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Missing Poor in the U.S.

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Abstract

Given that poor individuals face worse survival conditions than non-poor individuals, one can expect that a steeper income/mortality gradient leads, through stronger income-based selection, to a lower poverty rate at the old age (i.e. the "missing poor" hypothesis). This paper uses U.S. state-level data on poverty at age 65+ and life expectancy by income levels to provide an empirical test of the missing poor hypothesis. Using air pollution as an instrument for mortality differentials, we show that instrumented changes in mortality differentials have a negative and statistically significant effect on old-age poverty: a 1 % increase in the mortality differential implies a 9 % decrease in the 65+ headcount poverty rate. Using those regression results, we compute hypothetical old-age poverty rates while neutralizing the impact of the income/mortality gradient, and show that correcting for heterogeneity in income-based selection effects modifies the comparison of old-age poverty prevalence across states.

Keywords: poverty, measurement, income/mortality gradient, selection biases, comparability.

JEL classification codes: I32.

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1 Introduction

The income/mortality gradient is a widely established stylized fact: empirical studies show, for various countries and epochs, that lower incomes are statistically related with higher mortality risks.[1] Those studies show that, although the strength of the income/mortality gradient varies across gender (it is stronger for men than for women) and also across countries, it remains true that poorer individuals have, on average, shorter lives than richer individuals.[2]

What are the implications of the income/mortality relationship for poverty measurement? In a pioneer work, Kanbur and Mukherjee (2007) argued that standard poverty measures tend, under income-differentiated mortality, to underestimate the actual extent of the poverty phenomenon. Actually, given that poor individuals face, on average, worse survival conditions than non-poor individuals, measures of old-age poverty are subject to a selection bias: these measure poverty only within the surviving population, which includes, due to excess mortality of the poor, a relatively smaller proportion of poor individuals. Income-based selection leads thus to an under-representation of poor individuals at the old age.

Under income-differentiated mortality, low levels of measured old-age poverty may not be caused by a better situation of the old, but may be due to a steeper income/mortality gradient, leading to a stronger income-based selection. Making an analogy with Sen's (1998) expression of "missing women", we can coin this hypothesis the "missing poor" hypothesis. According to that hypothesis, a steeper income/mortality gradient leads, through stronger income-based selection, to a lower poverty rate at the old age.

The missing poor hypothesis has important consequences for the comparison of old-age poverty measures across countries. When the strength of the income/mortality gradient varies across space, the missing poor phenomenon questions the comparability of old-age poverty measures across countries. If the missing poor hypothesis is correct, observed gaps in standard poverty measures may be due not to "true" differences in the prevalence of poverty, but may be due to income-based selection processes of unequal strengths across space.

Whereas the existence of an income/mortality gradient is strongly established, the missing poor hypothesis has not yet been tested empirically. Actually, the mere existence of an income/mortality gradient does not necessarily imply that poverty measures are biased downwards. True, the income/mortality gradient leads to some selection effects, but the final impact of those selection effects on poverty is not obvious, since selection effects induce general equilibrium effects (e.g. variations in wages) that may make poverty measures take, in

[1] See Pamuk (1985, 1988), Duleep (1986), Deaton and Paxson (1998), Backlund et al (1999), Deaton (2003), Jusot (2003), Bossuyt et al (2004), Van Oyen et al (2005), Duggan et al (2007), Cristia (2009), Salm (2012), Belloni et al (2013), Kalwij et al (2013), Chetty et al (2016) and Milligan and Schirle (2018). One exception is Snyder and Evans (2006), who find a negative correlation between income and survival.

[2] On the variation of the income/mortality gradient across gender and across European countries, see Lefebvre et al (2018).

theory, either lower or higher levels than in the absence of selection effects.[3]

The goal of this paper is to test empirically the missing poor hypothesis, that is, to investigate the effect of the mortality differential between the poor and the non-poor on measures of old-age poverty. For that purpose, we use U.S. state-level data on old-age poverty (age 65 and more) and on life expectancy at age 40 by income quartile, to test the missing poor hypothesis, that is, to examine whether higher mortality differentials between the poor and the non-poor tend to reduce measured old-age poverty.

At this stage, it should be stressed that the empirical test of the missing poor hypothesis raises several difficulties. From an empirical perspective, the estimation of a relation between poverty and mortality differentials may not necessarily validate or invalidate the missing poor hypothesis. A first estimation problem may come from reverse causation, i.e. a higher prevalence of poverty leading to stronger mortality differentials between the poor and the non-poor. Reverse causation from poverty to mortality differentials is plausible in theory, and cannot be excluded *a priori*. For instance, in the presence of capacity constraints, a larger prevalence of poverty can reduce the capacity of pro-poor programs to achieve their goals, leading, *in fine*, to larger mortality differentials between the poor and the non-poor. Besides reverse causation, another estimation problem may come from omitted variable biases: the estimated relation between poverty and mortality differentials may be due to a third variable affecting both mortality differentials and poverty measures, so that the observed relation would not suffice to validate the missing poor hypothesis.[4]

In order to avoid drawing fallacious conclusions on the impact of mortality differential on poverty measurement, it is important, when testing the missing poor hypothesis, to develop an empirical strategy that uses an instrumental variable for mortality differentials, which is not affected by poverty measurement, but which affects mortality differentials within the population. For that purpose, we use state-specific data on air pollution as an instrument for mortality differentials, and examine its impact on old-age poverty measurement.

Anticipating on our results, we show that, when we carry out simple OLS regressions of mortality differentials on old-age poverty, the mortality differential has negative and statistically significant effect on measured old-age poverty. Moreover, when we instrument for the mortality differential variable by means of an air pollution indicator, we find that the instrumented changes in mortality differentials have a negative and statistically significant effect on 65+ headcount poverty rate: a 1 % increase in the mortality differential leads to a 9 % decrease in the 65+ headcount poverty rate. Those results, which are robust to various specifications concerning control variables, provide some empirical support for the missing poor hypothesis, in the sense that, in line with that hypothesis, a steeper income/mortality gradient leads to reduce the measured old-age poverty.

In the light of this empirical support for the missing poor hypothesis, one

[3] See Lefebvre et al (2019) on how general equilibrium effects due to selection (i.e. excess mortality of the poor) can affect poverty measurement in an overlapping generations economy.

[4] For instance, unemployment, by reducing accidents at work, may reduce mortality differentials, while, at the same time, contribute to increase poverty.

can question the comparability of old-age poverty measures across states. Indeed, given that states are heterogeneous in the strength of the income-mortality gradient, and given that the income-mortality gradient has a negative and significant effect on old-age poverty measures, one can question the meaningfulness of comparing old-age poverty rates across states with unequal income-mortality gradients. This limited comparability of old-age poverty rates across states motivates us to use our regression results to calculate hypothetical old-age poverty rates while neutralizing the impact of the income/mortality gradient on the measurement of old-age poverty. We show that correcting for income-based selection effects tends to increase old-age poverty rates by between 1 and 3 percentage points. It is also shown that the comparison of old-age poverty across states is affected by heterogeneity in the income/mortality gradient across states. For instance, on the basis of standard headcount poverty rates, old-age poverty is larger in Massachusetts than in Maryland. However, once the missing poor bias is corrected, the ranking is reversed, and old-age poverty is lower in Massachusetts than in Maryland. Similar reversals arise for other states (e.g. New Jersey *vs* Nevada, Wisconsin *vs* Wyoming, etc.), suggesting that the missing poor bias affects the comparison of old-age poverty across U.S. states.

This paper is related to several branches of the literature. First, it is related to the literature on the "missing poor" problem, such as Kanbur and Mukherjee (2007) and Lefebvre et al (2013, 2018). Those papers are mainly theoretical, and, when turning to data, propose to correct poverty measures for selection biases by constructing counterfactuals: hypothetical income distributions are constructed while assuming that all individuals enjoy the survival conditions of the top income class. This paper complements those studies by adopting an econometric approach, whose goal is to provide empirical tests of the missing poor hypothesis. Another related literature is the one on the income/mortality gradient, to which we referred above. Whereas that literature is mainly descriptive, it documents a relation that is shown, in this paper, to have strong corollaries for poverty measurement. Thirdly, our paper is also related to the literature on the links between mortality and development, such as Acemoglu and Johnson (2007), Hazan (2009) and Cervelatti and Sunde (2011). While those papers examine, from a historical perspective, the link between life expectancy and GDP per capita, our paper adopts a cross-sectional approach to study a different relation, between life expectancy differentials and the prevalence of poverty.

The rest of the paper is organized as follows. Section 2 develops a model aimed at motivating our empirical explorations of the impact of mortality differentials between the poor and the non-poor on old-age poverty measurement. Section 3 presents the data. The estimation framework and the OLS estimates are presented in Section 4. The IV approach is developed in Section 5. Section 6 proposes to turn back to old-age poverty measures, by correcting these for the missing poor effect quantified in Section 5. Section 7 examines the robustness of our results to alternative specifications. Section 8 explores some potential mechanisms at work behind the missing poor phenomenon. Conclusions are drawn in Section 9.

2 Motivating theory

To frame the empirical analysis, we first develop a simple model of poverty measurement under income-based premature mortality, to examine how mortality differentials between the poor and the non-poor contribute to bias the measurement of poverty at the old age.

Let us consider an economy in which each cohort of individuals is a continuum of size 1.[5] For simplicity, human lifespan is composed of two periods. During the first period (young age), individuals earn some income level y^y. Survival to the second period (old age) arises with a probability $0 < \pi < 1$ that depends on the income level when being young. In case of survival, individuals enjoy an income y^o.

For the sake of simplicity, we assume that there exist two income levels, $\check{y}$ and $\hat{y}$, as well as a poverty line $z > 0$, with $\check{y} < z < \hat{y}$.

At the young age, a fraction $p > 0$ of individuals has income $y^y = \check{y}$, and thus lies in poverty, whereas a fraction $1-p$ has income $y^y = \hat{y}$, and thus escapes from poverty.

To keep the analysis simple, the survival rate to the old age takes only two levels, which depend on whether the income enjoyed at the young age is below or above the poverty line z:

$$\pi = \begin{cases} \check{\pi} \text{ if } y^y \leq z \\ \hat{\pi} \text{ if } y^y > z \end{cases}$$

We define the mortality differential as:

$$d \equiv \frac{\hat{\pi}}{\check{\pi}} > 1$$

The parameter d captures the strength of the income/mortality gradient. When the income-based mortality differential is low, d is close to 1, whereas when there is a strong mortality differential, d takes higher values.

There is some income mobility between the young age and the old age. The income mobility matrix M, defined conditionally on survival, is given by:

$$M \equiv \begin{pmatrix} \alpha & 1-\alpha \\ \beta & 1-\beta \end{pmatrix}$$

where $0 < \alpha < 1$ is the probability that an individual who is in poverty at the young age remains in poverty at the old age, whereas $0 < \beta < 1$ is the probability that an individual who is not poor at the young age falls into poverty at the old age. Given some hysteresis in income levels, the elements along the diagonal are higher than those outside the diagonal, implying that $\alpha > \beta$.

In our economy, the old-age headcount poverty rate H can be written as:

$$H = \frac{\alpha\check{\pi}p + \beta\hat{\pi}(1-p)}{\check{\pi}p + \hat{\pi}(1-p)} \tag{1}$$

[5] We make this hypothesis to use the law of large numbers.

Multiplying the numerator and the denominator by $\frac{1}{(1-p)\tilde{\pi}}$, the old-age headcount poverty rate can be rewritten as:

$$H = \frac{\frac{p}{1-p}\alpha + d\beta}{\frac{p}{1-p} + d} \tag{2}$$

This formula basically says that the old-age headcount poverty rate depends on (1) poverty at the young age (p); (2) income mobility (α and β); (3) the mortality differential between the non-poor and the poor (d).

Assuming, as an approximation, that the probability of falling into poverty at the old age β is low, and acknowledging that poverty at the young age is not a widespread phenomenon (i.e. p is low), so that $\frac{p}{1-p} << 1 < d$, one can, as a proxy, rewrite the old-age headcount poverty rate as:

$$H \approx \left(\frac{p}{1-p}\right)\frac{\alpha}{d} \tag{3}$$

Taking logs, we can rewrite the old-age headcount poverty rate as:

$$\log(H) \approx \underbrace{\log\left(\frac{p}{1-p}\right)}_{\text{initial poverty}} + \underbrace{\log(\alpha)}_{\text{effect of income mobility}} - \underbrace{\log(d)}_{\text{effect of mortality differential}} \tag{4}$$

This model suggests that, in theory, there are reasons to believe that a larger mortality differential d between the poor and the non-poor leads to lower measured old-age poverty, in line with the missing poor hypothesis. Having stressed this, it should be emphasized that this simple model abstracts from several forces that may be at work in real economies, and may invalidate the missing poor hypothesis. For instance, in real world economies, mortality differentials may affect the propensity to save, and, hence, capital accumulation, leading to changes in wages and in income levels. Thus, although this model points to a possible impact of mortality differentials on poverty measures, one needs to test empirically the missing poor hypothesis. This is the task of the next sections. For that purpose, our empirical strategy below is to estimate equations that are close to equation (4), and to test empirically whether the mortality differential has a statistically significant impact on the old-age poverty headcount ratio.

3 Data and descriptive statistics

We use recent estimates of the life expectancy by income across U.S. states, as released by the Health Inequality Project (see Chetty et al 2016). For each state, we have access to data of life expectancy computed at age 40 and by quartile of income, for men and women separately. As shown on Figure 1, the U.S. men in the fourth quartile of income live on average 10 years longer than those in the first quartile. The difference for women is smaller, but still the richest women live, on average, five years longer than the poorest women. Interestingly, the first

quartile life expectancy has, over the last 15 years, evolved in a much smoother manner than the life expectancy associated to the last income quartiles. Figure 1 tends thus to show that the income/mortality gradient has tended to become steeper over the last decade.

Figure 1: Life expectancy at age 40 by income quartile (years), males and females, U.S., 2001-2014.

These estimates of life expectancy by income quartile are used to define the mortality differential variable as the ratio between the life expectancy in the fourth and the first quartiles. This mortality differential indicator can be regarded as an indicator of the strength of the income/mortality gradient. The larger that indicator is, and the larger is the differential in survival conditions between the non-poor and the poor. Figures 2 and 3 show, respectively for men and women, the mortality differential across states in the U.S., for the year 2014. We observe a large heterogeneity, both for men and women, across U.S. states, with a lower differential on the West coast and a bigger differential in the center of the country. It should be stressed that the mortality differential is larger for men than women, for whom the variability of the mortality differential across states is lower, but remains sizeable.

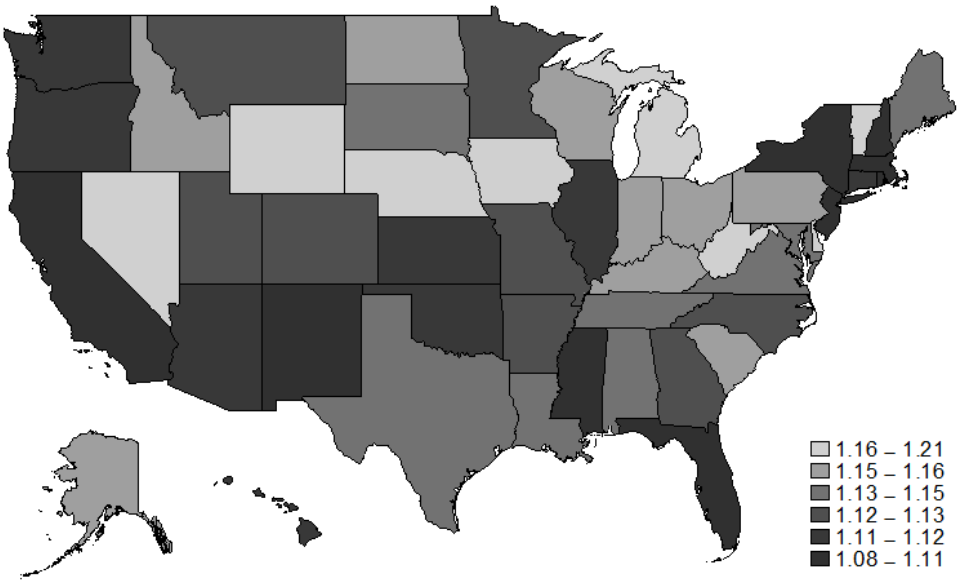

Figure 2: Mortality differential in the U.S., males, 2014.

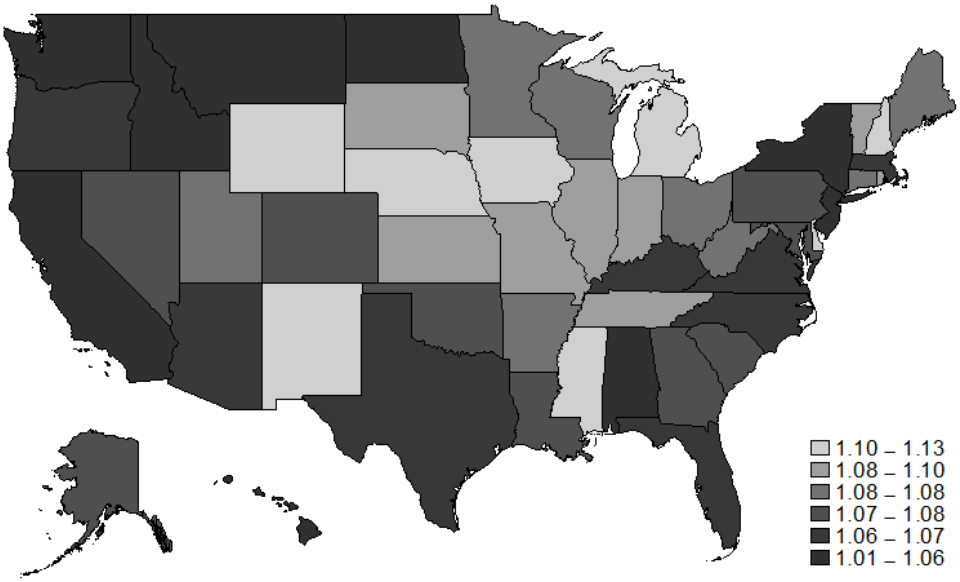

Figure 3: Mortality differential in the U.S., females, 2014.

Our aim is to relate the mortality differential to the level of poverty in each state. Data on poverty are obtained from the Current Population Survey of the

U.S. Census Bureau. We use the standard headcount poverty measure, which corresponds to the percentage of population in poverty at the state level. The official poverty threshold is a measure of need that does not vary across states, but that is updated for inflation using the Consumer Price Index (CPI-U). The official poverty definition uses money income before taxes and does not include capital gains or non-cash benefits (such as public housing, Medicaid, and food stamps).

Given that we are interested in estimating the impact of mortality differentials on poverty measurement, we use headcount poverty rates at age 65+. These data on the level of poverty are not available by sex. Figure 4 shows the headcount poverty rate for population aged 65+ by states for the U.S., in 2014. Figure 4 shows that there exists a large variation in old-age poverty across U.S. states. Poverty of the elderly is, in general, larger in the South than in the North, but with some important exceptions.

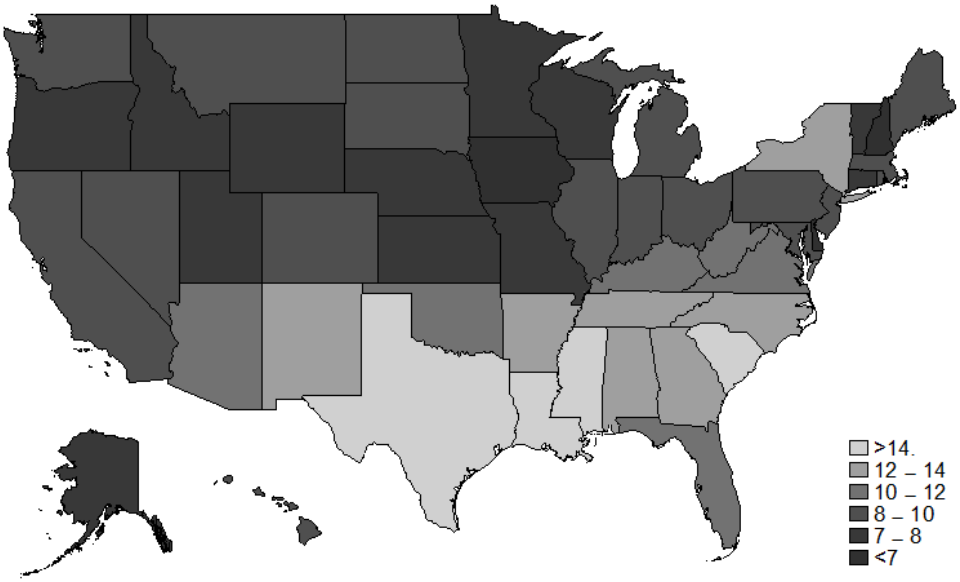

Figure 4: Headcount poverty rate (%), age 65+, United States, 2014.

How can one interpret the large heterogeneity in terms of old-age poverty across states? A naïve interpretation consists of stating that a larger value for the old-age headcount poverty rate in a state i than in a state j must necessarily account for a larger prevalence of old-age poverty in state i than in state j. However, this interpretation of Figure 4 assumes implicitly a kind of "full comparability" of poverty rates across states. The problem is that this "full comparability" assumption is a strong one. Actually, Figures 2 and 3 show that the states are actually quite heterogeneous in terms of the strength

of the income/mortality gradient. Some states are characterized by a steep income/mortality gradient, which implies that the population reaching the old age is strongly selected in terms of income. On the contrary, other states exhibit a weaker income/mortality relationship, so that income-based selection effects are here less sizeable. This heterogeneity in the strength of the income/mortality gradient may create serious interferences in the comparison of old-age poverty rates between states, which limit the degree of comparability of old-age poverty measures across states.

Actually, in the light of the substantial differences in terms of mortality differentials across states shown on Figures 2 and 3, one may wonder to what extent the sizeable differences in terms of old-age poverty rates shown on Figure 4 are due to selection effects varying across states. If the missing poor hypothesis is correct, then the old-age poverty rates shown on Figure 4 are not comparable across states. Indeed, in states with higher mortality differentials between the poor and the non-poor, selection effects are stronger, and tend to reduce the old-age poverty rate, whereas, in states with lower mortality differentials, selection effects are weaker, which pushes old-age headcount poverty rates up.

Hence, in order to be able to compare poverty rates across states in an accurate way, one must first test the missing poor hypothesis. As a starting point, Figure 5 displays a first empirical test of the missing poor hypothesis. It plots, for all U.S. states, the old-age poverty rate (65+) against the measure of mortality differential for the year 2014.[6] Since we only observe the poverty rate for both male and female together, we rely on an aggregate measure of the mortality differential that corresponds to the weighted average of gender-specific differential, where the weights are the relative population size. Quite interestingly, Figure 5 shows that there exists a decreasing relationship between the logarithm of the old-age poverty rate and the logarithm of the mortality differential. Clearly, states that exhibit a higher mortality differential tend to exhibit a lower old-age poverty rate, in line with the missing poor hypothesis.

[6] We use log value of our variable of interest because it is make the link with our theoretical model. On a more practical ground, it also allows to avoid that the results are too much impacted by extreme values. The same exercise is carried out for each year separately in the Appendix. Interestingly, the clear decreasing pattern is not observed for every year, making necessary the regression analysis where we control for various counfounding factors.

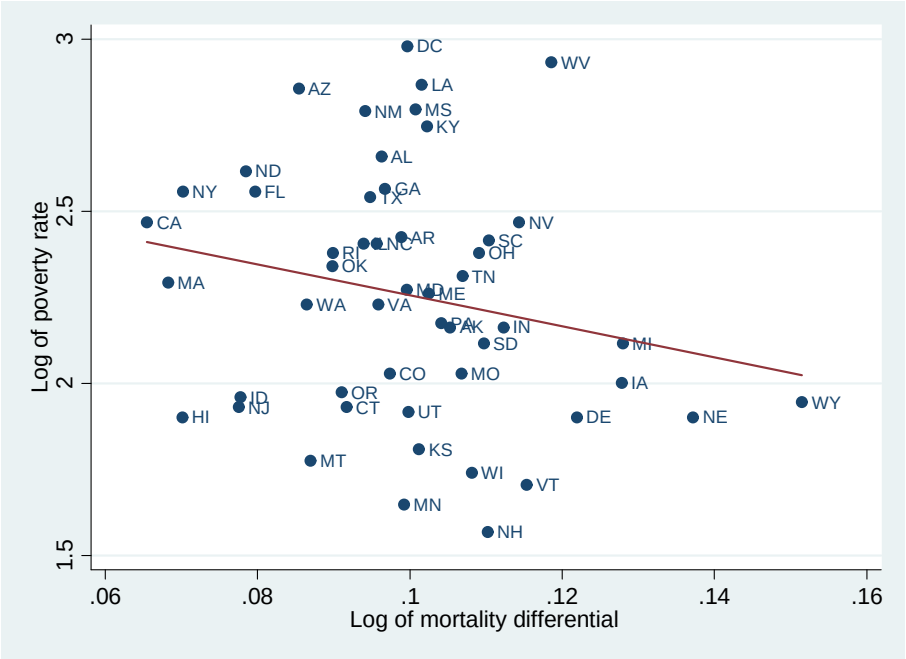

Figure 5: Poverty and mortality differential, 2014

However, one should be cautious before drawing conclusions from Figure 5. First, this figure does not necessarily imply that mortality differential has a causal effect on poverty. One cannot exclude a priori the possibility of some reverse causality, from poverty to mortality differentials. For instance, under capacity constraints in pro-poor programs, higher poverty rates, by leading to more congestion, reduce the capacity of those programs to achieve their goals, and, hence, lead to higher mortality differentials. In that case, the decreasing relationship between old-age poverty and mortality differential would be driven by the impact of poverty on mortality differentials, without any link with the missing poor hypothesis. Second, Figure 5 plots a relation between old-age poverty and mortality differential without any control. Actually, it may be the case that the plotted relation is driven by a third variable, which impacts both old-age poverty and mortality differentials. One should thus be cautious before interpreting Figure 5 as casting some light on the missing poor hypothesis.

In order to tackle these issues, we will, in the rest of this paper, carry out econometric regressions of old-age headcount poverty rate on our indicator of mortality differential, by controlling for a series of covariates that may also explain the variation of poverty across states and years. This is done using OLS estimates (Section 4) as well as an instrumental variable approach (Section 5), for the reasons exposed in the introduction.

4 OLS estimates

In order to test the missing poor hypothesis, that is, to test whether income-based mortality differentials affect old-age poverty, we use a state panel data model, controlling for fixed state characteristics and state differential in terms of longevity. In particular, we estimate the equation:

$$H_{st} = \beta MD_{st} + \gamma X_{st} + \alpha_s + \delta_t + \varepsilon_{st} \tag{5}$$

where H_{st} is the log of the old-age headcount poverty rate in state s in year t, that is, the proportion of individuals (aged 65+) in state s in year t who have income below the poverty threshold, MD_{st} is the log of mortality differential and α_s and δ_t are state and year fixed effects. X_{st} is a set of control variables that we detailed below and ε_{st} is the idiosyncratic error term.

As control variables, we include the log of per capita GDP expressed in 2009 dollars, available at the Bureau of Economic Analysis for each state and year, and the unemployment rate obtained from the Bureau of Labor Statistics. It corresponds to the annual average. Both variables allow us to control for the state-specific economic circumstances and the macroeconomic cycle. We also include the log of state welfare spending per capita, as calculated by the Urban Institute-Brookings Institution Tax Policy Center, to account for the size of public services. Finally, we introduce an indicator of income inequality within the population as given by the Gini index obtained from the US Census Bureau.

Table 1 reports OLS regressions as presented in equation (5). All regressions are weighted using state population in the state-year cell and we cluster the standard errors at the state level. We present here the results using the weighted mortality differential as the main explaining variable. Results based on the gender-specific differentials are presented in the appendix. Results are qualitatively similar.

Column (1) of Table 1 shows the results of the base model where we observe a clear and significant relationship between the mortality differential and the level of old-age poverty. The negative coefficient shows that when the income/mortality gradient is stronger, then the level of old-age poverty is reduced. This result is not much affected by the introduction of a series of covariates in column (2) to (4). The coefficient is between 1.6 and 1.9, depending on the econometric specification. When the differential increases by 1%, the level of poverty is reduced by 1.6% to 1.9% depending on specifications.

	(1) Basic	(2) + Controlling for Macroeconomic factors	(3) + Controlling for income distribution	(4) + Controlling for welfare expenditures
Mortality differential	-1.940**	-1.592***	-1.650***	-1.571**
	(0.944)	(0.552)	(0.550)	(0.609)
GDP per cap.		-0.300*	-0.338*	-0.308*
		(0.159)	(0.159)	(0.186)
Unemployment rate		-0.008*	-0.009*	-0.008
		(0.004)	(0.004)	(0.006)
Inequality			1.402**	1.426**
			(0.569)	(0.575)
Welfare exp. per cap.				-0.024
				(0.078)
Constant	4.675***	7.485***	7.291***	7.045***
	(1.030)	(1.663)	(1.658)	(1.847)
N	696	696	696	696
Adj. R-squared	0.589	0.593	0.590	0.592

Table 1: Results of OLS regressions.

Although our panel approach allows us to control for country-specific characteristics and time-varying factors, the causal interpretation of the relationship between mortality differentials and old-age poverty rates can be questioned on the grounds of endogenity issues. As we stressed in Section 1, it is indeed still possible that other variables which are varying across time and states are both related with the poverty rate and the life expectancy. It may also be the case that the direction of causation is reversed, and that it is the level of old-age poverty that explains the mortality differential. Thus the results of Table 1 do not allow us to conclude of any causal effect of the mortality differential on the level of measured old-age poverty. The goal of the next section is to develop an empirical strategy allowing us to identify and quantify the causal impact of mortality differential on poverty measurement.

5 IV estimates

In order to identify causal effects from mortality differentials to old-age poverty measures, we are completing the results obtained from OLS regressions by additional estimations relying on an instrumental variable, that is, a variable that is unrelated with our dependent variable but related with our explanatory variable. In our panel context, the challenge is to find a source of exogenous variation of the mortality differential across states.

The determinants of mortality are numerous, but can be categorized in three groups: socioeconomic status, genes and environmental conditions. The first is obviously the reason of our concern for reverse causality and the second is

rather difficult to measure, particularly at the aggregate level on which we are relying here. But a series of environmental characteristics are available and can be related to the level of mortality. In particular, it has been shown that mortality and morbidity are increasing with air pollution (see Anderson 2009, for a survey of evidence on air pollution and mortality). Furthermore the effect has been found to be more pronounced among persons with lower income and socioeconomic status (i.e. Forastiere et al 2006; Milojevic et al 2017; Di et al 2017).

In this section, we propose to exploit the level of air pollution measured at the state level as a source of exogenous variation of the mortality differential. The underlying intuition is that the extent of air pollution, by hitting poor and non-poor individuals in an asymmetric manner, is likely to strengthen the mortality differential, without affecting the old-age poverty rate. This identification strategy is valid only if the last condition is respected but we are confident that this exclusion restriction holds, especially once we control for macroeconomic factors as well as country fixed effects and government transfers.

We collected air pollution measures as provided by the America's Health Ranking from the United Health Foundation. The air pollution indicator corresponds to the average exposure of the general public to $PM_{2.5}$, i.e. particulate matter of 2.5 microns or less in size, as calculated by U.S. Environmental Protection Agency.

The first-stage relationship underlying the 2SLS estimates are shown in Panel A of Table 2. The results suggest that the level of pollution has a strong predictive power for the mortality differential, and that this predictive power is robust to various specifications regarding control variables. The results of previous epidemiological papers are thus confirmed, and we observe an increasing relationship between air pollution and the mortality differential. The effect remains significant and similar once we introduce other explaining variables.[7]

The 2SLS estimates are presented in Panel B of Table 2. The 2SLS coefficients are slightly higher than in the OLS model. Once we introduce control variables, the coefficient of the mortality differential increases a little to stabilize at a reduction of about 9% of the level of the old-age poverty rate for an increase of 1% of the mortality differential.

[7] In Table 2, we only present the estimates of the variables of interest. See the Appendix for a complete table and the effect of the other covariates.

	(1) Basic	(2) + Controlling for Macroeconomic factors	(3) + Controlling for income distribution	(4) + Controlling for welfare expenditures
	Panel A: First-stage			
Pollution	0.501***	0.387***	0.386***	0.383***
	(0.059)	(0.074)	(0.073)	(0.077)
F-stat for excluded instruments	71.90	27.29	24.71	24.78
P-value F-statistic	0.000	0.000	0.000	0.000
	Panel B: Second-stage			
Mortality differential	-6.948***	-8.224***	-9.385***	-9.422***
	(1.580)	(2.401)	(2.526)	(2.565)
N	696	696	696	696

Table 2: Results of IV regressions.

Table 2 provides a strong empirical support for the missing poor hypothesis. Clearly, the results of our IV regressions show that a 1 % rise in the strength of the income/mortality gradient tends to reduce the old-age headcount poverty rate by about 9 %. Thus, in states where poor individuals tend to die earlier than non-poor individuals, it is also the case that the measured old-age poverty rates are, *ceteris paribus*, lower. Thus the premature death of the poor pushes old-age poverty measures down. Table 2 suggests that this effect is far from negligible, and, based on our identification strategy, we can be confident that this effect from the mortality differential to the measure of old-age poverty is a causal effect, which goes from the mortality/income gradient to the old-age headcount poverty measure, and not the other way around.

As such, Table 2 provides also some empirical material against the - generally assumed - postulate of full comparability of poverty measures across space. If our calculations are correct, the strength of the income/mortality gradient varies across states, so that income-based selection effects also vary across states. This heterogeneity in the strength of selection effects across states tends to question the comparability of old-age poverty rates. In the light of our results, one can hardly claim, on the basis of Figure 4 alone, that old-age poverty is a more widespread phenomenon in one state than in another, since a lower value of the old-age headcount poverty rate may be due to a steeper income/mortality gradient in that state, leading more selection, that is, to lower poverty due to a lower (relative) survival of the poor in that state. Thus Table 2, by providing some support for the missing poor hypothesis, tends also to question the comparability of old-age poverty rates across states.

In the light of those results, one may wonder how one could correct or adjust existing poverty measures, in such a way as to make old-age poverty rates more comparable across states. This is the task of the next section.

6 Back to poverty measures

Whereas the previous section tends to question the comparability of old-age poverty measures across states, this section proposes to use the results of the regressions of Section 5 to build old-age poverty measures that are comparable, and do not suffer from the missing poor problem.

In this section, we use our results from regressions in Section 5 to simulate hypothetical old-age poverty rates, where the impact of mortality differential is, by construction, neutralized. For that purpose, we now impose, in a hypothetical way, that the income mortality differential variable takes, in each state, the value of 1, so that poor and non-poor individuals face the same survival conditions, and we use our regressions results to calculate the hypothetical poverty rates that would prevail in that hypothetical case. The simulated, hypothetical old-age poverty rate thus differs from the standard old-age poverty rate on an extent that depends on (1) how strong the income/mortality gradient is in that state; (2) the estimated causal impact of the income/mortality gradient on measured old-age poverty.

To present our results, Figure 6 shows, across the U.S. states, the size of the differential between, on the one hand, the hypothetical old-age poverty rate (correcting for the missing poor bias), and, on the other hand, the standard old-age headcount poverty rate. In every state, the differential is positive, that is, hypothetical old-age poverty rates are higher than the standard old-age poverty rates. This result is not surprising, since it was shown above that there exists a significant income/mortality gradient in U.S. states, and that this income/mortality gradient has a negative and statistically significant impact on old-age poverty measures. In the light of this, it does not come as a surprise that neutralizing the influence of the income/mortality gradient tends to lead to higher measures of old-age poverty. In other words, relying on standard (uncorrected) measures of old-age poverty tends to minor the prevalence of poverty at the old age, by focusing on measures that are subject to income-based selection effects.

Another important result shown on Figure 6 is that the magnitude of the differentials between the standard and the hypothetical old-age poverty rates varies across states, from about 1 percentage point to more than 3 percentage points. Note that the gap is larger in states where the mortality differential variable takes higher values, that is, in states where the income/mortality gradient is stronger. The gap between the simulated and the standard old-age poverty rate can be interpreted, for a given state, as a measure of the missing poor effect, that is, the impact of income-based selection effects on the measurement of old-age poverty.

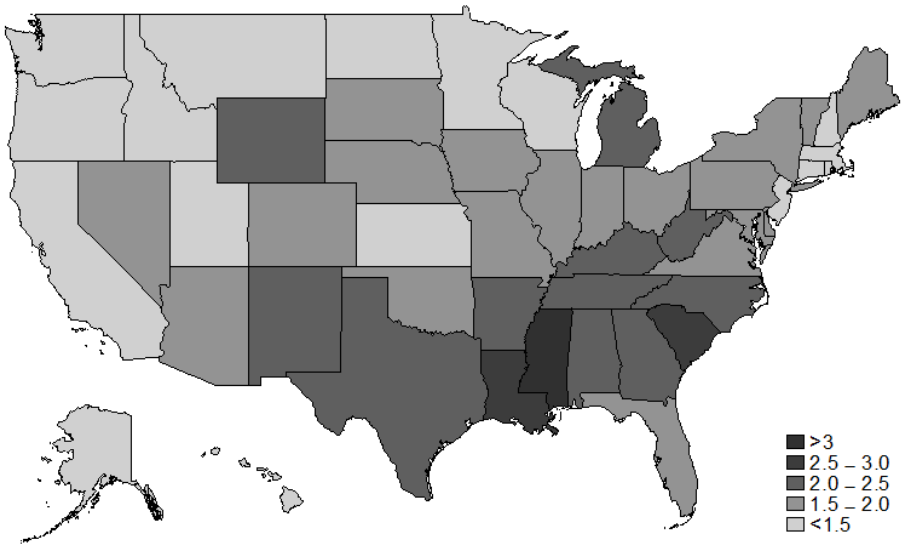

Figure 6: Differential between the old-age poverty rate corrected for the missing poor bias and the standard old-age poverty rate (in percentage points), U.S. states, 2014.

The varying size of the missing poor bias has important consequences regarding the comparison of old-age poverty across states. Quite interestingly, the ranking of states in terms of old-age poverty is affected by correcting selection effects. To see this, Figure 7 shows, state by state, the standard and the corrected old-age poverty rates. Figure 7 makes appear the occurrence of a large number of rank reversals between states (in terms of old-age poverty).

Take the example of Massachusetts (MA) and Maryland (MD). Standard headcount old-age poverty measures indicate that old-age poverty is slightly larger in Massachusetts than in Maryland. However, the mortality differential between the poor and the non-poor is much lower in Massachusetts than in Maryland. When computing hypothetical old-age poverty measures while neutralizing for the impact of the income/mortality gradient, it appears that old-age poverty is actually larger in Maryland than in Massachusetts. Another example of such a reversal of ranking across states is given by the comparison of old-age poverty in New Jersey (NJ) and Nevada (NV). Whereas old-age poverty is, on the basis of standard headcount ratios, higher in New Jersey than in Nevada, the correction of the selection bias leads to reverse that ranking. Another case of ranking reversal is given by the comparison of Wisconsin (WI) with Wyoming (WY). Those ranking reversals suggest that correcting for the missing poor bias affects the comparison of old-age poverty across U.S. states.

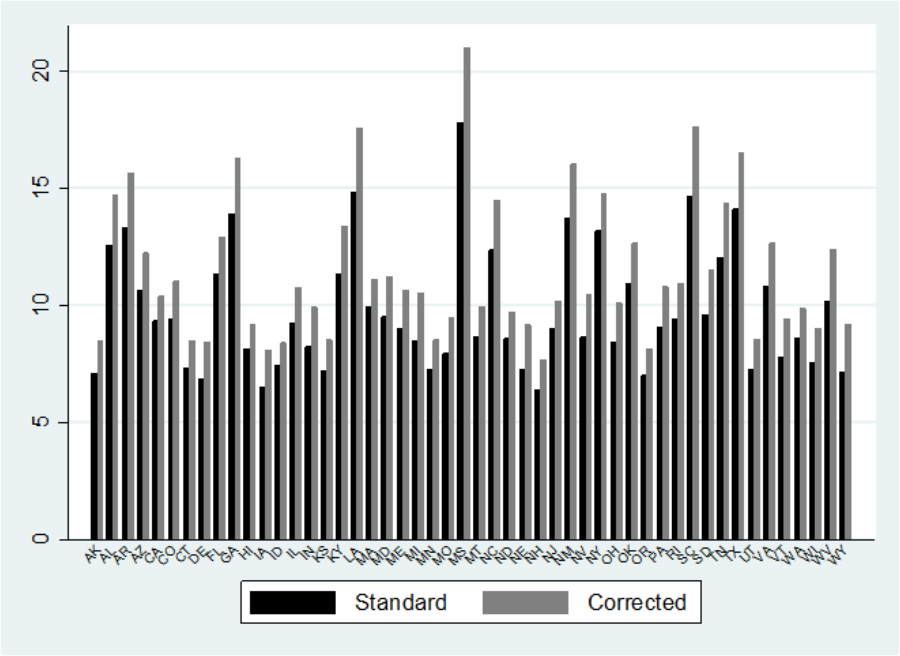

Figure 7: Standard old-age poverty rates and corrected old-age poverty rates (without mortality differentials), in percents, U.S. states, 2014.

To provide a more global view of the impact of correcting the missing poor bias on the comparison of old-age poverty across U.S. states, Figure 8 plots all states in terms of the standard old-age poverty headcount ratio (x axis, states being ranked from the lowest prevalence of poverty to the highest prevalence of poverty) and the levels of the corrected old-age poverty headcount ratio (y axis). Obviously, all points lie strictly above the 45° line, since the correction of the missing poor bias increases measured poverty in each state. But Figure 8 shows also how the correction of the missing poor bias affects the ranking of states in terms of old-age poverty. If the ranking were unaffected, all the points would define an increasing curve above the diagonal, whereas Figure 8 clearly shows that the curve defined by those points is not increasing, and reveals a large number of rank reversals in terms of old-age poverty.

In sum, this section shows that the missing poor phenomenon has an important impact on the measurement of old-age poverty in the U.S., and on the comparison of U.S. states in terms of old-age poverty. Whereas the missing poor bias is present in all states, it affects poverty measurement in varying degrees across states, depending on the strength of the income/mortality gradient. This section shows also that correcting for the missing poor bias affects the ranking of states in terms of old-age poverty.

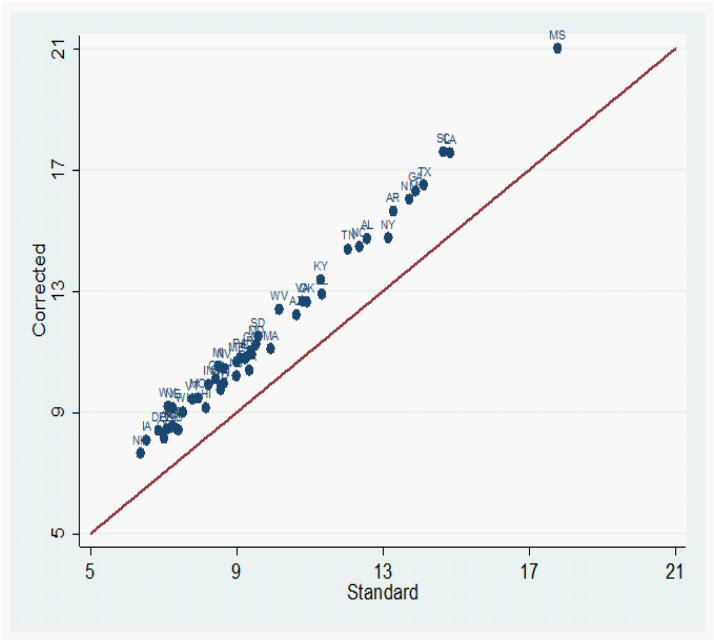

Figure 8: Standard *versus* corrected old-age poverty headcount ratios (in percents), U.S. states, 2014.

7 Robustness checks

7.1 The dependent variable

In order to check the robustness of our results, a first, natural, step consists of considering alternative indicators for the mortality differential variable. Up to now, we took, as an indicator, the ratio between the life expectancy in the last quartile and the life expectancy in the first quartile of the income distribution. In the following, we check that our results are consistent with other definitions.

For that purpose, we consider three alternative indicators of the mortality differential variable. First, in order to avoid that our results are driven by states in which life expectancy of the richest is higher than in other states, we propose to define the mortality differential indicator as the ratio between the life expectancy in the third and the first quartiles of the income distribution (L_3/L_1). Second, we propose another indicator, which is the ratio between the average life expectancy in the last two quartiles over the life expectancy in the first two quartiles ($(L_3 + L_4)/(L_1 + L_2)$). Thirdly, we define alternatively the mortality differential not as a ratio of life expectancies, but, instead, as a difference of life expectancies, between the last quartile and the first quartile of the income distribution, in such a way as to have a measure of the absolute

differential in survival conditions ($L_4 - L_1$).[8]

In Table 3, we report robustness tests when we change the definition of the mortality differential. The regressions are similar to the one presented in column (4) in Table 2. The first two specifications correspond to the case where we change the numerator and/or the denominator of the mortality differential variable. We observe estimates very similar to the results presented in Table 2, both in terms of significance and value of the coefficient. The last column displays rather different coefficients but similarly significant and of the same sign. Here the absolute definition of the differential changes the value of coefficient.

	(1)	(2)	(3)
	L_3/L_1	$(L_3+L_4)/(L_1+L_2)$	L_4-L_1
	Panel A: First-stage		
Pollution	0.313***	0.317***	31.959***
	(0.069)	(0.053)	(5.957)
F-stat for excluded instruments	20.82	35.33	28.27
P-value F-statistic	0.000	0.000	0.000
	Panel B: Second-stage		
Mortality differential	-11.557***	-11.434***	-0.113***
	(3.408)	(2.989)	(0.030)
N	696	696	696

Table 3: Robustness tests: mortality differential

In sum, Table 3 suggests that our main result - the negative effect of mortality differential on old-age poverty measures - is robust to the particular definition of the mortality differential variable that we use.

7.2 Samples

A second way to examine the robustness of our results consists in considering alternative subsamples. For that purpose, a first natural robustness check is to redefine our sample, to focus on a balanced panel. Actually, the sample of states that we used so far in our regressions is not perfectly balanced, since, for some years, data on poverty are not available for all states present in the mortality database. As shown in Table 4 (column (1)), focusing on the balanced panel ($N = 644$ instead of $N = 696$) does not affect our results: the mortality differential, instrumented by our pollution variable, has still a negative and statistically significant impact on old-age measured poverty.

[8] Although this indicator uses the same numbers as the one used in the benchmark case, these indicator convey different information.

	(1) Balanced panel	(2) Low income states	(3) Low and Middle income states
	Panel A: First-stage		
Pollution	0.384***	0.550***	0.551***
	(0.079)	(0.120)	(0.095)
F-stat for excluded instruments	23.25	20.84	33.40
P-value F-statistic	0.000	0.000	0.000
	Panel B: Second-stage		
Mortality differential	-9.213***	-8.359***	-7.058***
	(2.612)	(2.767)	(2.028)
N	644	331	522

Table 4: Robustness tests: samples

Another important robustness check concerns the partition of our sample of U.S. states into low income, medium income and high income states. Actually, one may wonder whether or not our results are driven by the mere fact that we are comparing states with unequal income levels. In order to check that our results are not driven by this, we divide our sample into three samples: low-income, medium-income and high-income states. States for which the GDP per capita is in the two first quartiles are classified as low income states, whereas states for which the GDP per capita is in the third (resp. fourth) quartile are classified as middle (resp. high) income state. As shown in Table 4 (columns (2) and (3)), restricting our sample to either low income states ($N = 331$) or to low and middle income states ($N = 522$) does not affect our results. The correlation between the measured old-age poverty and the mortality differential remains negative and statistically significant.

7.3 Additional covariates

Another important robustness check concerns the addition of covariates. Actually, U.S. states differ on many dimensions, and one may suspect that our correlations may be due to some kind of heterogeneity across states. Hence, in order to be sure that our regressions capture a significant relation between old-age measured poverty and the mortality differential and nothing else, it is important to add extra covariates in the analysis.

Obviously, a large number of additional variables could be potentially added on the right-hand side of our regression equation. In this subsection, we focus on two particular variables, which are likely to influence the comparison of old-age poverty across U.S. states: private insurance coverage and health expenditures. The addition of those covariates is motivated by the fact that health expenditures are quite sizeable at the old age, and that those expenditures increase

strongly with the age. As shown by De Nardi et al (2016), health spending more than double between the age of 70 and the age of 90. Medicare and the government pay for about 65 percent of it, about 20 percent is financed out-of-pocket, while 13 percent are paid by private insurance. In the light of those figures, one may expect that the proportion of individuals who are covered by private insurance may affect the comparison of U.S. states.

As shown in Table 5, controlling for the proportion of individuals covered by private insurance in the state does not substantially affect our results: it is still the case that the mortality differential variable affects negatively the measured old-age poverty. Moreover, controlling also for the amount of health spending in each state does not affect the robustness of our results.

	(1) With control for insurance	(2) With control for insurance and health spending	(3) With control for initial poverty
	Panel A: First-stage		
Pollution	0.423***	0.236***	0.379***
	(0.079)	(0.081)	(0.077)
F-stat for excluded instruments	28.69	8.43	24.38
P-value F-statistic	0.000	0.004	0.000
	Panel B: Second-stage		
Mortality differential	-9.921***	-15.048***	-9.341***
	(2.571)	(6.347)	(2.553)
N	696	696	696

Table 5: Robustness tests: additional covariates

Finally, another robustness check consists in adding, among the covariates, a measure of initial poverty. The underlying intuition is that, as shown in equation (4), one can expect, in theory, that the old-age poverty rate depends on poverty at earlier ages. In order to take this potential effect into account, we add, as an additional covariate, the measured poverty rate for population aged between 18 and 64 years, in each state, with a time lag of 20 years. As shown in Table 5 (column (3)), even when controlling for initial poverty, it remains true that the mortality differential affects old-age poverty negatively.

8 Mechanisms

The above analysis provides some empirical support for the missing poor hypothesis. Whereas the existence of a negative causal effect of the income-based mortality differential on the measured old-age poverty is a result on its own,

one may want to know more about the mechanisms at work behind that effect. Which mechanisms lie behind the missing poor phenomenon? Through which channels do poor individuals disappear from old-age headcount poverty rates?

Answering that question is complex, since there are potentially many mechanisms behind the missing poor phenomenon. For instance, one may think about policy variables, such as differences across states in terms of access to health care or to health care insurance. Despite the federal Medicaid program, it could be the case that states with lower measured old-age poverty are also characterized by a more restricted access to health care or health insurance (leading to a higher mortality differential). But other possible explanations of the missing poor phenomenon may consist of differences across states in terms of unhealthy lifestyles (alcohol, diet, etc.). It could be the case that states with lower measured old-age poverty are also characterized by a higher prevalence of risky lifestyles (leading, also, to a higher mortality differential).[9]

Besides the high number of potential channels, another difficulty lies in the fact that some mechanisms may not be captured by macro databases. For instance, a recent microeconomic study focussing on renal diseases and access to transplantation in France showed that, despite the highly egalitarian functioning of the French health system (which is supposed to treat all patients equally), there remain, at different stages of the medical process, significant inequalities in treatment across groups, depending on their socioeconomic status (Baudelot et al 2016). The existence of such micro effects in the U.S. cannot be excluded *a priori*, and macro data can hardly allow us to identify those effects.

A third difficulty is that one has also to be careful about issues of reverse causation: it cannot be excluded, in theory, that some channels work in both directions, from mortality differential to poverty measurement and *vice versa*. Thus one has to be extremely cautious in interpreting potential evidence supporting some particular mechanisms that may be at work.

In the light of those difficulties, one cannot reasonably claim to be able to identify a particular mechanism; instead, this section will present some empirical elements that may be interpreted as "clues" supporting, or, alternatively, rejecting, mechanisms at work behind the missing poor phenomenon.

8.1 Insurance coverage

A natural candidate mechanism behind the missing poor phenomenon consists in inequalities in health care access. Note that, if the missing poor phenomenon were driven by unequal access to health care (between the poor and the non-poor), then we would expect that, among states with the lowest measured old-age poverty, we would also find, *ceteris paribus*, a larger proportion of individuals with limited access to health care.

Note that poor individuals are eligible to the Medicaid program, and, as such, are not uninsured, and have access to health care. Although this seems

[9] Note that it is hard, without further analysis, to separate those two possible mechanisms, since both mecanisms lead to income-differentiated mortality.

to rule out *de facto* a "health care channel" for the missing poor phenomenon, one may want to check that restricted access health care within the rest of the population is not related to the prevalence of old-age poverty.

It is difficult to measure inequalities in health care access directly. One can nonetheless use the proportion of uninsured individuals in the population as a proxy measure of the extent to which access to health is limited in a given state. Using that proxy variable, the story behind the missing poor problem would go as follows: in states where a larger proportion of individuals are uninsured, access to health care would be more limited, which would lead to a lower measured old-age poverty through a selection effect.

Obviously, when studying the relation between insurance coverage and old-age measured poverty, one must be cautious about the possibility of reverse causality (i.e. from poverty to insurance coverage). In order to minimize the problems raised by reverse causality, our analysis uses lagged variables for insurance coverage. Figure 9 plots old-age poverty rates by states in 2016 against the proportion of the uninsured population by state in 2001.[10] The underlying intuition is that, although poverty in 2016 may affect insurance coverage in 2016, it is less likely to influence it 15 years before.

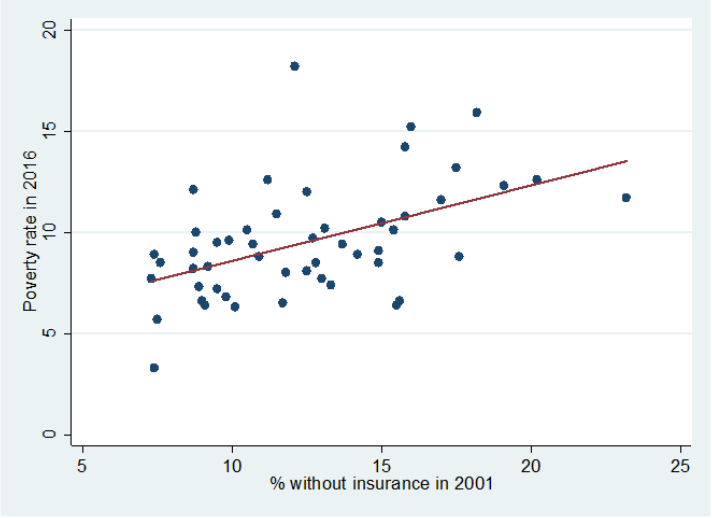

Figure 9: Percentage of uninsured individuals (2001) and old-age measured poverty (2016).

As shown in Figure 9, the relation between the (lagged) percentage of uninsured individuals in the population by states and the measured old-age poverty

[10] Sources: U.S. Census Bureau, 1-year American Community Surveys, 2018. The denominator includes the entire population of the state. The numerator includes individuals who are uninsured, in the sense that they are aged less than 65 (and thus non eligible to Medicare), not covered by Medicaid and not covered by private insurance.

is increasing. Thus Figure 9 does not support the existence of a "health insurance channel" behind the missing poor phenomenon. If limited insurance coverage were the mechanism driving the missing poor phenomenon, then there would be a *decreasing* relation between the proportion of uninsured individuals and the measured old-age poverty, unlike what appears on Figure 9.

8.2 Causes of death

Another approach consists of shifting the analysis from the study of overall mortality to the study of mortality by causes. Actually, studying mortality by causes is an indirect way to account for interstate heterogeneity in unhealthy lifestyles. If the missing poor phenomenon were driven by inequalities across states in terms of lifestyles, then it would also be the case that, among states with the lowest measured old-age poverty, we would find, *ceteris paribus*, a larger proportion of deaths related to causes associated to unhealthy lifestyles.

In order to examine the plausibility of the lifestyle channel, this section plots crude death rates by causes in 2001 against measured old-age poverty in 2016, for three distinct causes of death: alcohol related death, diabete-related death, and respiratory-related death.[11] As in the previous section, we use here time lags to try to minimize problems due to reverse causality.

Figure 10 shows a decreasing relationship between the (lagged) respiratory-related crude death rate and the measured old-age poverty. This decreasing relationship is not, of course, a proof that the missing poor phenomenon is driven by the "respiratory channel", but at least this constitutes a clue suggesting that this channel does not seem incompatible with the missing poor phenomenon. However, Figure 11, which shows an increasing relation between (lagged) alcohol-related death and old-age poverty, leads to the rejection of that "alcohol mechanism" as a driver of the missing poor phenomenon. Similarly, Figure 12 rejects the "diabete mechanism".

All in all, whereas the analysis of mechanisms behind the missing poor phenomenon would require another paper on its own, our macro analysis can nonetheless point to some clues suggesting that some mechanisms are more likely than others. Our analysis suggests that unequal health insurance coverage does not seem to be the driving force behind the missing poor problem, whereas some dimensions of lifestyles, leading to respiratory-related deaths, may play some role. Having stressed this, one should be extremely cautious when interpreting our results. The validity of the missing poor hypothesis is a robust result, but the identification of the underlying mechanisms is more complex, and the results presented here should be interpreted as clues rather than findings.[12]

[11] Our emphasis on alcohol-related deaths is motivated by empirical studies showing that this source of death is correlated with the socio-economic status (see Romelsjo et al 1996, Makela 1999 and Van Oers et al 1999). Our focus on respiratory diseases is motivated by empirical studies relating poverty to tobacco consumption, such as Flint and Novotny (1997) and Haustein (2006).

[12] We also carried out other tests concerning potential mechanisms, while also using a 15-year lag between the explanatory variable (measured in 2001) and the old-age poverty rate (measured in 2016). We found that health expenditures per capita are positively related to

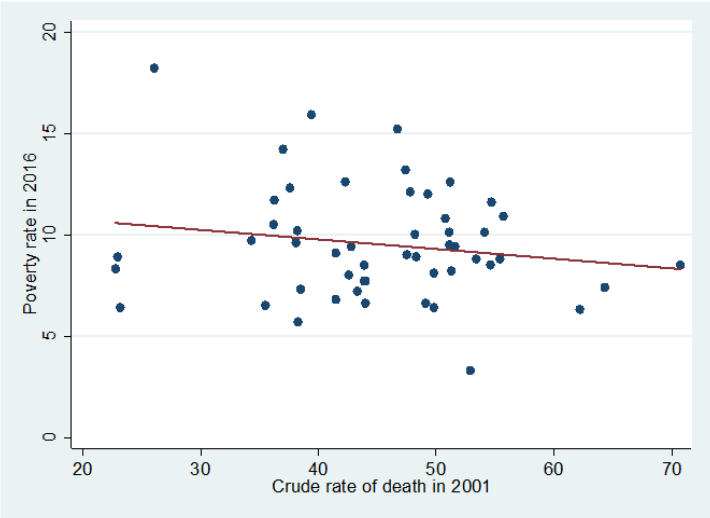

Figure 10: Respiratory related deaths, in per 100,000 (2001) and old-age measured poverty (2016), in percent.

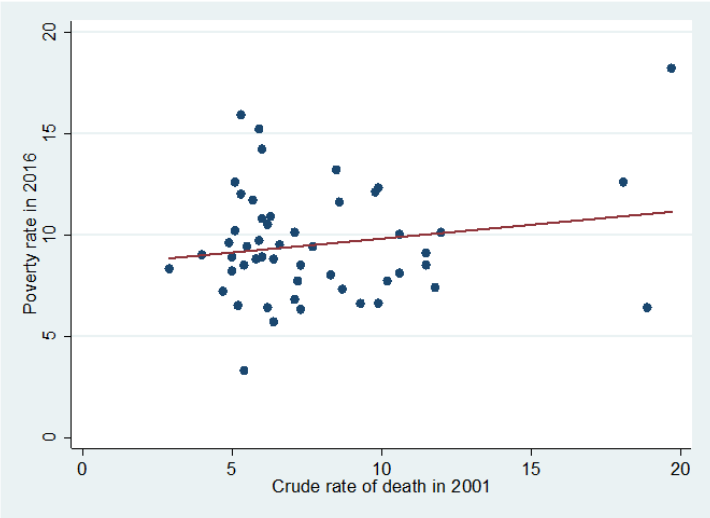

Figure 11: Alcohol related deaths (2001) in per 100,000 and old-age measured poverty (2016) in percent.

old-age povety measured 15 years later, whereas for nursing homes per capita we found a decreasing relationship with old-age poverty measured 15 years later. Those additional tests are available upon request.

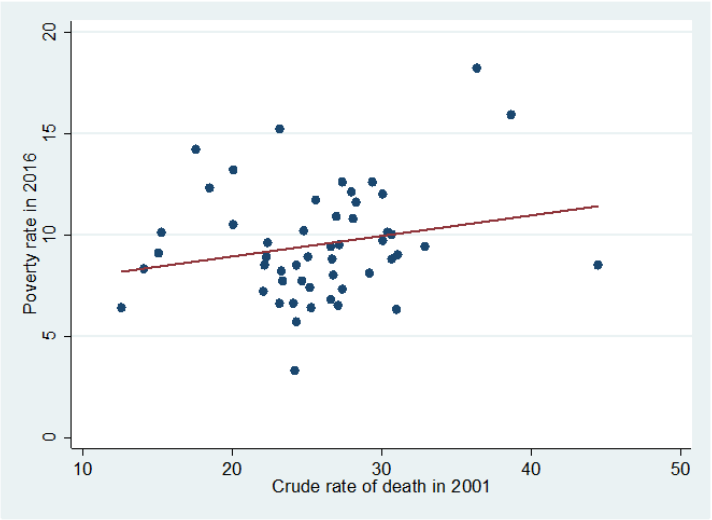

Figure 12: Diabetes related deaths (2001) in per 100,000 and old-age measured poverty (2016) in percent.

9 Conclusions

Although the existence of an income/mortality gradient is widely documented, there are few empirical attempts to test empirically the consequences of that gradient on the measurement of poverty at the old age. In particular, one may expect, in line with the missing poor hypothesis, that regions with a steeper income/mortality gradient tend also to exhibit lower old-age poverty measures, because of a stronger income-based selection, which excludes the poor from the population under study.

The goal of this paper was to provide an empirical test of the missing poor hypothesis. For that purpose, we used U.S. state-level data on poverty at age 65+ and life expectancy by income levels, and we tried to estimate the impact of variations in the mortality differential (measured by the ratio of the life expectancy of the first and the fourth income quartiles) on the headcount poverty rate at age 65+. We showed that the strength of the income/mortality gradient has a negative and statistically significant effect on the old-age headcount poverty rate, and that this effect is robust to various specifications concerning control variables. In order to address potential reverse causation issues, we also developed an IV approach, by instrumenting the mortality differential variable by means of air pollution statistics. Our IV regressions showed that a 1 % rise in the instrumented mortality differential causes a 9 % decrease in the old-age headcount poverty rate.

Given the large heterogeneity across states in terms of the strength of the in-

come/mortality gradient, and given the negative and significant impact of that gradient on old-age poverty measures, one can seriously question the comparability of standard old-age poverty rates. Actually, a lower measured old-age poverty may be due not to a lower prevalence of poverty, but to a stronger income-based selection, which excludes the poor from the old-age. In order to deal with this comparability problem, we proposed, in a second stage, to calculate hypothetical old-age poverty rates while neutralizing the income-mortality gradient (i.e. assuming that survival conditions are similar for all income quartiles). This thought experiment allowed us to show that neutralizing selection effects can lead to a rise of old-age headcount poverty rates by between 1 and 3 percentage points. Whereas those magnitudes may seem small, it should be stressed, however, that the large variation of the income/mortality gradient across states implies that correcting for the missing poor problem may significantly affect the comparison of states in terms of old-age poverty. In particular, we showed that the ranking of states in terms of standard old-age poverty rates may differ from the ranking of states in terms of hypothetical old-age poverty rates corrected for selection effects. Thus the missing poor problem is an important issue, that is not purely theoretical.

Our analysis provides thus some empirical support for the missing poor hypothesis, in the sense that a steeper income/mortality gradient leads, through stronger income-based selection, to a lower poverty rate at the old age. However, it should be stressed here that our results should be interpreted with caution. Actually, our empirical test of the missing poor hypothesis relies on highly aggregated, macro datasets. As such, our analysis does not provide direct, microeconometric evidence supporting the missing poor hypothesis, but, rather, some form of indirect macroeconomic evidence compatible with that hypothesis. In some sense, this paper provides a "first pass" for the missing poor hypothesis.

Having stressed this, our results are nonetheless important, since these point to a significant measurement problem for poverty. If our calculations are correct, the missing poor hypothesis is true, and standard old-age poverty measures are biased downwards due to selection bias associated to mortality differentials between the poor and the non-poor. Given that those biases vary across states, the existence of an income/mortality gradient varying across states tends to question the comparability of standard poverty measures at the old age.

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11 Appendix

11.1 Mortality differential and poverty by year

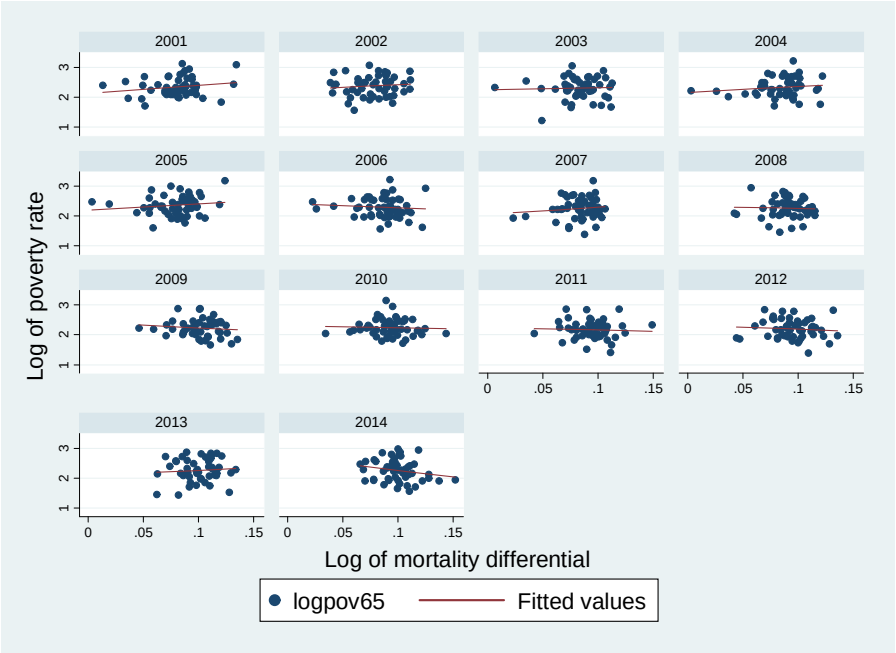

Figure A1: Poverty and mortality differential

11.2 OLS regressions by gender

	(1) Basic	(2) + Controlling for Macroeconomic factors	(3) + Controlling for income distribution	(4) + Controlling for welfare expenditures
Mortality differential	-1.296*	-1.057**	-1.088*	-0.975**
	(0.755)	(0.429)	(0.427)	(0.456)
GDP per cap.		-0.313**	-0.351**	-0.283
		(0.159)	(0.159)	(0.186)
Unemployment rate		-0.009**	-0.010**	-0.008
		(0.004)	(0.004)	(0.006)
Inequity			1.372**	1.429**
			(0.570)	(0.576)
Welfare exp. per cap.				-0.053
				(0.075)
Constant	4.022***	7.084***	6.878***	6.369***
	(0.852)	(1.651)	(1.647)	(1.797)
N	696	696	696	696
Adj. R-squared	0.587	0.588	0.591	0.591

Table A1: OLS regressions for men.

	(1) Basic	(2) + Controlling for Macroeconomic factors	(3) + Controlling for income distribution	(4) + Controlling for welfare expenditures
Mortality differential	-1.152**	-0.900**	-0.939**	-0.803*
	(0.528)	(0.440)	(0.438)	(0.463)
GDP per cap.		-0.353**	-0.392**	-0.300
		(0.157)	(0.158)	(0.187)
Unemployment rate		-0.009**	-0.011**	-0.007
		(0.004)	(0.004)	(0.006)
Inequality			1.375**	1.447**
			(0.571)	(0.577)
Welfare exp. per cap.				-0.068
				(0.074)
Constant	3.772***	7.264***	7.071***	6.390***
	(0.556)	(1.669)	(1.665)	(1.825)
N	696	696	696	696
Adj. R-squared	0.586	0.587	0.590	0.590

Table A2: OLS regressions for women.

11.3 IV estimates

	(1) Basic	(2) + Controlling for Macroeconomic factors	(3) + Controlling for income distribution	(4) + Controlling for welfare expenditures
	First-stage			
Pollution	0.501***	0.387***	0.386***	0.383***
	(0.059)	(0.074)	(0.073)	(0.077)
GDP per cap.		0.029**	0.029**	-0.024**
		(0.013)	(0.013)	(0.013)
Unemployment rate		0.001***	0.001***	-0.008*
		(0.001)	(0.001)	(0.001)
Inequality			0.005	-0.022
			(0.051)	(0.042)
Welfare exp. per cap.				0.052***
				(0.006)
Constant	1.054***	0.743***	0.741***	0.986***
	(0.005)	(0.137)	(0.137)	(0.131)
F-stat for excluded instruments	71.90	27.29	24.71	24.78
P-value	0.000	0.000	0.000	0.000

Table A3. IV regressions (first stage)

	Second-stage			
Mortality differential	-6.948***	-8.224***	-9.385***	-9.422***
	(1.580)	(2.401)	(2.526)	(2.565)
GDP per cap.		0.073	0.087	0.084
		(0.235)	(0.234)	(0.236)
Unemployment rate		0.006	0.006	0.006
		(0.007)	(0.007)	(0.007)
Inequity			1.744**	1.732**
			(0.769)	(0.770)
Welfare exp. per cap.				-0.084
				(0.072)
Constant	10.140***	10.730***	11.021***	11.033***
	(1.727)	(2.409)	(2.437)	(2.424)
N	696	696	696	696

Table A4. IV regressions (second stage)