly earnings. Although this scheme reduces the error due to bad recall of true earnings, it makes the PSID model less applicable to the Mexican case.[27]

The measurement error model proposed in Bound *et al.* (1994) is one where the measurement error has mean zero and is "mean reverting", i.e., it is negatively related to the true value of earnings. Later on, Pischke (1995) studied more carefully the same validation survey and the relationship between measurement error and earnings dynamics. He proposed a slightly different version of the Bound *et al.* (1994) measurement error model, in

[24]In the case of Mexico, it seems more plausible that such individuals would underreport their earnings out of fear for their personal safety.

[25]A similar route was adopted before by Fields *et al.* (2003a) following a variant of the model proposed by Bound *et al.* (1994).

[26]The validation study is the PSID Validation Study, for a description see Duncan and Hill (1985) and Duncan and Mathiowetz (1985).

[27]This way of recording earnings information can also generate biases for the individuals who don't have a regularly paid salary over a month.

which the mean reverting measurement error term applied only to the transitory component of earnings, i.e., when asked about their earnings for the preceding year individuals tended to report their usual earnings.

Based on the previous insights, the following measurement error model is assumed in this paper. Let y_{it} be reported earnings, y^*_{it} be the true value of current earnings and μ_{it} be the measurement error. Then

$$y_{it} = y^*_{it} + \mu_{it}. \tag{5}$$

True earnings are assumed to be the sum of two orthogonal components, permanent earnings, y^P_{it}, and transitory earnings, which in order to follow the notation established in section 4.2 will be denoted by ε_{it},[28]

$$y^*_{it} = y^P_{it} + \varepsilon_{it} \qquad\qquad y^P_{it} \perp \varepsilon_{it} \tag{6}$$

Furthermore, the measurement error is assumed to be linearly related to true earnings according to the following equation[29]

$$\mu_{it} = \alpha(y^*_{it} - y^P_{it}) + \zeta_{it} \qquad\qquad \alpha < 0 \tag{7}$$

where the term ζ_{it} is the idiosyncratic component of measurement error which has mean zero, finite variance σ^2_ζ, and it is uncorrelated with true earnings, but it can be autocorrelated. In particular, ζ_{it} is assumed to follow an AR(1) process, i.e.,

$$\zeta_{it} = \theta\zeta_{it-1} + \omega_{it} \qquad\qquad \omega_{it} \sim iid(0, \sigma^2_\omega) \tag{8}$$

If the measurement error follows the previous structure, then it can be shown that the OLS estimate of β_1 in eq. (1) will be given by

$$\hat{\beta}_1 = \beta_1\frac{V(y^*_{it-1})}{V(y_{it-1})} + (\rho - 1)\frac{\alpha(2+\alpha)V(\varepsilon_{it-1})}{V(y_{it-1})} + (\theta - 1)\frac{V(\zeta_{it-1})}{V(y_{it-1})} \tag{9}$$

as shown in the appendix.[30]

[28]Note that the term denoting permanent earnings y^P_{it} is not necessarily constant over time, making it consistent with the interpretation given under eqn. (2).

[29]Both in Bound *et al.* (1994) and Pischke (1995) the measurement error model is derived for log-earnings rather than earnings in levels; however, Pischke (1995) notes that the same structure also applies to the earnings variable in levels.

[30]Similar results with related measurement error models have been derived in Fields *et al.* (2003a) and Antman and Mckenzie (2005a).

Expression (9) can be used to make simple simulations by assuming possible values for ρ, θ and α. The simulations performed will aim at estimating what size of measurement error would lead to the erroneous conclusion that there is convergence in earnings, when in fact the true β_1 is zero.

Another result that follows from this assumed structure of measurement error, is that the estimated coefficient when regressing Δy_{it} on predicted permanent initial advantage $\hat{y}_{it-1}$ will be an unbiased estimator of the parameter for the relationship between Δy_{it} and y_{it}^P. A formal derivation is also included in the appendix.[31]

Finally, turning to the implications of measurement error on the conditional mobility estimations, a substitution of equations (5) and (7) into (4) leads to

$$\begin{aligned}\Delta y_{it} = (\rho_t - 1)y_{it-1} + Z_i\tilde{\gamma}_t + \sum_l \sum_m st(l,m)\pi_{lm} \\ + ((\alpha+1)\eta_{it} + (\theta - \rho)\zeta_{it-1} + \omega_{it}) \qquad (10)\end{aligned}$$

It is clear from this expression that the variable y_{it-1} is not independent from the error term, because of its correlation with ζ_{it-1}. This correlation will bias *all* the parameter estimates in the model, when estimated by OLS. The only exception to this would occur if $\theta = \rho$, i.e., if the correlation in transitory earnings equals the correlation in the idiosyncratic measurement error, something which seems extremely unlikely in practice.

The route taken when estimating eqn. (10) will be to use occupational dummies together with wealth proxies as instruments for y_{it-1} (in addition to all the variables included under Z and X). This method is not fully satisfactory, since in such IV estimation there will be a component of transitory mobility that will not be captured by the instruments, leading to underestimation of the conditional mobility in the model.

6.1.2 Results

As mentioned in the previous section, a simulation is performed in order to appraise the potential effects of measurement error on the unconditional

[31] It can be proved that this estimation will not be biased even under a more general measurement error structure in which the measurement error is correlated with both the transitory *and* the permanent components of earnings.

mobility estimates. The simulation based on equation (9), i.e.,

$$\hat{\beta}_1 = \beta_1 \frac{V(y^*_{it-1})}{V(y_{it-1})} + (\rho - 1)\frac{\alpha(2+\alpha)V(\varepsilon_{it-1})}{V(y_{it-1})} + (\theta - 1)\frac{V(\zeta_{it-1})}{V(y_{it-1})}$$

consists in assuming that the true $\beta_1 = 0$, i.e., the mobility profiles are unrelated to initial earnings, and it is asked "How big should the measurement error be in order to lead to the conclusion of convergence, when in fact there is none?". More specifically, the simulation tries to capture how big should the variance for the measurement error component be as a fraction of the total variance of *reported* earnings, in order to obtain convergence of the magnitudes observed.

Under the assumption of no convergence in true earnings, i.e if $\beta_1 = 0$, there are 4 unknowns in equation (9): the α parameter that arises because of the correlation between measurement error and true earnings, the autocorrelation parameter ρ in transitory earnings, the variance of this term $V(\varepsilon_{it-1})$, and the autocorrelation parameter θ in the idiosyncratic component of measurement error.[32] Since these are too many parameters to identify, the simulation here presented will further assume $\theta = 0$ and equal variances between transitory earnings and the idiosyncratic component of measurement error, i.e., $V(\varepsilon_{it-1}) = V(\zeta_{it-1})$. Assuming $\theta = 0$ just makes stronger the potential impact of measurement error. This is because, for a given variance of the measurement error, higher values of θ would make $\hat{\beta}_1$ bigger, making it harder to find convergence as a result of measurement error. The assumption that transitory earnings and the idiosyncratic measurement error have equal variances has no further basis than pure convenience.[33] Finally, the value of the parameter $\hat{\beta}_1$ selected for the simulation is -0.438, which is the average of the $\hat{\beta}_{1t}$ calculated for each panel t by Least Squares.

Figure 10 shows the ratio of the variance of the measurement error to the variance of initial reported earnings, as a function of ρ and α. This ratio is sometimes called the *noise-to-signal* ratio. The graph plots how big this ratio must be in order to give an OLS parameter of $\hat{\beta}_1 = -0.438$, when in fact the true $\beta_1 = 0$. Hence, the lower this ratio is on the graph, the more pernicious is the effect of measurement error on the OLS estimates, (i.e. it is easier for them to be biased towards finding convergence).

[32]The variance of $V(\zeta_{it-1})$ can be obtained from the total variance in reported earnings if α and $V(\varepsilon_{it-1})$ are known.

[33]In the study of Pischke (1995) for the U.S., the magnitudes of these components are found to be roughly the same, but this result varied for different time periods.

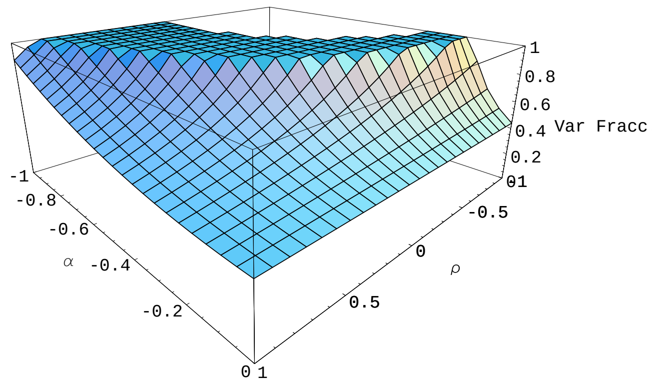


Figure 10: Measurement error Simulation

This graph shows that in order for the convergence result to be completely due to measurement error, the noise-to-signal ratio needs to be *at least* 40% (when $\alpha = 0$), and higher if α is negative. This is a relatively high noise-to-signal ratio. For comparison purposes, for the U.S. Bound and Krueger (1991) found this ratio to be around 28% in their sample of men in the CPS. Although, without further information coming from validation studies applied to Mexico, it cannot be evaluated whether such numbers are too high or not, it seems unlikely that the convergence result obtained is entirely due to measurement error.[34]

Regarding the impact of measurement error on the conditional mobility estimates, equation (10) showed that if earnings were measured with error, initial reported earnings would be correlated with the error term and this would bias all the Least Squares parameter estimates. The approach taken here is to use a series of regressors as instruments for the initial level of earnings y_{it-1}, and use the predicted measure of $\hat{y}_{it-1}$ as a regressor in the conditional mobility equation. The instruments include all the regressors in (4) (i.e., age, education, gender, sector, and regional dummies), as well as wealth proxies and dummies for occupation.

[34]There is a region of values for α, ρ where it is impossible for the measurement error to generate such convergence pattern. In particular, the range of values forming the flat part on the top of Figure 10, and the area that follows afterwards, are ranges of values where convergence cannot arise.

Instead of doing a simple 2-Stages Least Squares (2SLS) to correct for the measurement error bias, I estimate a whole system of equations by 3-Stages Least Squares (3SLS). The equations jointly estimated are

$$\begin{aligned} y_{it-1} &= Z_i\gamma_{t-1} + X_{it-1}\kappa_{t-1} + (\alpha\varepsilon_{it-1} + \zeta_{it-1}) \\ y_{it} &= Z_i\gamma_t + X_{it}\kappa_t + (\alpha\varepsilon_{it} + \zeta_{it}) \\ \Delta y_{it} &= (\rho_t - 1)y_{it-1} + Z_i\tilde{\gamma}_t + \sum_l \sum_m st(l,m)\pi_{lm} \\ &\quad + ((\alpha+1)\eta_{it} + (\theta - \rho)\zeta_{it-1} + \omega_{it}) \end{aligned}$$

Estimating this system by 3SLS partially corrects the measurement error bias (in the same way the standard 2SLS-IV estimator does), but in addition to that it can be used to perform a specification test for the structure of the earnings model proposed in equations (2)-(4). The assumed structural form is testable, since equation (4) (the third equation on the system) was derived from the first two equations by assuming that the error term ε_{it} is autocorrelated. In particular, the restrictions $\tilde{\gamma}_t = \gamma_t - \rho_t\gamma_{t-1}$ and $\pi_{lm} = \kappa_t(m) - \rho_t\kappa_{t-1}(l)$ are testable using the information from earnings equations in the first and final periods, together with the mobility equation. Such test provides information on whether the assumed structure is rejected by the data or not.

The results of the 3SLS estimations are presented in two parts. First, the parameter estimates for $(\hat{\rho}_t^{3SLS} - 1)$ are presented in Figure 11. Then the full regression results for the pooled subperiods are included in Tables 4 and 5.

As it can be appreciated in Figure 11 once the instrumentation is performed the conditional mobility appears to be divergent for some of the early periods in the sample, but it becomes convergent afterwards. In general, the estimated conditional convergence rates are smaller than the ones estimated via LS.[35]

The analysis of the full regression results in Tables 4 and 5 shows some differences with respect to the OLS estimations in Tables 2 and 3. In particular, the effects of human capital variables on mobility are smaller. Age variables are not always statistically significant, exhibiting a increasing concave pattern only during the aftermath of the Peso crisis. Education has a positive effect most of the periods in the specification in levels, but after

[35]The standard errors in the 3SLS estimations are quite small most likely due to the use of extra information coming from the two earnings equations.

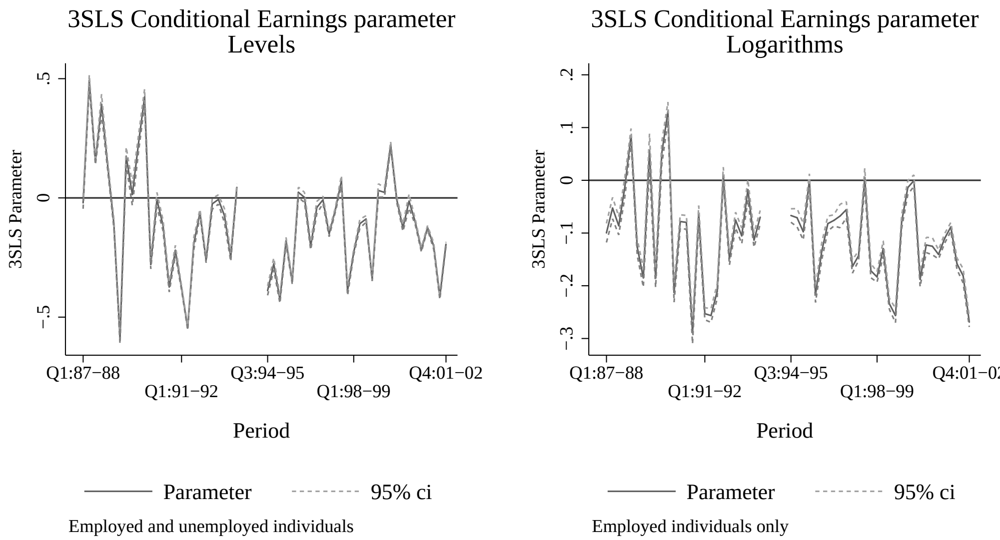


Figure 11: 3SLS. Conditional Mobility Parameter

the Peso crisis the U-pattern appears again with an inflexion point around 6 yrs. of education. This convex pattern is also found in most of the periods for the logarithmic specification, with an inflexion point fluctuating between 6 and 10 years of education. Being male has a positive effect on mobility, but the effect is much smaller than the one obtained in the LS regression. Finally, the patterns of conditional mobility by sector transition look similar to the ones estimated under LS. The regional dummies are not significant in many cases, but in general the US Border and North regions have a higher conditional mobility.[36]

[36]With the exception of the period 1987-93 for the US Border cities.

Table 4: 3SLS Regression. Levels. Dep. Var.: Change in Reported Earnings

	Q1:87-Q2:94		Q3:94-Q1:99		Q2:99-Q4:02	
	Coef./(s.e.)		Coef./(s.e.)		Coef./(s.e.)	
Initial Earnings	-0.043	***	-0.187	***	-0.096	***
	(0.00)		(0.00)		(0.00)	
Age						
Linear	11.147		34.551	**	-33.537	*
	(17.58)		(14.19)		(19.73)	
Squared	-0.194		-0.450	**	0.346	
	(0.22)		(0.18)		(0.24)	
Education						
Linear	-17.619		-59.777	***	-22.235	
	(14.98)		(12.46)		(17.85)	
Squared	2.301	***	5.395	***	2.127	**
	(0.74)		(0.59)		(0.84)	
Male	95.379	**	70.771	**	62.262	
	(40.28)		(31.68)		(43.60)	
Sector Transitions						
Unemployed to Informal Worker	4332.743	***	3217.986	***	4296.387	***
	(155.43)		(103.84)		(173.67)	
Unemployed to Informal Self-employed	5649.411	***	3976.265	***	5285.783	***
	(146.82)		(98.56)		(168.86)	
Unemployed to Formal Self-employed	13180.098	***	12411.062	***	13286.223	***
	(482.10)		(292.52)		(574.57)	
Unemployed to Formal Worker	4888.759	***	4085.576	***	5154.720	***
	(138.09)		(91.93)		(159.33)	
Informal Worker to Unemployed	-3827.637	***	-2653.410	***	-3655.622	***
	(156.54)		(99.18)		(172.06)	
Informal Worker to Informal Worker	493.490	***	578.438	***	644.917	***
	(188.54)		(116.38)		(208.29)	
Informal Worker to Informal Self-employed	1801.668	***	1324.286	***	1579.085	***
	(186.54)		(116.46)		(207.45)	
Informal Worker to Formal Self-employed	9111.391	***	9050.538	***	9616.414	***
	(279.79)		(385.59)		(367.22)	
Informal Worker to Formal Worker	1042.026	***	1394.146	***	1458.778	***
	(184.53)		(114.53)		(205.23)	
Informal Self-employed to Unemployed	-4979.779	***	-3368.817	***	-4493.590	***
	(147.09)		(93.00)		(163.61)	
Informal Self-employed to Informal Worker	-635.633	***	-130.672		-194.015	
	(186.48)		(116.42)		(207.69)	
Informal Self-employed to Informal Self-employed	670.471	***	618.488	***	745.816	***
	(180.38)		(110.97)		(202.38)	
Informal Self-employed to Formal Self-employed	8015.395	***	8294.042	***	8752.673	***
	(227.60)		(178.64)		(265.68)	
Informal Self-employed to Formal Worker	-95.897		688.850	***	629.868	***
	(180.35)		(112.10)		(202.41)	

Table 4 (Continued)

Formal Self-employed to Unemployed	-1.10e+04	***	-9626.783	***	-1.16e+04	***
	(280.26)		(201.61)		(614.66)	
Formal Self-employed to Informal Worker	-6473.768	***	-6303.426	***	-7283.754	***
	(257.47)		(248.90)		(328.12)	
Formal Self-employed to Informal Self-employed	-5136.017	***	-5551.103	***	-6399.542	***
	(218.29)		(163.02)		(261.32)	
Formal Self-employed to Formal Self-employed	2126.333	***	2167.275	***	1667.414	***
	(247.91)		(196.46)		(295.24)	
Formal Self-employed to Formal Worker	-5920.108	***	-5383.258	***	-6532.291	***
	(219.83)		(166.09)		(261.98)	
Formal Worker to Unemployed	-4272.175	***	-3337.792	***	-4375.583	***
	(137.25)		(83.74)		(148.90)	
Formal Worker to Informal Worker	102.979		-36.649		-11.295	
	(184.54)		(115.08)		(206.24)	
Formal Worker to Informal Self-employed	1408.169	***	695.048	***	924.396	***
	(180.26)		(112.31)		(202.97)	
Formal Worker to Formal Self-employed	8685.365	***	8402.423	***	8915.297	***
	(229.01)		(186.66)		(267.00)	
Formal Worker to Formal Worker	646.397	***	784.346	***	802.634	***
	(176.80)		(107.71)		(199.05)	
Region						
Mexico City (omitted)						
US Border	-178.441	**	233.129	***	133.992	*
	(80.22)		(56.90)		(77.18)	
North	53.027		75.213	**	136.869	***
	(48.37)		(37.50)		(52.03)	
Center	20.800		-31.795		127.379	**
	(49.21)		(40.61)		(55.71)	
South	-92.753		-137.895		-54.463	
	(125.75)		(91.28)		(117.07)	
Constant	-594.602		-942.625	***	384.103	
	(387.20)		(298.34)		(438.81)	
R-squared	0.0534		0.2086		0.1139	
N	98318		83107		55411	

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 5: 3SLS Regression. Dep. Var.: Change in Reported Log-Earnings

	Q1:87-Q2:94		Q3:94-Q1:99		Q2:99-Q4:02	
	Coef./(s.e.)		Coef./(s.e.)		Coef./(s.e.)	
Initial Log-Earnings	-0.0330	***	-0.1057	***	-0.0894	***
	(0.001)		(0.001)		(0.002)	
Age						
Linear	-0.0005		0.0040	**	-0.0029	
	(0.002)		(0.002)		(0.002)	
Squared	-0.0000		-0.0001	**	0.0000	
	(0.000)		(0.000)		(0.000)	
Education						
Linear	-0.0051	***	-0.0018		-0.0081	***
	(0.002)		(0.002)		(0.002)	
Squared	0.0004	***	0.0004	***	0.0004	***
	(0.000)		(0.000)		(0.000)	
Male	-0.0143	***	0.0210	***	0.0111	**
	(0.004)		(0.004)		(0.005)	
Sector Transitions						
Informal Worker to Informal Worker (omitted)						
Informal Worker to Informal Self-employed	0.2033	***	0.1760	***	0.1689	***
	(0.007)		(0.007)		(0.008)	
Informal Worker to Formal Self-employed	0.7596	***	0.9149	***	0.8380	***
	(0.022)		(0.040)		(0.035)	
Informal Worker to Formal Worker	0.1843	***	0.3125	***	0.2602	***
	(0.006)		(0.006)		(0.007)	
Informal Self-employed to Informal Worker	-0.1915	***	-0.1580	***	-0.1732	***
	(0.007)		(0.006)		(0.008)	
Informal Self-employed to Informal Self-employed	0.0102		0.0181	**	-0.0035	
	(0.009)		(0.008)		(0.010)	
Informal Self-employed to Formal Self-employed	0.5637	***	0.7513	***	0.6593	***
	(0.018)		(0.020)		(0.023)	
Informal Self-employed to Formal Worker	-0.0077		0.1571	***	0.0891	***
	(0.009)		(0.008)		(0.010)	
Formal Self-employed to Informal Worker	-0.6897	***	-0.7362	***	-0.7897	***
	(0.019)		(0.026)		(0.030)	
Formal Self-employed to Informal Self-employed	-0.4881	***	-0.5537	***	-0.6136	***
	(0.016)		(0.018)		(0.022)	
Formal Self-employed to Formal Self-employed	0.0635	***	0.1782	***	0.0488	*
	(0.020)		(0.023)		(0.027)	
Formal Self-employed to Formal Worker	-0.5067	***	-0.4145	***	-0.5251	***
	(0.016)		(0.018)		(0.022)	
Formal Worker to Informal Worker	-0.1796	***	-0.2439	***	-0.2317	***
	(0.006)		(0.006)		(0.007)	
Formal Worker to Informal Self-employed	0.0219	**	-0.0714	***	-0.0638	***
	(0.009)		(0.008)		(0.010)	
Formal Worker to Formal Self-employed	0.5733	***	0.6602	***	0.5989	***
	(0.018)		(0.021)		(0.023)	
Formal Worker to Formal Worker	0.0037		0.0683	***	0.0276	***
	(0.008)		(0.007)		(0.009)	

Table 5 (Continued)

Region						
Mexico City (omitted)						
US Border	-0.0295	***	0.0553	***	0.0174	*
	(0.009)		(0.008)		(0.009)	
North	0.0003		0.0201	***	0.0341	***
	(0.005)		(0.005)		(0.006)	
Center	0.0026		-0.0020		0.0298	***
	(0.005)		(0.006)		(0.007)	
South	-0.0135		-0.0165		-0.0071	
	(0.013)		(0.012)		(0.014)	
Constant	0.3332	***	0.6335	***	0.8357	***
	(0.039)		(0.040)		(0.048)	
R-squared	0.0332		0.1050		0.0933	
N	95602		79649		53851	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Specification Tests for the Conditional Earnings Model

	Levels		Logarithms	
Period	χ stat	p-value	χ stat	p-value
Q1:87-Q2:93	25.11	0.456	6.06	0.993
Q3:94-Q1:99	59.21	0.000	96.99	0.000
Q2:99-Q4:01	13.29	0.973	13.45	0.706

Finally, the results for the tests of the structural relationships are presented in Table 6. The test is a joint test of the hypotheses $\tilde{\gamma}_t = \gamma_t - \rho_t\gamma_{t-1}$ and $\pi_{lm} = \kappa_t(m) - \rho_t\kappa_{t-1}(l)$. The results show that the data fail to reject these hypotheses during the first and last periods of the sample. This is a positive thing, since it means that the assumed structure does not contradict the data. However, for the years in the aftermath of the Peso crisis the data strongly rejects the model proposed. This means that the shock that came after this crisis altered the dynamic structure of earnings, and mobility cannot be described by the same model that explained reasonably well the other periods. A more careful study on what is the structure of earnings dynamics during this period is an interesting topic that deserves further research.

6.2 Attrition Bias

6.2.1 Theory

Attrition bias and non-reporting of earnings can be a serious problems for a mobility study like this one. They entail a loss of identification power if the information is not missing at random. The approach taken in this paper to deal with these problems is to abandon the pretension of obtaining precise point estimates of $E(\Delta y_{it}|y_{it-1})$, and instead turn to partial identification techniques (see for instance Manski, 1995, 2003). The idea underlying partial identification analysis is to provide a whole region where the conditional expectation of interest can lie, given that the information available is not complete. The advantage of this approach is that the set of assumptions made in generating the identification regions are minimal. In this paper partial identification analysis will be applied to the case of missing outcomes (earnings in the final period) due to attrition and non-reporting. These are the two biggest sources for missing data in the sample.

In order to see how these methods work in the present context, let z_i be an indicator variable on whether the earnings of individual i are observed in the final period. Also for simplicity of exposition, rescale $E(\Delta y_{it}|y_{it-1})$ to make it lie inside the [0,1] interval. By the law of iterated expectations

$$\begin{aligned} E(\Delta y_{it}|y_{it-1}) = E(\Delta y_{it}|y_{it-1}, z_i = 1) \cdot P(z_i = 1|y_{it-1}) \\ + E(\Delta y_{it}|y_{it-1}, z_i = 0) \cdot P(z_i = 0|y_{it-1}). \end{aligned}$$

The data alone reveals $E(\Delta y_{it}|y_{it-1}, z_i = 1)$, $P(z_i = 1|y_{it-1})$ and $P(z_i = 0|y_{it-1})$ only. Since the whole expectation was rescaled to lie between [0,1], it follows that the lowest value $E(\Delta y_{it}|y_{it-1})$ can possibly take is

$$E(\Delta y_{it}|y_{it-1}, z_i = 1) \cdot P(z_i = 1|y_{it-1})$$

when $E(\Delta y_{it}|y_{it-1}, z_i = 0) = 0$, and the highest value it can take is

$$E(\Delta y_{it}|y_{it-1}, z_i = 1) \cdot P(z_i = 1|y_{it-1}) + P(z_i = 0|y_{it-1})$$

when $E(\Delta y_{it}|y_{it-1}, z_i = 0) = 1$. Any point between these two bounds forms the identification region $H[E(\Delta y_{it}|y_{it-1})]$. Any values inside this region are logically possible for $E(\Delta y_{it}|y_{it-1})$, given the amount of attrition and non-response in the data. This identification region is estimated by nonparametric methods.

It is important to remark that without any extra assumptions, the information contained in $H[E(\Delta y_{it}|y_{it-1})]$ is all that the data reveals about $E(\Delta y_{it}|y_{it-1})$. Hence, the estimation of this region without further assumptions gives the worst case scenario for the impact of attrition and non-reporting. Plausible extra assumptions can narrow the width of the identification region $H[E(\Delta y_{it}|y_{it-1})]$. However, that refinement will be left for further extensions of this exercise.

6.2.2 Results

Before showing the partial identification analysis, a descriptive look at the problem of attrition and non-reporting in the panel is presented. Figure 12 displays the number of missing individuals in the panel, as a fraction of the potential population of interest. This graph shows that around 50% of the potential population is missing from the panel. The bottom two graphs plot the reasons why individuals are not included in the sample. The list of possible reasons include attrition (i.e., disappearing from the sample in further re-interviews), mismatch of individuals between the first and final periods according to gender, age and education, missing earnings information, missing dwellings information,[37] missing sector information, and outliers in the earnings variables.[38]

[37]Relevant only after the third quarter of 1994 when the dwelling questionnaire was introduced.

[38]An individual was considered to have earnings beyond the normal if for a single month it reported having earned more than 30,000 US Dollars, and reports much smaller amounts in other interviews.

From these graphs it is clear that the main reason for missing individuals from the sample is attrition. The fact that the ENEU tracks dwellings and not households explains part of this high attrition. It is important to note that the peak of attrition found around 1988-89 is of a different nature than the attrition present at other years. In these years it was not that an exceptionally high number of individuals were leaving their households, but rather that entire households were not matchable over the panel years. Perhaps there was an undocumented change in the areas surveyed in the panel, or there could be mistakes in the coding of household identification numbers. In any case, this extra peak in attrition is less worrisome as it is unlikely to be driven by economic reasons, and it probably does not generate much bias in the estimates. Besides attrition, the other categories more relevant for the exclusion of individuals from the sample are missing earnings reports, missing dwelling characteristics, and mismatches.[39]

The demographic characteristics for the missing individuals are presented in Figure 13.[40] This figure shows that there is a clear difference in the demographic characteristics of individuals who are missing because of attrition and mismatches, and the ones missing due to non-reporting of earnings. Overall, the latter are more educated, older, have a higher fraction of males, and have higher earnings.[41]

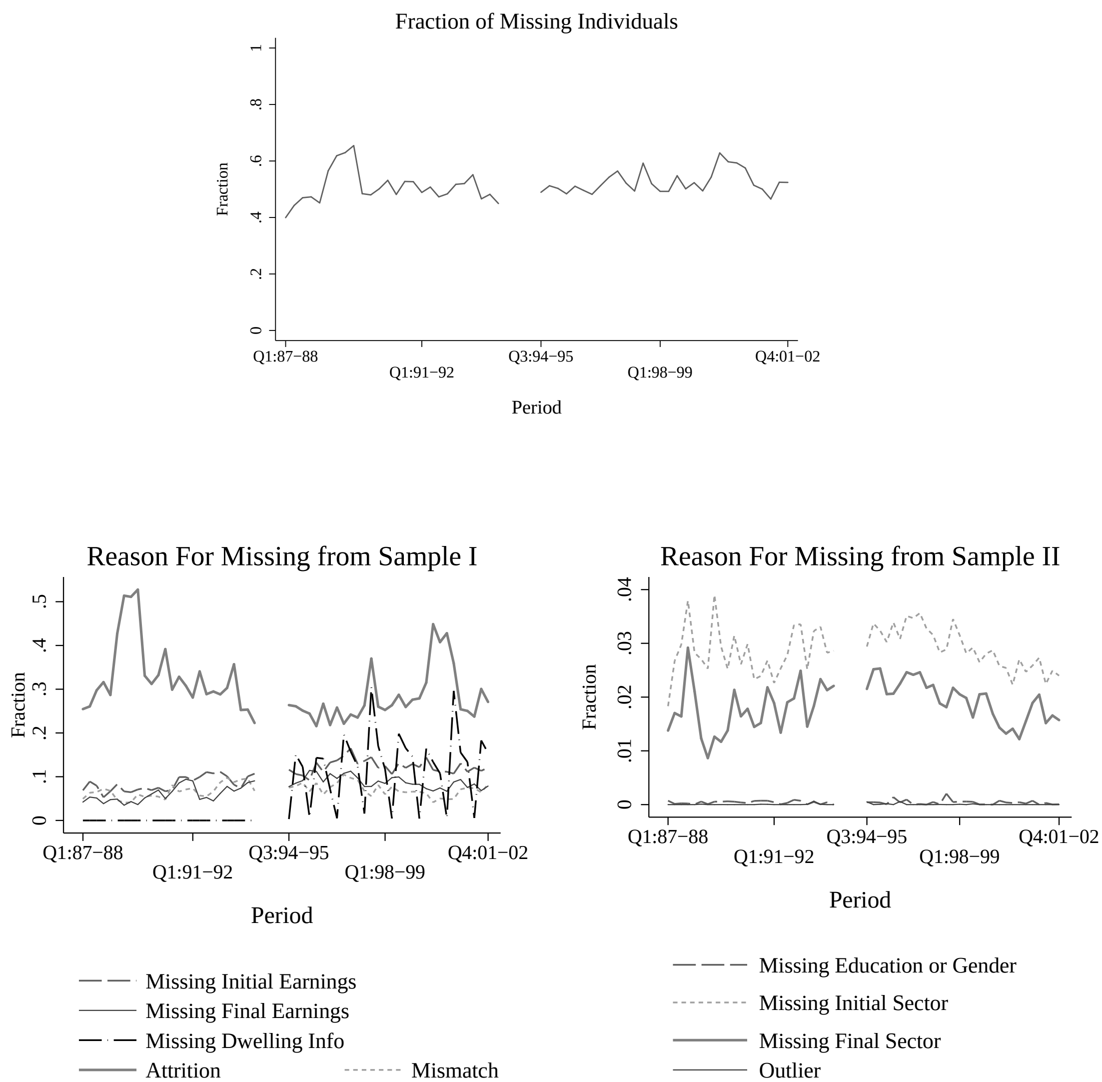
A comparison of Figure 13 with Table 1 shows that the attritors do not differ much in their characteristics from the individuals included in the sample; however, there are substantial differences between the individuals not reporting their earnings and the ones included in the sample. This is a cause of concern, because if educated high-income individuals are not reporting their earnings when they experience positive mobility, then the previous conclusions could be wrong.

The bounds of partial identification for the unconditional mobility expec-

[39] In this figure the categories are not exclusive, i.e., an individual could be mismatched and also not report earnings.

[40] In this figure the categories *are* exclusive. This means that for an individual with multiple causes for being excluded from the sample, he will be first classified as an attritor (if he is one), and if he is not priority is then given to mismatch and finally to non reporting. Also notice that this figure only includes the main categories for missing from the sample.

[41] Most of the individuals who do not report earnings, do so at one point in time only. Hence, the earnings plotted for such individuals are either the earnings at the year before or at the year after, depending on when they refused to answer. Of course, there is a small fraction of individuals who do not report any of them.



Note: Categories are not mutally exclusive

Figure 12: Amount and Composition of Attrition

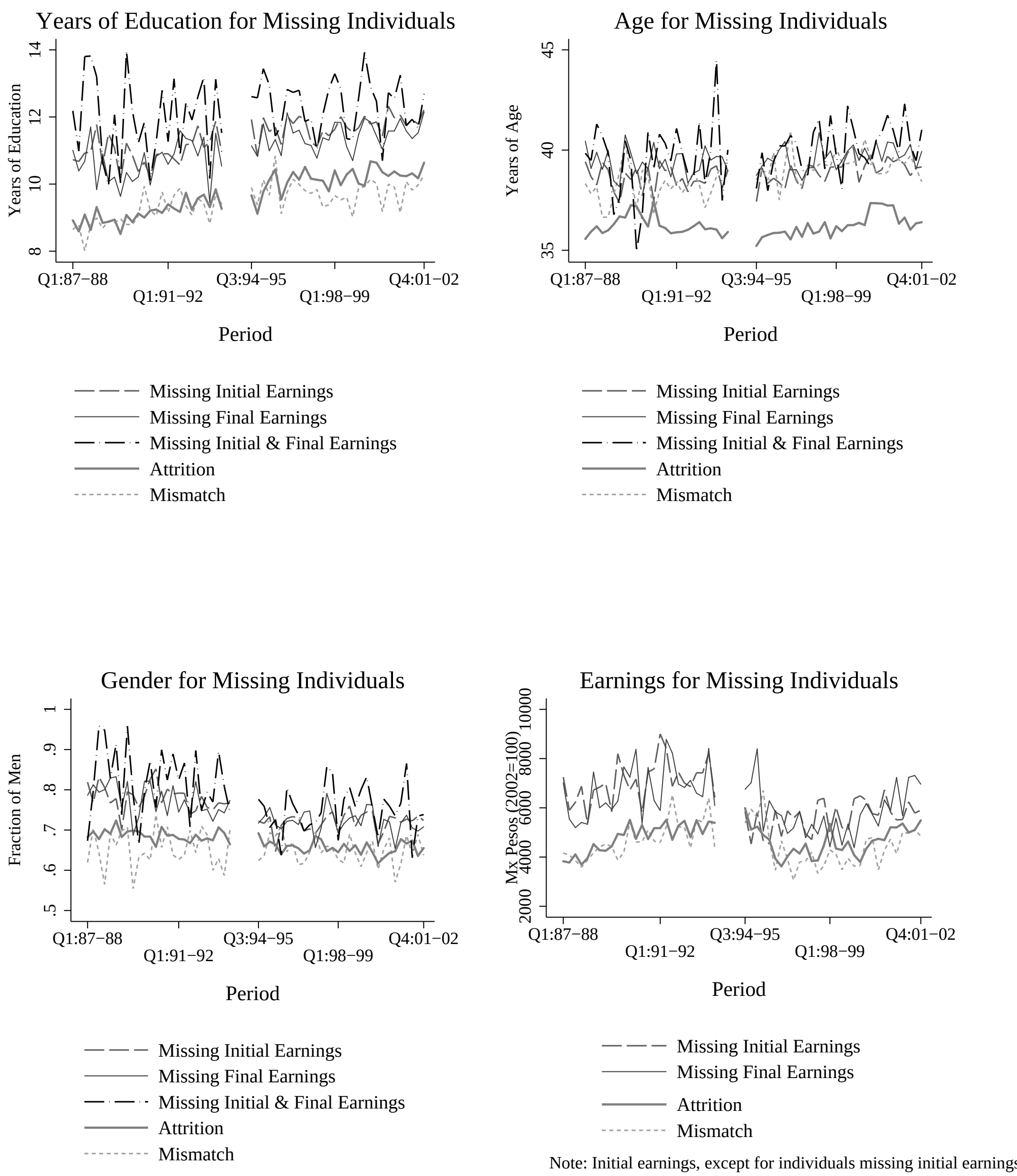


Figure 13: Demographic Characteristics of Missing Individuals

tation $E(\Delta y_{it}|y_{it-1})$, i.e.,

$$\frac{E(\Delta y_{it}|y_{it-1}, z_i = 1)P(z_i = 1|y_{it-1})}{E(\Delta y_{it}|y_{it-1}, z_i = 1)P(z_i = 1|y_{it-1}) + P(z_i = 0|y_{it-1})}$$

are presented in Figure 14. This figure also includes a kernel regression on the complete data. Also, for the sake of compactness, the graph plots the results only for some select years. It is important to remember that this analysis corresponds only to the effects on the mobility estimates of attrition, mismatch of individuals over time, and non-reporting of earnings in the last period, which were the main reasons why individuals were dropped from the sample.

The solid line in the figures displays the kernel estimates for the individuals with complete information. As expected from the previous analysis, these estimates have a negative slope, especially for high-earners. The dotted lines in the graph denote the lower and upper bounds of the identification region $H[E(\Delta y_{it}|y_{it-1})]$. Recalling, the meaning of this region is that, without further assumptions, $E(\Delta y_{it}|y_{it-1})$ can lie anywhere inside these bounds.

As it can be seen, for 1987 and 1989 it cannot be ruled out that the mobility was not convergent, since the identification region contains the zero line inside it.[42] For 1995 and 2001 the result of convergence still holds in the presence of attrition. In general, the results change depending on the year selected, but the loss of information due to attrition and non-reporting is substantial. One interesting result to note is that the bounds become wider for initially rich individuals. The reason for this is that rich individuals are more prone to not reporting their earnings in the final period, hence the probability of including them in the sample is smaller.

The results presented only focused on the impact of attrition in the unconditional mobility estimates, but they are indicative of the perverse effects of this problem in terms of loss of information. This loss of information also affects the conditional mobility estimates.

Before closing this section it is important to remark that the bounds previously presented are the more negative scenario that can be faced. In particular, unlike other treatments of the attrition problem, no assumptions about the attrition process were made. If one starts building up extra assumptions (e.g., a fraction x of the population with missing values is actually

[42] Notice however, that the bounds are much narrower for 1987. This is because attrition was considerably lower in that year.

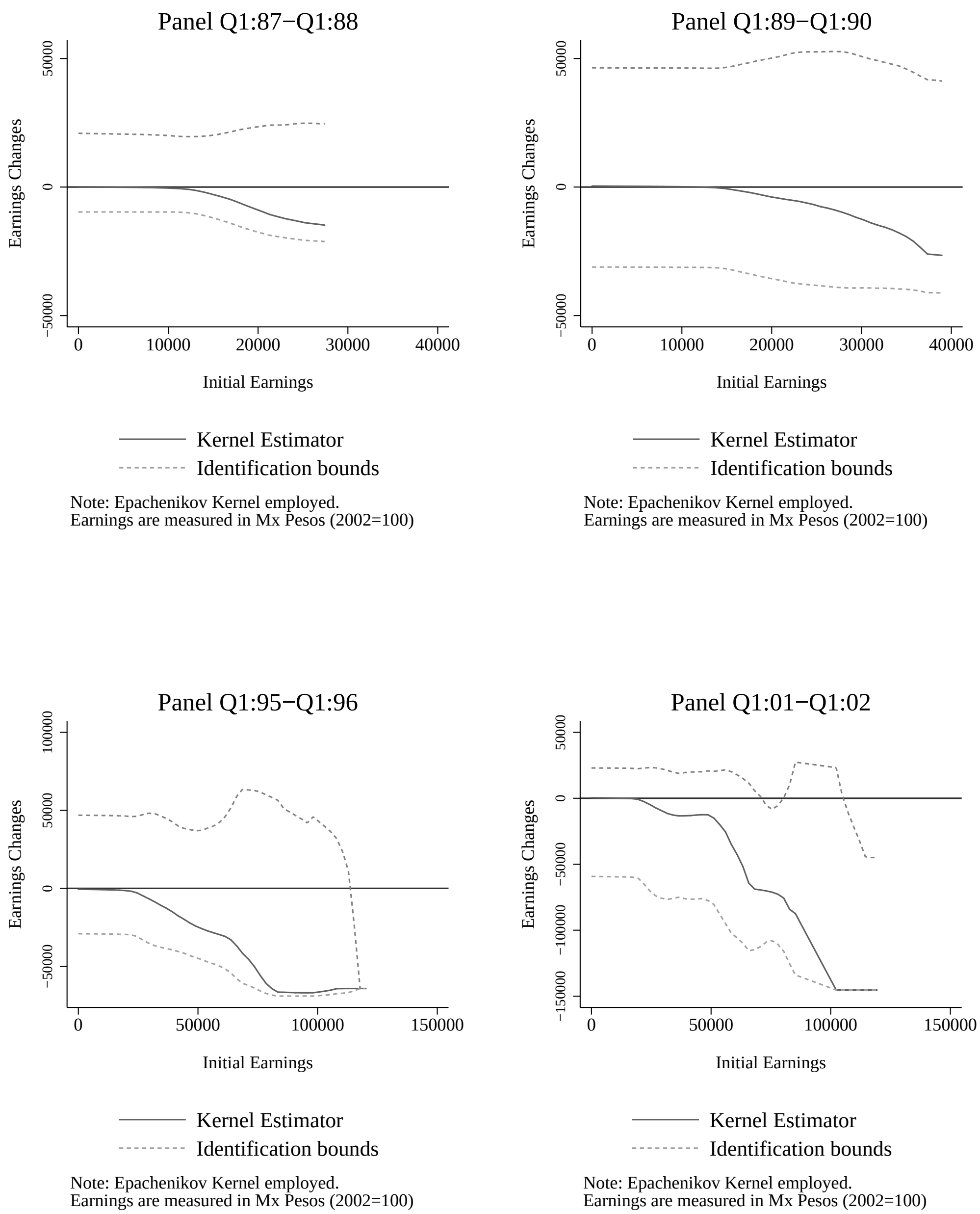


Figure 14: Partial Identification Bounds on Unconditional Mobility Expectation

missing at random), tighter bounds can be obtained and more positive conclusions will be reached. One interesting line of research to pursue would be to find a potential instrument that, with some extra assumptions, could help narrow these identification bounds.

This section, rather than giving a completely negative panorama of the effects of missing data on the analysis, attempts to call for caution in the presence of such high levels of attrition. The results presented throughout the paper are still meaningful for the subsample of the population with complete information. To what extent they can be extended to the whole urban population is something that requires more analysis.

7 Conclusions

This paper studied the relationship between different measures of initial advantage and earnings mobility, as well as the impact of socioeconomic characteristics of the individual on earnings mobility.

The answer to the question of "Are the most advantaged individuals gaining more (losing less) in terms of earnings changes?" is that in the majority of the estimations the most advantaged individuals either kept their advantage or lost more than the rest of the population. When reported earnings were taken as the measure of initial advantage, having a higher initial advantage was found to be negatively associated with mobility. In other words, there was convergence in earnings between high-earners and low-earners. This convergence fluctuated over time, but its magnitude was rather stable. However, there is evidence suggesting that this result is reflecting an adjustment of earnings from a transitory shock back to its more permanent level. For this reason, when the relationship between mobility and several proxy measures for permanent advantage was considered, there was less or no convergence at all. In these estimations, a low convergence pattern was found during the late nineties-early 00's, and the only case of strong convergence in earnings occurred right after the 1994 Peso crisis. This macroeconomic shock led to convergence in earnings when measured in levels, but not in log-earnings. This indicates that during this period high-earners lost more than everybody else in absolute terms, but their losses were proportional to their higher initial earnings.

In general, what these results imply is that, most of the times, the mobility experienced over a year did not alter the permanent advantage of individuals.

The only major exception being the aftermath of the 1994 Peso crisis. In these years, the negative effects of the crisis were spread throughout the economy and not even high-earners managed to avoid losses.

Regarding the answers to the question "What is the *ceteris paribus* impact of socioeconomic characteristics of the individual on earnings mobility?" it was found that holding everything else constant, more education led to negative mobility for individuals with low levels of education, but after a certain point more education was associated with upward conditional mobility (the inflexion point fluctuated between 2 and 4 years of education depending on the period analyzed). Being male had a strong positive effect on mobility, and age had a small, but positive effect also.

Also *ceteris paribus*, transitions into formal self-employment were the ones that brought the largest gains (smallest losses), while transitions into informal wage work were associated with the largest losses (smallest gains), excluding of course the movements into unemployment. Transitions into formal wage work and informal self-employment were between the two previous categories. In general, it seems that transitions to informal self-employment brought more positive mobility than the ones into formal wage work. However, this result was sometimes reversed in the period following the 1994 Peso crisis. One problem with the previous results is that in the case of self-employed individuals, it is not possible to discern how much of the reported earnings are payments to the labor factor, and how much are returns to physical capital.

Living in cities along the US Border and in the North brought upward conditional mobility, while living in the Center and South brought more negative conditional mobility.

Finally, the answer to the question of "How do these socioeconomic factors affect the impact of initial earnings on mobility?" it was found that, holding everything else constant, initial earnings were negatively related to earnings mobility, meaning that individual earnings converged to their own conditional mean. This conditional convergence rate was slightly stronger than the unconditional one, meaning that the overall impact of the individual socioeconomic characteristics generated divergence in earnings.

Simulations on the potential impact of measurement error showed that this error needs to be quite large in order to be the *sole* reason underlying the convergence results found in the unconditional mobility regressions with reported earnings. Needless to say, not much else can be said without a proper validation study on the amount of measurement error that applies for a country like Mexico. It is important to remark that the results found for the unconditional relationship between mobility and permanent advantage are not affected by this potential measurement error. The effects of measurement error on the conditional mobility estimates cannot be fully controlled for, but the instrumented conditional mobility estimates are smaller than the ones obtained under the assumption of no measurement error.

The amount of attrition in the panel, mismatch over time and non-reporting of the earnings variable led to important losses of information in the panel. The effects of these problems in the mobility estimates were assessed by means of partial identification analysis. The partial identification bounds estimated were large and, depending on the year selected, they sometimes challenged the convergence results obtained in the unconditional mobility estimations. Although these conclusions pertain to the worst case scenario in terms of the potential impact of attrition, mismatch and non-reporting, they warn against generalizing the results obtained to the whole urban population without further research on this topic.

A Appendix

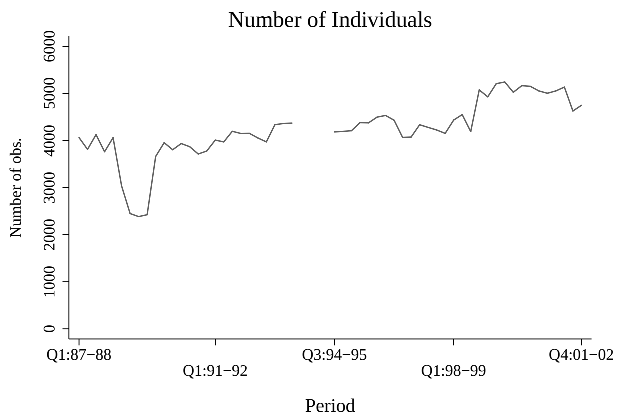


Figure 15: Number of Observations per Panel

A.1 Regions and their Cities

City	**Region**
Mexico City	Mexico City
Guadalajara	Center
León	
Puebla	
Orizaba	
Veracruz	
Chihuahua	North
Monterrey	
Tampico	
Torreón	
San Luis Potosí	
Mérida	Sur
Ciudad Juárez	US Border
Tijuana	
Matamoros	
Nuevo Laredo	

A.2 Proofs for Expressions in Section 6.1

This appendix presents derivations of equation (9) and establishes the consistency of the IV parameter estimating the relationship between earnings mobility and permanent advantage.

Proof of (9)

Note first that, under the measurement error model described by (5)-(8), the estimated covariance between reported mobility and initial reported earnings equals

$$cov(\Delta y_{it}, y_{it-1}) = cov(\Delta y_{it}^P + (1+\alpha)\Delta\varepsilon_{it} + \Delta\zeta_{it}, y_{it-1}^P + (1+\alpha)\varepsilon_{it-1} + \zeta_{it-1})$$

with $y_{it}^P \perp \varepsilon_{it} \perp \zeta_{it}$. Hence

$$\begin{aligned} cov(\Delta y_{it}, y_{it-1}) &= cov(\Delta y_{it}^P, y_{it-1}^P) + (1+\alpha)^2 cov(\Delta\varepsilon_{it}, \varepsilon_{it-1}) + cov(\Delta\zeta_{it}, \zeta_{it-1}) \\ &= cov(\Delta y_{it}^*, y_{it-1}^*) + \alpha(2+\alpha)(\rho-1)V(\varepsilon_{it-1}) + (\theta-1)V(\zeta_{it-1}) \end{aligned}$$

Dividing this expression by $V(y_{it-1})$ and recalling that the true (unbiased) $\beta_1 = cov(\Delta y_{it}^*, y_{it-1}^*)/V(y_{it-1}^*)$, gives the biased OLS $\hat{\beta}_1$ parameter in expression (9).

Proof of unbiasedness of the IV parameter estimating the relationship between earnings mobility and predicted permanent advantage.

First notice that the unbiasedness of $\hat{y}_{it-1}$ follows because if measured earnings y_{it} equal $y_{it} = y_{it}^P + (1+\alpha)\varepsilon_{it} + \zeta_{it}$, and if permanent earnings are the component of earnings determined by a vector of variables W_{it} affecting permanent advantage, i.e., $y_{it}^P = W_{it}\Gamma_t$, then the first-stage regression of y_{it-1} on W_{it} will give unbiased estimators of Γ_t, by the assumed orthogonality between y_{it}^P, ε_{it}, and ζ_{it}. With these $\hat{\Gamma}_t$ the predicted $\hat{y}_{it-1}$ will also be unbiased.

The unbiasedness of $\hat{y}_{it-1}$ being established, it easily follows that the second-stage regression

$$\Delta y_{it} = \beta_0^P + \beta_1^P \hat{y}_{it-1} + u_{it}$$

will give unbiased estimators of β_1^P, by the aforementioned orthogonality conditions and the fact that $\Delta y_{it} = \Delta y_{it}^P + (1+\alpha)\Delta\varepsilon_{it} + \Delta\zeta_{it}$.

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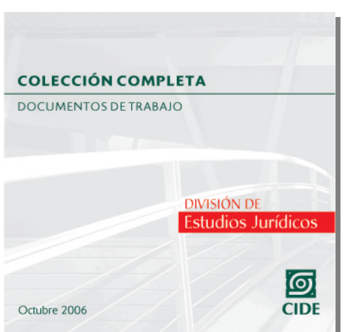

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