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ABSTRACT

Standard measures of poverty may reveal nothing about whether the poorest of the poor are being lifted-up or left-behind, yet this is a widespread concern among policy makers and citizens. The paper assesses whether public spending on social protection benefits the poorest and hence lifts the floor, and what role economic development plays. Evidence is presented for the developing world and the US. Across developing countries, a higher mean income comes with a higher floor. The bulk of this income effect is direct rather than via higher spending on social protection. That spending generally lifts the floor though this is mainly due to social insurance; on average, social assistance adds only 1.5 cents per day to the floor. Turning to the US, the paper finds that the floor has been sinking over the last 30 years, associated with an inequitable growth process. Food stamp spending partially compensates the poorest, and helped stabilize the floor in the wake of the 2008 financial crisis. The poorest in the US gain more from food stamps than average spending on food stamps, though the program's impact on the floor per $ spent has fallen over time.

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1. Introduction

Development policy discussions often emphasize the need to assure that the poorest are not being "left behind." For example, the title of the 2017 Policy Paper of the UK's Department for International Development (DFID) is "Leaving No One Behind: Our Promise," and the paper's main theme is DFID's goal of prioritizing "the poorest of the poor." One can find many prominent examples of public claims suggesting that DFID's concern is neither isolated nor unjustified—claims that the poorest are in fact being "left behind."[2]

This concern about the poorest echoes an important school of moral philosophy that has argued that we should judge a society's progress by its ability to enhance the living standards of the poorest, as exemplified by the principle of justice proposed by Rawls (1971). This principle is often advocated for practice, including in the UN's Sustainable Development Goals, which advocate effort to "ensure no one is left behind" (UN, 2017), and the Swiss Constitution, which states that: "…the strength of a people is measured by the well-being of its weakest members."

But how can we implement this idea, and test whether or not the "poorest are left behind"? This can be interpreted as referring to the lowest level of material living. That lower bound can be called the "floor." If the poorest have been left behind then the floor will have stayed put; if the poorest benefit then it will be lifted. This idea of the floor should not be confused with the "biological floor." Human physiology makes it highly plausible that there is a biological minimum, given that there are strictly positive nutritional requirements for basal metabolism and normal activities. However, economic development and the institutions of (private and public) redistribution can in principle assure that the lower bound is lifted above the biological floor. The question is whether, and to what extent, that happens in reality.

The idea of lifting the floor above the mere biological minimum for survival has long played a prominent role in social policy discussions. Direct interventions have been used against poverty in rich countries and are becoming popular in poorer ones. The policies concerned are of various types with various labels, including "anti-poverty programmes," "targeted interventions,"

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[2] For example, in 2011, the U.N.'s Secretary-General Ban Ki-moon claimed that: "The poorest of the world are being left behind. We need to reach out and lift them into our lifeboat." (This was at the launch of the report: United Nations, 2011). Similarly, in 2014, the International Labor Organization's Director-General, Guy Ryder, wrote that "Poverty is not yet defeated. Far too many are being left behind." Also, the Vatican's representative to the United Nations claimed in 2015 that the poorest of the world are being left behind (James, 2015).

"social safety nets," "social assistance," "social insurance" and "social protection." Following the World Bank's usage, we shall put them all under the heading "social protection" (SP).[3] SP coverage in the developing world has expanded rapidly over the last 20 years, with one or more programs now found in most countries (Ravallion, 2017). In terms of population coverage, the two largest social assistance programs are clearly China's *Di Bao* program (a cash program targeted to the poor) and India's *National Rural Employment Guarantee Scheme* (a workfare scheme), both of which can be interpreted as efforts to lift the floor—to assure a minimum standard of living above the biological floor. However, there are continuing concerns that such efforts are not having much impact on poverty.[4] Various reasons are given including a lack of political will, weak administrative capacity for policy implementation, ignorance of their rights among poor people, and social stigma associated with targeted programs.

The main approach to assessing the poverty impacts of social policies has been to compare measures of poverty before and after policy intervention. For transfer policies, this is typically done by comparing measures based on the observed gross income distributions with those for the distributions obtained by subtracting reported transfer receipts for each household.[5] This can be called the "counting approach." There is a large literature on that approach, in which various axioms for a desirable poverty measure have been proposed. The aggregate measure is usually a population-weighted average of individual measures across the population (counting the non-poor as having zero poverty); Atkinson (1987) characterizes the class of additive measures in the literature. The most widely used measure in practice is the headcount index, giving the proportion of the population living below the poverty line. Higher weight can also be

[3] Public spending on SP is sometimes called "social spending" although this term might also be taken to include public health and education spending. To avoid any confusion we will use the more precise term "SP spending."

[4] With regard to the two examples above, on the *Di Bao* program see Ravallion and Chen (2015) and on the India program see Dutta et al. (2014). Evidence on the under-coverage of poor people in cash transfer programs (in Latin America) can be found in Robles et al. (2015). Casual observations of specific antipoverty policies in practice have also expressed concerns about leaving the poorest behind. For example, an article in the *Economist* magazine (2015) on China's poor-area development programs asked how much those programs have helped reduce poverty, and the article's answer referred to how little living standards had risen in one clearly very poor village (in Shanxi) that had apparently been left behind.

[5] This has been the main approach in the literature on benefit-incidence analysis (Kakwani, 1986; van de Walle, 1998). Recent examples include Lindert et al. (2006), Martinez-Vazquez (2008), the country studies summarized in Lustig et al. (2014) and Piketty et al. (2018). There is also a literature on the effects of social spending on health and education outcomes for which a different approach is required since one cannot simply net out the gains from social spending; examples include Anand and Ravallion (1993), Bidani and Ravallion (1997) and Haile and Nino-Zarazua (2018). Here we focus on (cash or imputable in-kind) transfers.

put on poorer people. An example is the Foster-Greer-Thorbecke (FGT) (1984) squared poverty gap (SPG), which weighs poverty gaps by poverty gaps in forming the aggregate measure.

The literature on the counting approach has found that absolute poverty measures tend to be lower in countries with a higher mean income, and that these measures tend to fall in growing economies.[6] There is also evidence that social protection spending has generally reduced poverty when measured using the counting approach. For example, in the cross-country data set that we use in this paper we find that SP spending in developing countries roughly halves the average poverty gap index (the agregate gap below the poverty line normalized by the line). Despite often-heard claims to the contrary, the evidence also suggests that America's antipoverty programs have reduced poverty, though there has been debate on the extent of this impact.[7] The counting approach suggests that, as a rule (and there are exceptions), economic development and social protection tend to reduce poverty.

While the counting approach is of obvious interest and importance, it does not adequately address prevailing concerns about whether the poorest are being left behind. To illustrate the inadequacy of prevailing approaches, Figure 1 shows two pairs of cumulative distribution functions with and without a social protection policy (or before and after economic growth). There is first order dominance in both panels (a) and (b)—an unambiguous change in the aforementioned class of standard (additive) poverty measures. But there is a big difference. In (a), the floor has not risen, but in (b) it has. The poorest in (a) have been "left behind." Existing poverty measures (including those that give higher weight to poorer people) can readily fall without any change in the floor, as in Figure 1, panel (a). Instead, we need to measure the floor, side-by-side with the counting approach.

This is not easy. There are limits to how well we could ever hope to measure the floor from standard household surveys. The sampling frame is typically those who live in some form of dwelling, so homeless people and those living in institutions (such as worker dormitories or prisons) are under-represented or even excluded, and they could well be concentrated among the poorest stratum. For example, recent rural migrants in cities living in dormitories or slums could well be under-represented. Ideally one would use something like the lower bound of mean household consumption or income, measured accurately over a much longer period than what is

[6] For an overview of the evidence see Ravallion (2016, Chapter 8).
[7] See, for example, Meyer and Sullivan (2012) and the comment by Hoynes (2012).

typically measured with survey data. If we were to know the true consumption observed over a long enough period in panel data for a large-enough sample we could reliably estimate the floor directly as this long-run mean. But that is not the data normally available. And we must recognize the existence of measurement errors in the cross-sectional survey data available for most countries. There are also likely to be transient effects in those data, whereby observed incomes (or consumption expenditures) in a survey fall temporarily below the floor (such as due to seasonality or a spell of illness), but recover later. Given the measurement errors and transient factors, there is a non-negligible chance that the observed consumption or income of potentially anyone within some stratum of low observed values could in fact be the level of the floor. Some form of averaging is clearly necessary.

Here we follow the approach to measuring the floor in Ravallion (2016b). The essential idea is to estimate the floor by taking a weighted mean of the observed consumptions or incomes (depending on the data) within some stratum that is agreed to be poor. The weights reflect our uncertainty about who is the poorest, but highest weight is given to the lowest observed value, declining thereafter within the stratum. Ravallion (2016b) did not study national values of the floor or the role played by social protection. Here we do national estimates, and explore how the floor varies with social protection spending and what role a higher mean income plays.

The following section outlines our approach to measuring the floor, while Section 3 discusses how social protection and economic development might impact the floor. We then turn to illustrative evidence. First we look at cross-country evidence on the level of the floor across developing countries, how much it responds to aggregate public spending on SP, and the differences between countries in the efficacy of that spending in lifting the floor (Section 4). Then we turn in Section 5 to the US where we focus on a single program, the Supplemental Nutrition Assistance Program (SNAP), including SNAP's role in preventing the floor from falling in a period of inequitable economic growth and financial crisis. Section 6 concludes.

2. Measuring the floor

We need an estimator for the floor that does not require panel data but can be implemented with (single or repeated) cross-sectional surveys, while recognizing that the lowest observed income in such a survey is unlikely to be a reliable indicator. We follow the approach in Ravallion (2016b), which this section summarizes for completness.

Without loss of generality, we can postulate that any observed income level within a stratum of poor people has some probability of being the floor. These probabilities are not data, but there are some defensible assumptions we can make in lieu of the missing data. While we are uncertain as to whether the lowest observed value is the floor, it seems reasonable to assume that this value has the highest probability of being the floor—that our data are sufficiently good to believe that the probability is highest for the person who appears to be the worst off. It also seems reasonable to assume that the probability of being the poorest household declines as the observed measure of income rises. And beyond some point it would be reasonable to say that there is no chance of finding the true floor.

We have an observed distribution of household income, y_i, i=1,…n, the quantiles of which are denoted $q(p)$; the mean is denoted m and the median is denoted $y^{med} = q(0.5)$. There is a corresponding (unobserved) distribution y_i^* after eliminating the transient effects and measurement errors. Let the floor of the y_i^* distribution be denoted $y_{min}^* = \min(y_i^*, i = 1, \dots n)$. We can treat y_{min}^* as a random variable, with a probability distribution given the data. The task is to estimate the mean of that distribution based on the observed incomes. We can write this as:

$$E(y_{min}^*|y) = \sum_{i=1}^{n} \phi(y_i)y_i \quad (1)$$

Here the probability that person i, with y_i, is the worst off person is $\phi(y_i)$. This attains its maximum value for $y_{min} = \min(y_i\ , i = 1, \dots n)$ and then falls monotonically with y_i, until it reaches zero at some threshold z, above which there is no chance of someone with that income being the poorest. Those living below the threshold z can be called the "reference group." (In applications, it is natural to use prevailing poverty lines for z, but this is not essential.) The specific functional form satisfying these assumptions proposed by Ravallion (2016b) is:

$$\phi(y_i) = k(1 - y_i/z)^{\alpha} \text{ for } y_i \leq z \quad (2)$$

$$= 0 \text{ for } y_i > z$$

Here there are three parameters, k, α and z, all positive constants. The k parameter assures that the probabilities add up to unity, which requires that $k = 1/(nP_{\alpha})$ where P_{α} is the FGT measure:

$$P_{\alpha} = \frac{1}{n}\sum_{y_i \leq z}(1 - y_i/z)^{\alpha} \quad (3)$$

SPG is for $\alpha = 2$ and the poverty gap (*PG*) index is for $\alpha = 1$. However, the interpretation of α is different to that of the FGT index. Here α determines how fast the chance of being the poorest person falls with y, rather than the aversion to inequality among the poor, as in the FGT index.

We can then derive the following formula for the expected value of the floor:

$$E(y^*_{min}|y) = z(1 - P_{\alpha+1}/P_\alpha) \ \ (\alpha \geq 1) \tag{4}$$

For example, if we assume that the probability of being the worst off person falls linearly with *y* up to *z* ($\alpha = 1$) then the expected value of the floor is $z(1 - SPG/PG)$. (Note that $\alpha = 0$ is ruled out by our assumption that the probability falls as *y* increases among those with $y_i \leq z$.[8]) We will call $(1 - P_{\alpha+1}/P_\alpha)$ the "floor ratio." Note that the P_α poverty measures and the threshold *z* are all measured on the observed distribution of income. Thus the proposed measure of the floor is operational, in that it can be implemented on readily available data.

Notice that there is nothing to guarantee that a specific process of poverty-reducing growth, or a specific spending program that benefits poor people, lifts the floor, based on this measure. From (4), for a fixed *z*, lifting the floor requires a larger proportionate reduction in $P_{\alpha+1}$ than P_α, i.e., a greater response of the more distribution-sensitive measure.[9] For example, for $\alpha = 1$, and denoting the mean of the observed distribution by *m*, we have:

$$\frac{\partial \ln E(y^*_{min})}{\partial \ln m} = \left(\frac{\partial \ln PG}{\partial \ln m} - \frac{\partial \ln SPG}{\partial \ln m}\right)\left(\frac{SPG/PG}{1-SPG/PG}\right) \tag{5}$$

Later we address the empirical question of how the floor responds to a higher mean. But first we need to address some theoretical issues that arise when we introduce social protection spending.

3. Social protection, economic development and the floor

We now explore further how one might expect the floor to respond to both economic development—defined as a rising mean income—and public spending on social protection.

Introducing social protection: We now distinguish the pre-transfer floor (y^{*pre}_{min}) from its post-transfer value (y^{*post}_{min}). We are interested in how y^{*post}_{min} varies with both SP spending per capita, denoted τ, and overall economic development as measured by the mean *m* of the observed distribution (*y*). It is assumed that SP spending is financed by domestic taxes, so that *m* is the same before and after transfers.

[8] If one uses $\alpha = 0$ then every consumption below *z* is deemed equally likely to be the lowest, so *z*(1-*PG/H*) is the equally-weighted mean for the poor, where *H* is the headcount index.

[9] This is a natural consequence of putting higher weight on lower observed incomes when calculating the floor.

We can start by thinking of y_{min}^{*pre} as being determined by how the overall mean, *m*, is shared within an economy, while the gain in the floor from SP spending, $y_{min}^{*post} - y_{min}^{*pre}$, is determined by how that spending is shared. This suggests a separable structure of the form:

$$y_{min}^{*post} = \vartheta(m) + \varphi(\tau) \tag{6}$$

Here $\vartheta(.)$ and $\varphi(.)$ are the sharing functions determining the pre-transfer floor and the gains from SP respectively. These functions need not be increasing; for example, the nature of the growth process may put downward pressure on the floor. A special case is when the poorest receive the mean SP spending, which we test as the null hypothesis that:

$$y_{min}^{*post} = y_{min}^{*pre} + \tau \tag{7}$$

There are numerous reasons why $y_{min}^{*post} - y_{min}^{*pre}$ could differ from τ, including successful efforts at targeting the poorest ($y_{min}^{*post} - y_{min}^{*pre} > \tau$), administrative costs, losses due to corruption, or social exclusion of the poorest (all resulting in $y_{min}^{*post} - y_{min}^{*pre} < \tau$).

Our empirical analysis follows standard practice in benefit-incidence analysis of estimating the pre-transfer distribution by subtracting transfers received at the household level. This ignores behavioral responses such as through saving, labor supply or private transfers. In defense, it might be argued that strong behavioral responses are unlikely among the poorest, who have the least scope for substitution. However, that might be considered a strong assumption.

We can provide a partial test of that assumption, which can be thought of as a consistency check on our empirical analysis. The transfer received by the poorest is denoted τ_{min}. If there are behavioral responses by the poorest then $\hat{y}_{min}^{*pre} = y_{min}^{*post} - \tau_{min}$ will underestimate the true value, y_{min}^{*pre}. The extent of the error due to behavioral responses is $b \equiv y_{min}^{*pre} - \hat{y}_{min}^{*pre} \geq 0$. Our test assumes that: (i) the true value of y_{min}^{*pre} is a function of the mean, *m*, as discussed above, and (ii) the behavioral effect *b* is non-decreasing function of mean spending. Thus:

$$\hat{y}_{min}^{*pre} = \vartheta(m) - b(\tau) \quad (b'(\tau) \geq 0) \tag{8}$$

The test is then to see if there is a partial correlation between the estimated pre-transfer floor and mean spending at a given value of mean income. Intuitively, when it is correctly measured, $\hat{y}_{min}^{*pre}$ should not vary with the level of SP spending at a given mean income.

The separability in (6) might be considered a strong assumption. A higher mean income may well come with administrative capabilities (including better information systems) that allow governments to better reach the poorest. To see how, suppose that economic development brings structural changes such that a rising share of national income is derived from formal-sector activities amenable to taxation. Engels Law implies this as long as the income elasticity of demand for informal sector activities is less than unity. Given that agriculture is the main informal sector in developing countries it is reasonable to assume that economic growth in such countries comes with formalization, generating greater administrative capability including for effective SP. Then it can be expected that economic development allows higher public spending on SP and supports a greater capacity to make that spending effective in reaching the poorest. To give another example, lack of knowledge about how to access public programs has often been identified as a factor weakening the coverage of poor people by social protection policies.[10] At the same time, economic development tends to come with higher literacy rates, which can be expected to promote greater knowledge, and greater efficacy in dealing with public administrations. Then the marginal gains to the poorest from higher SP spending will tend to rise with mean income when comparing different countries.

Instead of equation (6) we write the relationship in the more general form:

$$y_{min}^{*post} = f(\tau, m) \tag{9}$$

Here f is some (smooth) function. (Following the discussion in the previous section, we do not assume that this function is increasing in its arguments.) So the pre-transfer floor is $y_{min}^{*pre} = \vartheta(m) = f(0, m)$. We shall test separability. When the cross-partial derivative $f_{\tau m}$ is positive we will say that there is weak complementarity. The degree of complementarity plays a role in how economic development impacts the floor, as discussed further below.

It is also of interest to know how much differences in the impact of SP on the floor stem from differences in the overall level of spending versus differences in transfer efficiency. For this purpose we measure what we term Floor Transfer Efficiency (FTE), defined as:

$$FTE = (y_{min}^{*post} - y_{min}^{*pre})/\tau \tag{10}$$

We also measure the efficiency of transfers in reaching poor people as a whole. Here a standard measure in the literature is what we term Gap Transfer Efficiency (GTE), defined as the share of total transfers received by the poor, which is the reduction in the aggregate poverty gap per $ spent.[11]

[10] See, for example, Ravallion et al. (2015) in the context of a large workfare program in India and Daponte et al. (1999) on the context of food stamps in the US.

[11] GTE is standard output in the ADePT Social Protection software used by the World Bank (Tesliuc and Leite, 2010), although there it is called the "cost-benefit ratio." We prefer our terminology.

Economic development and the floor: We can identify two channels in how economic development impacts the floor. The first is direct, in that it holds at any given level of SP spending. This channel arises through the distribution of the market income gains associated with economic growth. Intuitively, the more "pro-poor" the growth process—such as the more it augments demand for relatively unskilled labor—the stronger is this direct channel. However, being "pro-poor" is not the same thing as reaching the poorest, as discussed in the Introduction and Section 2; poverty measures can fall yet the poorest are left behind. Indeed, we may see a sinking floor with certain growth processes. Suppose, for example, that growth is generated by greater trade openness and technological change, both of which put downward pressure on unskilled wages and hence the floor. Then we could see the floor fall as the mean rises.

The second channel is indirect, via higher SP spending. As has long been recognized, a potentially important channel by which economic growth can reduce poverty is via higher SP spending.[12] But is this channel important in practice, and does it embrace the poorest? The growth may be heavily concentrated among an elite who use their economic power to further reinforce their positions by promoting political opposition to redistributive tax and spending policies, with implications for the poorest as well as many others. Alternatively, the growth may come with similar or even large gains to electorally influential middle-class citizens who then support anti-poverty efforts, for either altruistic reasons or as insurance given the down-side risks they face. We will be interested in the combined effect of these two channels as well as the components, to see how the level of the floor varies with the level of economic development allowing social policies to adjust.

A simple theoretical model of the political economy of the indirect channel provides some insights.[13] In keeping with Meltzer and Richard (1981), let us assume that the overall level of SP spending is chosen by the median voter. In the present context, what is the relevant distribution for identifying the median voter? Even if $y_i^* - y_i$ has zero mean, the observed median need not equal the true median. One might argue that the observed median is more relevant to the political economy of transfer policy, as this reflects transient factors that could still sway electors. Against this view, the observed distribution also includes measurement errors

[12] See, for example, the discussion in Anand and Ravallion (1993). The UN's *Human Development Reports* have often emphasized this channel; see, for example, United Nations (2016).

[13] This is not the only model one can write down, but it will suffice for this purpose.

that may or may not matter to electoral outcomes. Here we will assume that the relevant median, y^{med}, is that of the observed distribution. The model can be modified to allow the alternative assumption that it is the median of the y_i^* distribution that matters. Of course, for our empirical work we have no choice but to use the observed medians.

A uniform tax τ is levied to finance SP spending, which depletes the current net income of the median voter.[14] The median voter is assumed to also care about the floor. This could be due to altruism or a self-interested concern about personally falling to the floor in the future. And the median voter is assumed to take account of the effects of higher SP spending on the floor. The utility function is $u(y)$ which is strictly increasing and concave. The median voter faces the average tax needed to finance the spending on SP, so the choice of τ maximizes:

$$u(y^{med} - \tau) + \rho u(y_{min}^{*post}) \quad (0 < \rho < 1) \tag{11}$$

Where ρ is the altruism weight on the utility of the poorest, or the probability of falling to the floor in the future. We allow the possibility that $f_{\tau\tau} > 0$, but that this is bounded above such that:

$$\frac{f_{\tau\tau}}{f_\tau^2} < \frac{-u_{yy}\left(y_{min}^{*post}\right)}{u_y\left(y_{min}^{*post}\right)} \tag{12}$$

When combined with our assumption that $u(y)$ is strictly concave for all y, the condition in (12) assures that the second-order condition for a unique optimal level of SP spending is satisfied.

The median voter's optimal spending on SP, given y^{med} and m, solves the first-order condition:

$$u_y(y^{med} - \tau) = \rho u_y(f(\tau, m)) f_\tau(\tau, m) \tag{13}$$

We can write the solution as:

$$\tau = \tau(y^{med}, m) \tag{14}$$

with first derivatives:[15]

$$\tau_{y^{med}} = \frac{u_{yy}(y^{med} - \tau)}{u_{yy}(y^{med} - \tau) + \rho[u_y\left(y_{min}^{*post}\right) f_{\tau\tau} + u_{yy}\left(y_{min}^{*post}\right) f_\tau^2]} > 0 \tag{15.1}$$

$$\tau_m = \frac{-\rho[u_y\left(y_{min}^{*post}\right) f_{\tau m} + u_{yy}\left(y_{min}^{*post}\right) f_\tau f_m]}{u_{yy}(y^{med} - \tau) + \rho[u_y\left(y_{min}^{*post}\right) f_{\tau\tau} + u_{yy}\left(y_{min}^{*post}\right) f_\tau^2]} \tag{15.2}$$

[14] Instead one can posit a tax on the median voter that is an increasing function of τ without changing the main argument.

[15] We treat ρ as a constant in the following derivation. Instead, one might prefer to assume that altruism develops as the mean income rises—that altruism gets little weight in very poor societies. Then ρ can be treated as a rising function of m. This adds an extra positive effect to τ_m in the following analysis.

While $\tau_{y^{med}} > 0$ (given (10) and $u_{yy} < 0$), the sign of τ_m is ambiguous. The model allows the possibility that a higher mean at given median—interpretable as higher "inequality"—lowers SP spending. A key issue here is the degree of complementarity between SP spending (higher τ) and economic development (higher *m*) in raising the floor, as indicated by the cross-partial derivative $f_{\tau m}$ (equation 15.2). Complementarity can arise in a number of ways. Countries that are more developed economically may well have greater administrative capabilities for reaching the poorest of the poor. This may also reflect specifics about the type of SP spending; if this facilitates the promotional objective whereby poor people receiving transfers are empowered or incentivized to participate directly in economic development then there is complementarity. Suppose that $f_\tau > 0,\ f_m > 0$ and that:

$$\frac{f_{\tau m}}{f_\tau f_m} > \frac{-u_{yy}\left(y_{min}^{*post}\right)}{u_y\left(y_{min}^{*post}\right)} \tag{16}$$

If this condition holds then we will say that there is strong complementarity between economic development and SP spending in how they influence the level of the floor. It is evident from (15.2) that strong complementarity implies that $\tau_m > 0$. However, suppose instead that the separability in (6) holds, or that there is substitutability between a higher mean income and SP spending in determining the floor ($f_{\tau m} < 0$). Then we have $\tau_m \leq 0$.

When we consider the bivariate relationship between SP spending and economic development we need to bring in the effect of a higher mean on the median. The total effect of economic development on SP spending is:

$$\frac{d\tau}{dm} = \tau_m + \tau_{y^{med}} \frac{dy^{med}}{dm} \tag{17}$$

Intuitively, the higher the impact of *m* on y^{med} the more "equitable" the growth process can be said to be. (Indeed, we can think of m/y^{med} as an indicator of inequality as noted.) Of course, the implications for the floor also depend on the incidence of SP spending.

Though our model is simple, it can be used to illustrate a wide range of possibilities. Consider the following stylized, but illustrative, cases.

Case 1: Equitable growth brings both a direct and indirect gain to the poorest. In this case, growth in the mean lifts the floor directly ($f_m > 0$) as well as indirectly via SP spending. Sufficient conditions for the latter channel to work are that growth in the mean also lifts the median, $\frac{dy^{med}}{dm} \geq 0$, and that there is strong complementarity. Then the effect on the floor is:

$$\frac{dy_{min}^{*post}}{dm} = f_m + f_\tau \frac{d\tau}{dm} > 0 \quad (18)$$

Recall that if the function $f(\tau, m)$ only exhibits weak complementarity (or substitutability) then the sign of τ_m reverses. It is still possible to find that $\frac{d\tau}{dm} > 0$ and (hence) $\frac{dy_{min}^{*post}}{dm} > 0$; the necessary and sufficient condition for $\frac{d\tau}{dm} > 0$ is that:

$$\rho[u_y(y_{min}^{*post})f_{\tau m} + u_{yy}(y_{min}^{*post})f_\tau f_m] - u_{yy}(y^{med} - \tau)\frac{dy^{med}}{dm} > 0 \quad (19)$$

Case 2: Inequitable growth leaves the poorest behind. If there is only (at most) weak complementarity and economic development is inequality increasing then it is possible to find that neither SP spending nor the level of the floor respond positively to a higher mean income. To illustrate one possible scenario, suppose that $\frac{f_{\tau m}}{f_\tau f_m} \leq \frac{-u_{yy}(y_{min}^{*post})}{u_y(y_{min}^{*post})}$, and that economic growth (a high *m*) does not benefit the median voter ($\frac{dy^{med}}{dm} = 0$). Then SP spending falls with a rising mean ($\tau_m < 0$). Furthermore, suppose that the poorest do not share directly in overall economic gains ($f_m = 0$). Then $f_m + f_\tau \tau_m < 0$, i.e., the floor falls as the mean rises.

4. Cross-country evidence

We first take the ideas of the last two sections to a cross-country setting. Here we mainly rely on the World Bank's "Atlas of Social Protection" (ASPIRE) as accessed mid-2017.[16] This draws on 262 household surveys in 122 countries in the developing world, from 1998 to 2014.[17] All currency conversions were done at purchasing power parity (for 2005 as the base).

In the World Bank's classification, SP spending comprises social insurance (mainly public pension schemes covering old age and disability), social assistance (cash and in-kind transfers and workfare schemes, often targeted to the poor), and labor market programs (training, entrepreneurship support, unemployment benefits). There is clearly a degree of substitutability

[16] ASPIRE is essentially a cross-country compilation of the outputs from a software program produced by the World Bank, *ADePT Social Protection*. Tesliuc and Leite (2010) provide a user manual. The ASPIRE team kindly provided detailed output tables from this software by country which we used to build our data set.

[17] We dropped Zimbabwe from the ASPIRE data as there were clearly serious data quality problems. (There have been numerous problems with Zimbabwe's data in recent times, so this problem was not unexpected.) Whenever SP spending data are used we also dropped Sierra Leone, for which the ASPIRE data show an extremely small positive level of spending relative to the estimated gain in the floor. This may well be a data error. When we take logs the very large negative value for Sierra Leone creates a clear outlier.

among these components; if a country is less generous in social insurance it may make up for this using social assistance. We include all components of SP in our analysis. However (as we will see), a large share of SP spending is contributory pensions.[18] We shall comment on the implications of separating out this component as it is rather different given that receipts reflect, in part, past contributions (though governments can still influence current disbursements). Thus we also provide results for social assistance on its own.

One option is to estimate the floor by fixing the poverty line across countries at (say) the World Bank's international line. This approach was rejected as it yields very small subsamples for estimating the floor in many countries, and hence volatile measures. Instead we use poverty lines set at $q(0.2)$ across all countries, i.e., the poorest 20% in each country define the reference group. The ASPIRE data set provides *PG* and *SPG*, and we use these two measures to construct our estimates of the floor for $\alpha = 1$. The value of $q(0.2)$ is then held constant for a given country when re-calculating the poverty measures net of transfers. In the ASPIRE dataset, the computations for *SPG* and *PG* pre-transfer are done assuming no behavioral responses. We maintain that assumption, though we provide the test described in Section 3. ASPIRE also provides data on SP transfers received per capita, which we use as our measure of τ.

Summary statistics: Table 1 provides summary statistics. Mean SP spending is \$0.88 per person per day. The bulk of this is contributory pensions (\$0.67); social assistance accounts for almost all the rest (\$0.19). The (un-weighted) mean floor post-transfers is \$1.69 a day, though varying widely, from \$0.12 to \$7.34. There is undoubtedly measurement error; it is very hard to believe that anyone lives at \$0.12 per day. While acknowledging the likely measurement errors, we focus on the overall patterns in the data, i.e., the (conditional and unconditional) means. Figure 2 plots the densities of $\hat{y}_{min}^{*post}$ and $\hat{y}_{min}^{*pre}$. The densities are skewed to the right. As we can observe in panel (b) of Figure 2, a log transformation helps to normalize the distributions of both floors. We use this transformation in the bulk of the following analysis. When we study the covariates of the gain in the floor due to SP spending we will use the proportionate gain, $\ln(y_{min}^{*post}/y_{min}^{*pre})$, as our preferred measure.

SP lifted the floor by \$0.48 a day on average (Table 1, comparing post- and pre-transfers). This is well below the mean spending per capita of \$0.88. The estimated value of

[18] Contributory pensions are classified as social insurance by the World Bank; non-contributory social pensions are classified as social assistance.

$y_{min}^{*post} - y_{min}^{*pre} - \tau$ is significantly different from zero (*t* = -3.968). Thus, we can reject the null hypothesis in (7). We also observe in Table 1 that SP spending reduced the headcount index by about 7% points on average (recall that the post-transfer index is 20%). There is also a substantial decline in the average poverty gap index, from 10.9% to 5.8%.

The bulk of the impact of SP in developing countries is due to public pensions, which lift the floor by $0.38 a day (Table 1). This too is below the mean spending on such pensions, which is $0.67 per day. Social assistance on its own only raised the floor by $0.015 per day on average—merely 8% of the (already low) level of average spending on social assistance (Table 1). The bulk of the impact of SP on the headcount index (5% points) is also due to contributory pensions. Social assistance on its own reduced the poverty rate by 2% points.

Countries that spend more on social protection tend to have a higher floor. Figure 3 plots the data; the correlation coefficient is 0.751. Mechanically, this relationship reflects both differing levels of SP spending and differing transfer efficiencies. Transfer efficiency in reaching the poorest varies greatly. Figure 4 gives the empirical density function for FTE.[19] (Recall that this is the ratio of the gain in the floor due to SP to mean spending.) We see that very few countries attain a value of FTE of unity or more. For the bulk of countries (87% of the sample), the gain to the poorest is less than mean SP spending. FTE tends to be better for social assistance on its own, for which the median value is 0.934, as compared to a median of 0.630 for all SP; 43% of countries have FTE for social assistance greater than unity.

In addition to FTE, we measure the efficacy of SP in reaching the poorest 20%, giving our second measure of transfer efficiency, GTE. The two measures are correlated (*r* = 0.505), but certainly not perfectly; some countries are better than others at reaching the poorest people given their efficacy in reaching the poorest 20%. GTE is positively correlated with spending per capita (*r* = 0.656), but that is not true for FTE (*r* = -0.021). As countries spend more on social protection, a larger share of that spending tends to reach the poorest 20% but not the poorest. Figure 5 plots the relationships with average SP spending for both FTE and GTE (it is easier to see if one logs spending per capita). This points to a notable difference in efficacy in reaching the poorest quintile versus the poorest households. By implication, relative efficiency in reaching the poorest (FTE/GTE) declines with mean spending (*r* = -0.430).

[19] Recall that Sierra Leone is dropped; this makes the bulk of the density function easier to see in Figure 3.

However, the bulk of the variance in the impact of higher SP spending on the floor (as evident in Figure 3) is due to the variance in aggregate levels of that spending, rather than its efficiency in reaching the poorest. If one decomposes the variance in $\ln(y_{min}^{*post} - y_{min}^{*pre})$ into the variance in log spending per capita, the variance in the log of FTE, and the covariance, the first component accounts for 77%, with the variance in log FTE accounting for 14% and the covariance representing 9%. (Recall that FTE has a low correlation with sending per capita.)

It will be recalled that our political economy model in Section 3 suggests that a key factor in determining the impact of economic growth on the floor is how much the median rises with the mean. We find a positive relationship. Indeed, the OLS elasticity of the median to the mean—the regression coefficient of log median on log mean—is not significantly different from unity; the regression coefficient is 1.012 with a robust standard error of 0.017. Figure 6 plots the data, showing the strong positive relationship in logs.

Countries with a higher median tend to have higher SP spending (Figure 7), as in our theoretical model. The correlation coefficient (between logs) is 0.711. For social assistance on its own the correlation is lower but still significant (r = 0.545). Given the pattern in Figure 6, it is no surprise that the bivariate relationships are very similar with the mean, though as we will see below, the stronger partial correlation is with the median, once one controls for the mean.

Richer countries tend to have a higher floor. Figure 8 plots the (log) floor, both pre- and post-transfer, against the (log) mean. Also notice that the two regression lines diverge. The pre-transfer floor has an elasticity of about 0.8 to the mean, while it is 0.9 for the post-transfer floor. The income elasticity of the pre-transfer floor is significantly less than unity (t = 3.2), implying that the (pre-transfer) floor tends to fall as a share of the mean as the latter rises. By contrast, the income elasticity of the post-transfer floor is not significantly different from unity, implying that the floor does not fall relative to the mean as economies develop. Thus we see that, on average, SP spending in developing countries is able to negate the tendency for the pre-transfer floor to fall as a share of the mean as the mean rises with economic development.

These elasticities also imply substantial absolute divergence between the floor and the mean. At the mean points from Table 1, a $1.00 increase in mean income comes with a $0.11 increase in the pre-transfer floor, and a $0.19 increase in the post-transfer floor.

Despite this strong correlation between the floor and mean income, the FGT poverty measures do not provide reliable indicators of the level of the floor. Indeed, the (post-transfer)

PG and SPG measures have only weak negative correlations with the (post-transfer) floor; *r* = -0.179 and -0.150 respectively. Differences in income distribution (at a given mean) are clearly clouding these relationships. A much better indicator of the floor is the quantile of the poorest 1% ($q(0.01)$) for which *r* = 0.945. However, for the reasons noted in Section 2, $q(0.01)$ could be a noisy measure. It is no harder to calculate our measure in (4) from the same primary data.

In terms of the model in Section 3, the strong positive relationship between the level of the floor and mean income reflects both higher SP spending in richer countries and a direct effect at given spending. We will now use regressions to separate out these effects.

Partial correlations: To allow for multiple covariates, we now explore these relationships further using regressions. (We do not intend that these are to be given a casual interpretation, but only as a convenient means of testing for partial correlations.)

First, we reexamine how SP spending varies with the mean, but now controlling for the median. Recall that our model of the political economy of SP spending implies that the median matters independently of the mean (equation 13), but only the comparative static effect of the median is predicted in sign. The expected positive effect of the median is confirmed by the regressions in Table 2. By contrast, the (log) mean has a negative effect but not significantly different from zero. This pattern is also found for social assistance on its own (Table 2). This is inconsistent with strong complementarity (Section 3), although a weaker form of complementarity may still be present, with $f_{\tau m} > 0$ even if equation (16) does not hold. (We will return to this point.) Note also that the elasticity w.r.t. the median is high, though at mean points the slope (for total SP) is much lower at 0.49. Of course, this is the partial elasticity, holding the mean constant. As we have seen, the two variables co-move with an elasticity of about unity; the elasticity falls by one third if the mean increases at the same rate. What is clear from Table 2 is that the median is a far stronger predictor of SP spending across countries than the mean.

Next, we test for behavioral responses by the poorest. Recall that an implication of our assumption that the pre-transfer floor is the post-transfer value less SP spending received by the poorest is that we should not find a correlation between the estimated pre-transfer floor and mean spending (Section 3). While there is a significantly positive (zero-order) correlation between the pre-transfer floor and average SP spending (*r* = 0.511), this vanishes when we control for the mean. The partial correlation falls to 0.068; Figure 9 plots the two series (with log floor predicted at mean income) while Table 3 gives the regression where we also see clearly that countries with a higher overall mean have a higher pre-transfer floor. The restriction that SP spending does not affect the pre-transfer floor at a given mean performs well. This provides support for our estimation method ignoring any behavioral responses of the poorest.

Table 3 gives the regressions for the gain in the floor attributed to SP, i.e., our estimate of $\ln(y_{min}^{*post}/y_{min}^{*pre})$. We see that higher aggregate transfers contribute to a larger impact of transfers on the floor. Noting that we can obtain the regression for the post-transfer floor by adding that for $\ln(y_{min}^{*post}/y_{min}^{*pre})$ to that for $\ln(y_{min}^{*pre})$, we see that there is both a direct effect of higher mean income on the post-transfer floor and an indirect effect, via higher SP spending; the direct effect is 0.642 (s.e.=0.070). However, when normalized by the total income elasticity of 0.923 (Figure 8), we see that the bulk (70%=0.642/0.923) of the effect is direct.

We find that there is a positive interaction effect between average transfers and the mean, which helps in raising the impact of SP on the floor (Table 3, Column 5). This suggests weak complementarity between SP spending and economic development in how they influence the efficacy of SP in raising the floor; a higher mean income comes with higher marginal gains to the poorest from higher public spending on SP.

One clue to the role played by heterogeneity in transfer effectiveness is to augment the regressions with gap transfer efficiency; recall that this is the impact of SP spending on the aggregate poverty gap for the poorest 20% per $ of spending. Here we are interested in seeing whether countries that are more efficient at reaching the poorest 20% also tend to do better at lifting the floor, and here we can expect both an additive effect and an interaction effect with mean transfers. We can go further and allow a complete set of interaction effects, including with the mean. This augmented specification is in Column (6) of Table 3.

As expected, there is a strong interaction between GTE and transfer spending in their effects on the extent to which SP lifts the floor. There is also a negative interaction effect between mean income and transfer efficiency; it is in poor countries where the effectiveness in transferring money to the poorest 20% tends to matter more to lifting the floor. When we evaluate the total effects at the mean points, we find a significant positive effect of SP spending and GTE on the extent to which those transfers succeed in raising the floor (Table 3, lower panel). Once we control for the level of transfers and transfer efficiency we do not find that higher average incomes come with a greater impact of transfers on the floor.

5. Evidence for the US

It is of obvious interest to also look at time-series evidence.[20] One reason for choosing the US for this purpose is the availability of the required data over a long period. Another reason is that there has been a strong tendency of rising inequality in the US, and limited progress against poverty pre-transfers. For example, our data indicate that the real value of median family income grew at only 0.5% per annum over 1988-2016, while $q(0.2)$ and the official poverty rate were essentially stationary.[21] Yet there was sizable growth in top incomes over this period; for example, $q(0.99)$ grew 1.6% per annum in real terms (as shown later). The questions here are: What happened to the floor in the US over time, and what role did SP play?

We focus on the largest direct intervention against poverty in the US, the food stamps program (SNAP).[22] SNAP is a Federal program (administered by the Department of Agriculture) that helps poor families purchase food. In (fiscal) 2016, SNAP covered about 44 million Americans (14% of the population) at a cost of $71 billion, representing $125 per person per month for food stamp beneficiaries. The program is targeted to poor families (below 130% of the official poverty threshold), and aims to provide larger benefits to poorer families.[23] There are concerns that the program does not reach all those who are eligible.[24] As a case study, SNAP is also of interest given that spending levels change substantially over time (as we will see).

Time series data for a single country are unlikely to have much power for testing the political economy model of SP spending in Section 3, and so we will not try to explain SNAP spending by the time series variation in median income (though this may still be an underlying longer-term property of the data). There are, however, two significant policy changes worth noting. First, a series of reforms in 1996-98 put emphasis on reducing perceived "leakage" to those not considered eligible, including greater use of work requirements, though this can also

[20] All standard errors reported in this section are robust to residual autocorrelation as well as heteroscedasticity. We use the method for estimating robust standard errors in Newey and West (1987), as programmed in *EViews* 10. We used the automatic lag specification with the degrees of freedom adjustment.

[21] The trend rate of growth in the median based on the Current Population Survey (discussed below) is 0.528% per annum (s.e.=0.122%). (All growth rates for incomes in this paper are estimated by the regression coefficient of log income on the year.) The corresponding growth rate for $q(0.2)$ was -0.107% per annum (s.e.=0.161%). Over 1988-2016, the regression coefficient of the official poverty rate (in %) on the year is 0.016 (s.e.=0.035).

[22] Spending on SNAP overtook the next largest program, the Earned Income Tax Credit, in 2010 (Hoynes, 2012).

[23] For further information and analysis on SNAP see Bartfeld et al. (2016) and Hoynes and Schanzenbach (2016). On the benefits to children from poor families see Jolliffe et al. (2005). For a broader overview of antipoverty policies in the US see Ben-Shalom et al. (2012).

[24] See, for example the discussion in *The Economist* (2011).

reduce participation by eligible participants including some of the poorest.[25] Time limits and recertification became stricter; legal immigrants were variously eligible, then ineligible, then eligible again, but growing concerns about status reduced their participation. Able-bodied adults without dependents found it harder to access SNAP. Electronic Benefit Transfer (EBT) was also introduced around the turn of the century (earlier in some states).[26] It is unclear on a priori grounds whether this would help the poorest, although the bulk of the decline in FTE appears to have preceded EBT. Second, spending on the program surged in the aftermath of the 2008 financial crisis and the subsequent rise in unemployment. As a part of the American Recovery and Reinvestment Act, SNAP benefits increased by 14% in April 2009.[27] This "SNAP stimulus" is consistent with a longer-term pattern of (countercyclical) co-movement between SNAP spending and the unemployment rate (Hoynes and Schanzenbach, 2016).

To assess the effects of SNAP on the floor, we use the micro data from the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS).[28] We use 29 years of CPS-ASEC data from 1989 to 2017, which allows us to estimate the floor and SNAP benefit levels from 1988 to 2016. The CPS is administered by the Census Bureau for the Bureau of Labor Statistics and collects data from a nationally representative sample of households on employment, unemployment, earnings, occupation, and hours of work. We are measuring the floor in terms of income not consumption.[29] The measure of family income we use is the same as that for the US official poverty estimates. This includes money income before taxes from several sources (such as wages, salary, net-income from self-employment, social security payments, pensions, interest, dividends, alimony, other forms of periodic monetary income), but excludes capital gains and non-cash benefits such as fringe benefits or noncash government social

[25] The first reform was the Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (with most provisions effective from mid-1997), followed by the Balanced Budget Act of 1997 and the Agricultural Research, Extension, and Education Reform Act of 1998 (USDA, 2017). For further discussion of these reforms to SNAP and their implications see Currie and Grogger (2001).
[26] EBT entailed that SNAP recipients paid for food using a "debit card." (If the pin code is verified and the account balance is adequate then payment is accepted.)
[27] For example, the maximum SNAP benefit for a household of three rose from $463 to $526 per month. Under ARRA, states could suspend time limits for unemployed able-bodied adults. Subsequent legislation imposed an expiration date of November 2013 for the 13.6% SNAP benefit increase.
[28] Further information on the CPS can be found in United States Census Bureau (2017).
[29] There has been a debate on the choice between consumption and income as measures of economic welfare, including in the US (Slesnick, 2001; Meyer and Sullivan, 2012). We do not take a position on this issue here, as we have little choice for the present purpose given both the data available and the fact that (unlike consumption) income is additive with respect to SP spending.

assistance programs.[30] As has been noted in the literature on poverty in the US, the official poverty measure does not reflect the impact of SNAP or the next largest antipoverty program, the Earned Income Tax Credit, because these income sources are omitted from the income aggregates used for the official measures.[31] To obtain our "post-SNAP" income distribution, we add in the face value of the food stamps (or in later years, the credit value on the EBT card).

We provide results for the US assuming that the poorest household has an observed income below the official US poverty threshold, which is fixed in real terms over time. However, the official poverty thresholds vary by family size and composition.[32] It is thus simpler to express the floor for the US as a proportion of the threshold, though to aid interpretation we provide some illustrative calculations for a family of four with two children. For consistency with our cross-country analysis we will also provide calculations using the poorest 20% as the reference group, i.e., using $q(0.2)$ as the fixed threshold for calculating the floor before and after SNAP.[33] However (unlike the cross-country data), $q(0.2)$ is virtually constant, with no trend in either direction (a regression coefficient of the log of the real value on time of -0.001, with s.e=0.002). Nor does $q(0.2)$ change much over time in real terms; the CV of $\ln q(0.2)$ is 1.22%. For most of the analysis we treat $q(0.2)$ as fixed before and after SNAP, which appears to be the most common practice in the literature (as exemplified by the World Bank's ASPIRE data set discussed in Section 3). Thus, similarly to the official threshold, we focus on the floor ratio.

[30] The CPS public-release measure of income imposes top-codes on income components, and the methodology for top-coding changed over our time period in a manner that affects a small number of observations in our analytical sample. Because top-coding is done by income components and not total income, and because it has not (typically) been based on winsorizing but rather a hot-deck method, it is possible for poor families to have public-release income estimates that are greater than the poverty threshold. Some important information on these households can be extracted from the data because the variable (*povll*in the unit-record CPS public-release data) creates a series of bins based on the ratio of family income (as measured by the confidential data) to the family's poverty threshold. For example, in the 2008 CPS data there are nine sample observations (from a total of more than 206,000 observations) where income as reported in the public-release files, is greater than the poverty threshold, but these are poor families as identified by *povll* (which is based on the confidential income data). In the 2008 CPS files, all nine of these observations had income that was less than the poverty line, but greater than 75 percent of this line. For these nine observations, we re-assign their income to be the mid-point of their bin. This re-assignment occurs in nine of our years. The number of re-assignments made in any given year is less than 24, with a total count of 90 re-assignments overall years from the more than four million total observations we examine.

[31] The literature has pointed to a number of limitations of the official poverty measures, related to both the income concept and the poverty lines; for further discussion see Citro and Michael (1995), Blank (2008) and Meyer and Sullivan (2012). The Census Bureau has recently introduced an alternative "supplemental" measure that addresses some of these concerns (Short, 2011). Here we only address the problem related to the exclusion of SNAP from the official income aggregates based on the CPS.

[32] The thresholds can be found here.

[33] For computational convenience, the quantile for p=0.2 is fixed prior to adding SNAP receipts, while in the cross-country analysis it is fixed in the post-transfer distribution.

However, we recognize that this is a methodological choice that might be questioned, so we also present some key results on the response of the real quantile to SNAP spending.

The floor can respond to transient changes in the level of prices—inflationary shocks. When the price level increases the nominal poverty line rises, expanding the reference group. This effect will be negligibly small for a small change in prices, given that income at the poverty line has zero weight in equation (2), though larger changes can alter the floor. However, there will also be some short-term impacts below the line, as the weights depend on the threshold, as can also be seen from (2). As we will see, the transient effect of inflation on the floor is positive. We address this issue by including a control for the inflation rate—change in the log of the CPI—in all our regressions for the (log) floor. Note that if it is only a temporary change in the price level, with no real effects, then the floor will return to its previous level in due course.

Summary statistics: Table 4 provides summary statistics; a more complete addendum is available from the authors. Figure 10a plots SNAP spending per capita, while 10b gives spending per recipient and the participation rate. We see that spending per capita fluctuated over time, due mainly to participation rates, which closely tracked spending. Spending per recipient grew over time, with a growth rate of 1.342% per annum (s.e.=0.135%). There was a marked increase in spending per capita in the 2000-11 period; spending doubled between 2006 and 2012, but has tended to fall again in recent years.

Figure 11 plots our estimates of the floor ratio before and after food stamps, using both the official threshold and the poorest 20%, and for both $\alpha = 1$ and $\alpha = 2$.[34] Naturally, the floor is lower for $\alpha = 2$, whereby the probability of a given income being the floor declines as a quadratic rather than linearly, but the trajectories are similar, and *r* = 0.998 for the official threshold and 0.997 for the poorest 20%. The following discussion focuses on $\alpha = 1$, in keeping with the cross-country analysis. The pattern over time is similar between the two thresholds.

We find a trend decline in the post-transfer floor. Using the official threshold, the trend rate of change (regression coefficient of the log floor on time) is -1.321% per annum (s.e.= 0.110%). Using the poorest 20%, the rate is somewhat lower, at -0.812% per annum (s.e.= 0.033%). The floor stabilized in the period 2003-12; the bulk of the decline (especially using the

[34] Note that expressing the floor in $'s makes no difference to the pattern over time for panel (a) since the threshold is fixed in real terms. The threshold does vary over time when using the poorest 20% but (as noted) the fluctuations are small with no trend. So the pattern in Figure 11, panel (b), is very similar if one calculates the floor in $s.

official threshold) was in the period 1996-2001, coinciding with the aforementioned social policy reforms. There are clear signs that the post-SNAP floor stabilized from the early 2000s. There is no significant trend in the post-SNAP floor from around 2003, though the fall in the pre-SNAP floor continued, albeit at a slower pace (Figure 11). However, the last few years of the series have seen a resumption in the floor's decline (Figure 11). While we find that food stamps lifted the floor, the sharp decline in the floor 1996-2001 cannot be attributed to changes in SNAP alone since we also see a similar decline in the pre-SNAP floor, though less than we find post-SNAP.

Standard poverty measures are not highly correlated with the floor. This can be seen by comparing Figure 11 with 12, which gives the three standard FGT poverty measures. For example, the periods 1993-2000 and 2012-16 saw declining poverty measures but a sinking floor. And the sharp rise in poverty measures in the crisis period (2008-10) came with a relatively stable floor post-SNAP. The proportionate changes over time in the floor are roughly orthogonal to those for the (post-SNAP) headcount index (r($\Delta\ln(y_{min}^{*post}$, ΔH)=0.041) and not highly correlated for the two poverty gap indices (r = -0.268 for changes in PG and r = -0.492 for SPG). Tracking standard poverty measures alone is clearly not very informative about what is happening to the floor.

To help put the level of the floor in perspective, Figure 13 plots the overall mean and various quantiles—the median ($q(0.5)$) and top income quantiles, $q(0.9), q(0.95)$ and $q(0.99)$. The latter quantiles fluctuate around a strong positive trend, as indicated. A marked divergence is evident between the top incomes and the mean, median and (especially) the floor (shown for the official threshold). Note also that these are quantiles based on the CPS. There is a likely bias in the CPS at the high end, associated with under-reporting and selective compliance.[35] The bias is unlikely to be confined to the top 1%, or even 10%, so the quantiles will be affected, implying that correcting for these problems would show even greater divergence from the floor.[36]

Relative divergence is also evident. The growth rate of the (pre-SNAP) floor over the whole period is -1.28% per annum (s.e.=0.061), while it is -1.32% (s.e.=0.110) for the post-SNAP floor. As noted, $q(0.2)$ shows essentially zero growth while the median has a small

[35] For evidence on the latter see Korinek et al. (2007).
[36] See, for example, the estimates combining CPS with income tax and national accounts data in Piketty et al. (2018) and Worldwide Inequality Database. Of course, such adjustments to survey data require a great many assumptions.

positive growth rate. By contrast the growth rate of $q(0.99)$ is 1.63% per annum (s.e.=0.320). For a family of four people, the floor fell from about 9.2% of $q(0.99)$ in 1988 to 3.9% in 2016.

Food stamps raised the floor by 0.03 on average (as a proportion of the official poverty line). Using the official threshold, the mean post-SNAP floor is about 36% of the official poverty threshold, which was $24,036 a year in 2015 for a family of four (two adults and two children) or $16.50 per person per day (rounding up slightly). So the mean floor in that year's prices is $5.89 a day, while the pre-transfer value is $5.40; the corresponding numbers in 2010 prices are $5.41 a day post-SNAP and $4.95 pre-SNAP, corresponding to an official poverty threshold of $15.15 a day.[37] The floor for the US is about the level of the highest floors in the developing countries.

Unlike the cross-country data set, the mean gain to the poorest now exceeds mean spending on food stamps; the difference between the mean gain for the poorest and mean spending of $0.12 a day (in 2010 prices) is statistically significant (s.e.=0.041).[38] So (again) the null in (6) is rejected, but this time the gain to the poorest exceeds mean spending.

Similarly to the cross-country data (Figure 6), we find that the US median increased with the overall mean. However, the ratio of the median to the mean has been falling over time, from around 0.78 to 0.71 (Table 4). While the median rises with the mean, as in the cross-country data set and consistently with our model in Section 3, unlike the cross-country data, it does so with an elasticity less than unity.[39] This decline in the median relative to the mean as the latter increases is another aspect of the inequitable growth process of the US. The Gini index of inequality in family income rose from 0.40 to 0.48 over this period (Table 4).

We find that there has been a decline in FTE—the ratio of the gain in the floor that we attribute to SNAP to spending per capita on the program (Figure 14). The program is reaching the poorest, but efficiency in raising the floor has declined appreciably over time. In the late 1980s, the gain in the floor was about 2.5 times mean spending, but by the last five years of the series it had fallen to about the same level (an FTE of unity). Food stamps used to be much more effective in reaching the poorest, although the welfare-reform period starting in the mid-1990s appears to have come with a slowing down in the rate of decline in FTE (Figure 14). Note also that the decline in FTE largely preceded the large expansion in SNAP spending under ARRA in

[37] The poverty threshold was $22,113 a year in 2010 for a family of four (two adults and two children).
[38] Note that SNAP spending in Table 4 is monthly.
[39] The OLS regression coefficient of the log median on the log mean is 0.611 with a standard error of 0.037. One can reject the null that the coefficient is unity with t = 10.64.

the latter sub-period (including the crisis) but the latter expansion clearly did not come with better performance in reaching the poorest.

Partial correlations: Given that these data are a time series we include the lagged dependent variable. As noted, we also include a control for price inflation, which can have a transient effect on the floor. Table 5 provides regressions analogous to those for the cross-country data set (Table 2). Columns (1)-(2) use the official threshold while (3)-(4) give the corresponding results using the poorest 20%. Serial dependence in the floor is evident.[40] We also find a significant effect of price inflation on the pre-SNAP floor based on the official threshold to define the reference group, though this is smaller using the poorest 20%.

Similarly to the cross-country data, we find that the pre-SNAP floor is unaffected by food stamp spending (Table 5, Columns (1) and (3)). So the US evidence is consistent with our assumption of no behavioral effects at the floor. However, similarly to the cross-country data, SNAP spending has a significant positive effect on the gain to the floor from SNAP, though the elasticity is quite low at around 0.03 (Table 5). On further decomposing log SNAP spending per capita into log spending per recipient and log participation rate we find that the latter accounts for the spending impact.[41] Unlike the cross-country sample, there is no sign of interaction effects between mean income and SNAP spending in determining the gain from SNAP.[42]

In contrast to the cross-country data, food stamp spending in the US does not exhibit a positive mean income effect; regressing log food stamp spending on its own lagged value and the log of the mean, the coefficient on the latter is -0.139 with a standard error of 0.176. So in the case of food stamps in the US there is only the potential for a direct effect on the floor.

A notable difference between the cross-country results and those for the US is that growth in mean income in the US has come with a lower floor. This is not because growth in mean income has bypassed poor people as a whole. Using the (fixed) official poverty threshold, the empirical elasticities of both SPG and PG to the mean are both negative; regressing $\Delta \ln SPG$ and $\Delta \ln PG$ (using the official lines) on $\Delta \ln m$ the elasticities are -1.339 (s.e.=0.197) and -1.437

[40] Without the lagged dependent variable as a regressor, the regressions exhibit significant autocorrelation in the errors, as revealed by (say) the Durbin-Watson statistic. We also tested the robustness of each regression to adding an independent time trend. The key findings were robust to adding a time trend.

[41] For example, replacing $\ln \tau$ by the log spending per recipient and the log participation rate, the latter has a coefficient of 0.039 (s.e.=0.006) while the effect of the former is not significantly different from zero. This is not surprising given that the participation rate closely tracks spending per capita (Figure 10).

[42] For example, adding $\ln \tau . \ln m$ to the regression in Column (2) its coefficient is 0.105 (s.e.=0.120).

(0.156) respectively. It is the fact that the (absolute) elasticity w.r.t. the mean is lower for SPG than for PG that is driving our result on the (negative) growth effect on the floor (equation 5). This also implies that price inflation will have a short-term positive effect on the floor.[43] As discussed in Section 3, this negative growth effect can also be given an economic interpretation as the combination of (i) only weak complementarity (or even substitutability) between SNAP and economic development with (ii) a distributional effect, whereby growth has come with changes in relative distribution that have gone against the poorest.

The use of the poorest 20% to define the reference group offers a further insight because for this case the fixity of the poverty rate cuts off this channel linking growth in the mean to poverty reduction when one uses a fixed poverty line. Thus, there is no need to add a control for inequality; the negative effect of higher mean income on the floor evident in Table 6 can be directly interpreted as a distributional effect.

The last two columns of Table 5 provide regressions for the log of $q^{post}(0.2)/q^{pre}(0.2)$ (in obvious notation). No variable is individually significant in Column (5), but one can also reject the joint restriction that only SNAP sending matters (F=0.577; prob.=0.637), and Column (6) gives this regression. As noted, it is a methodological choice whether to fix the quantile or the percentile when comparing poverty measures with and without transfers. If one prefers to fix the percentile then this almost doubles the elasticity of the floor to SNAP spending (adding 0.028 from Column 6 to the elasticity of 0.034 from Column 4).

6. Conclusions

To test whether public spending on social protection has reached the poorest we must be able to measure and monitor the floor—the lower bound of the distribution of income. It is clearly not enough to look at the evolution of any standard poverty measure, which can fall with or without an increase in the level of living of the poorest. Instead, we need to focus on the floor directly, and we must recognize that the lowest observed income in a cross-sectional survey need not be a good indicator of the true floor of living standards given transient effects and measurement errors in the data. To address these concerns we have measured the floor as a

[43] Note that our measure of the floor is homogeneous of degree zero in the mean and the poverty line given that this also holds for the P_α poverty measures.

weighted mean for those in a chosen reference group deemed to include the poorest, with highest weight on those observed to be poorest.

To help motivate our empirical analysis, we have outlined a simple model of the determination of the floor, which is taken to depend on public spending on social protection and on economic development, measured by mean income, while the level of social protection spending depends in turn on the mean. Thus, there is both a direct effect of economic development on the floor, and an indirect effect via social protection. A key role is played by the extent of complementarity between SP spending and development; complementarity exists when higher SP spending increases the marginal gains to the poorest from growth. If this complementarity is not too weak, and the growth process is not too inequitable, then the floor will rise with economic development; the poorest will not be left behind. But that need not hold, and there is no guarantee that the poorest will see any gain from overall economic development.

We have assembled illustrative evidence from both cross-country data on social protection spending in developing countries (the bulk of which is social insurance) and time series data for a specific social program in the US, namely food stamps. We find that higher SP spending helps to lift the floor in both data sets. The poorest benefit from this spending. There is considerable variability across countries, the bulk of which (in terms of variance) is due to differences in the level of SP spending rather than the transfer efficiency of that spending. However, for the cross-country sample, the gain from SP for those living at the floor is significantly less than aggregate spending per capita. Social insurance (mainly public pensions for old age and disability) does the "heavy lifting" of the floor in developing countries. Social assistance on its own lifts the floor by merely 1.5 cents per day. For food stamps in the US, the gain to the poorest is significantly greater than mean spending, though substantially less so over time. We find a marked decline in the efficacy of food stamps in reaching the poorest since the mid-1990s.

We also find that higher average income tends to come with a higher pre-transfer floor, though not enough to prevent a <u>relative</u> decline in the floor with overall growth, and large absolute divergence. This is the sense in which it can be said that the poorest tend to be "left behind" with economic development. The bulk of the efficacy of economic growth in lifting the floor appears to be direct, rather than via higher SP spending. Nonetheless, SP spending comes close to assuring that the post-transfer floor does not sink relative to the mean when comparing low and high-mean countries. Statistically, while the pre-transfer floor tends to fall relative to the mean as the latter rises, we cannot reject the null hypothesis that the post-transfer floor stays at a constant share of the mean.

There is also evidence for the cross-country data, but not the US, of complementarity between SP spending and economic development, as evident in a strong positive interaction effect between SP spending and mean income in regressions for the gains to the poorest from higher SP spending. Along with rising SP spending, this complementarity plays a positive role in helping to assure that the poorest benefit from growth.

In contrast to the cross-country sample, for the US we find that economic growth has come with a sinking floor. We interpret this as the combined effect of relatively weak complementarity with a quite strong distributional effect; controlling for the rise in overall inequality, growth in mean income has had little effect on the floor. The floor in the US fell most markedly in the 1990s, but began to stabilize in the 2000s. The financial crisis put downward pressure on the floor, but the expansion of the food stamps program in the wake of the crisis was able to prevent a fall in the floor despite the inequality-increasing growth process. The expansion of SNAP in the 2000s partly compensated for the downward pressure on the floor coming from the unequal nature of the growth process. Extra food stamps helped assure that the poorest could at least maintain their (low) living standards.

While our results are suggestive of causal interpretations, there are warnings to note. Our estimates of the impacts of SP on the floor can only be interpreted as the true causal impacts under the assumption that there are no behavioral responses by the poorest. For both the cross-country data set and the US data, we have shown that our estimates of the pre-transfer floors under this assumption are uncorrelated with mean transfers (at a given mean income) across countries. This test is broadly consistent with our assumption of behavioral neutrality for the poorest, but it cannot be deemed conclusive. There may still be some bias in our estimates due to behavioral responses by the poorest. We also warn against giving our regressions a causal interpretation; we provide them as only descriptive tools for measuring (partial) correlations.

In thinking about further work on this topic, we point to two directions. First, measuring and monitoring the floor over time should now be feasible for many countries, using the methods in this paper. We suggest that an estimate of the floor should become a staple in the existing dashboard of social indicators. Doing so would make poverty measurement more relevant to the ongoing concerns among policy makers and citizens about not leaving the poorest behind.

Second, further research is called for on the causal interpretation of the link to social protection. The greater concern here is possibly not behavioral responses by the poorest but rather the endogeneity of SP spending. Our regression error term includes the (likely) heterogeneity across countries in the impact of SP on the floor. The regression coefficients we have estimated are interpreted as average impacts. Suppose that countries that are more effective at using aggregate SP spending to reach the poorest tend to spend more on SP. Then our variable for SP spending per capita will be positively correlated with the error term, implying that OLS overestimates the true mean impact on the floor. However, we cannot rule out the possibility of the opposite direction of bias, as would be the case if the countries that are better at using SP spending to reach the poorest tend to spend less on SP.

Identifying behavioral responses of the poorest and impacts of social protection on the floor remains a challenge, though probably one that is better addressed by a more micro-empirical and program-specific approach using impact evaluation tools. We hope that this study has at least pointed to the desirability and feasibility in such efforts of including a focus on the living standards of the poorest. Doing so will remove an obvious disconnect between how social policy makers view their objectives and how economists assess their progress. Greater interest in measuring the floor will hopefully also come with extra effort in survey design and processing to more accurately measure the living standards of the poorest.

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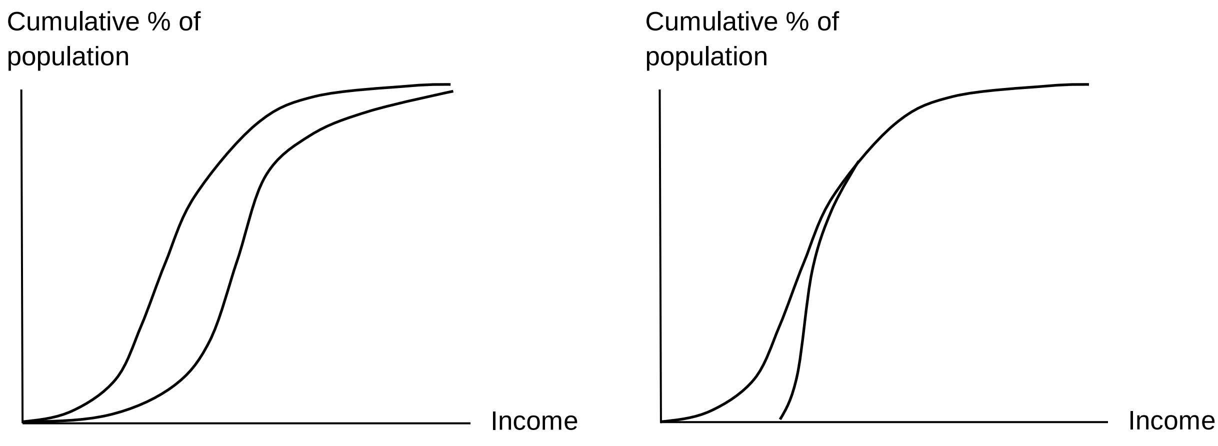
Figure 1: Both pairs of distributions show first-order dominance but with very different implications for the floor



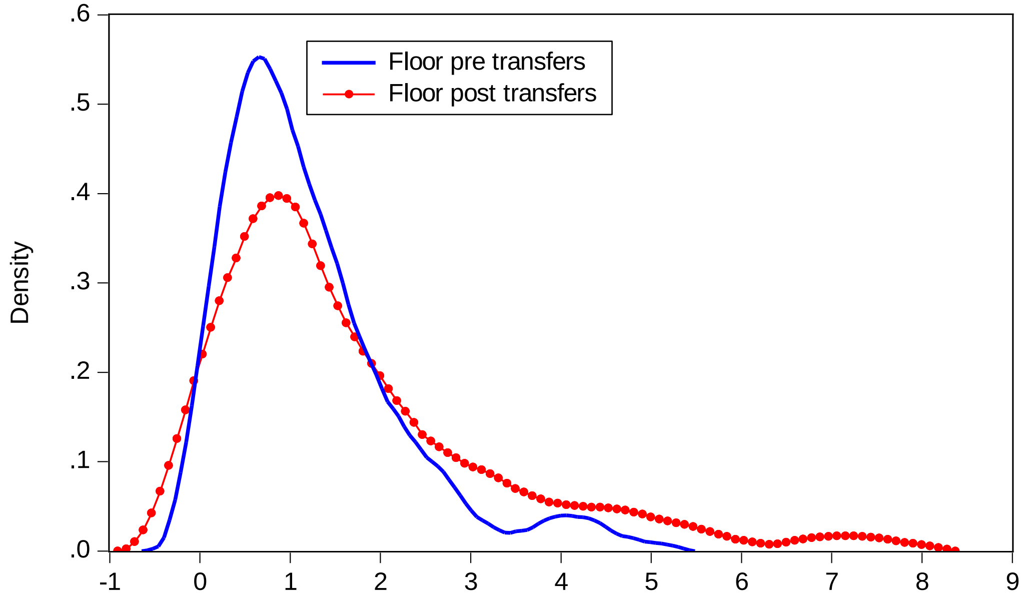
(a) The floor has not risen; poorest are left behind

(b) Floor has risen

Source: Ravallion (2016a).

Figure 2: Kernel density functions for the floor across countries

(a) Linear



(b) Log transformation

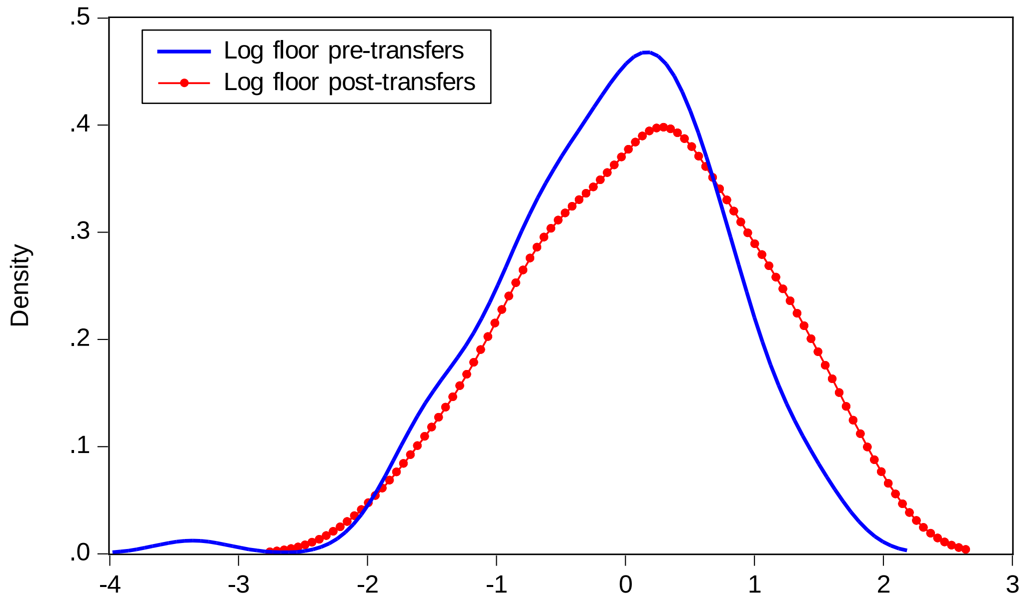


Figure 3: Higher SP spending comes with a higher floor

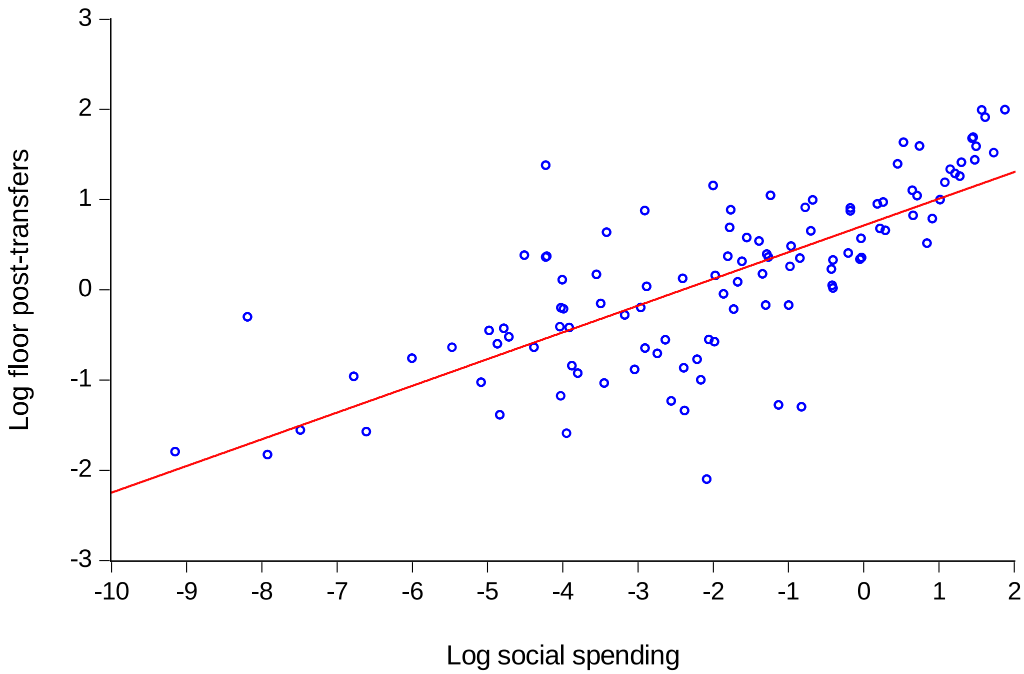


Figure 4: Kernel density functions for floor transfer efficiency

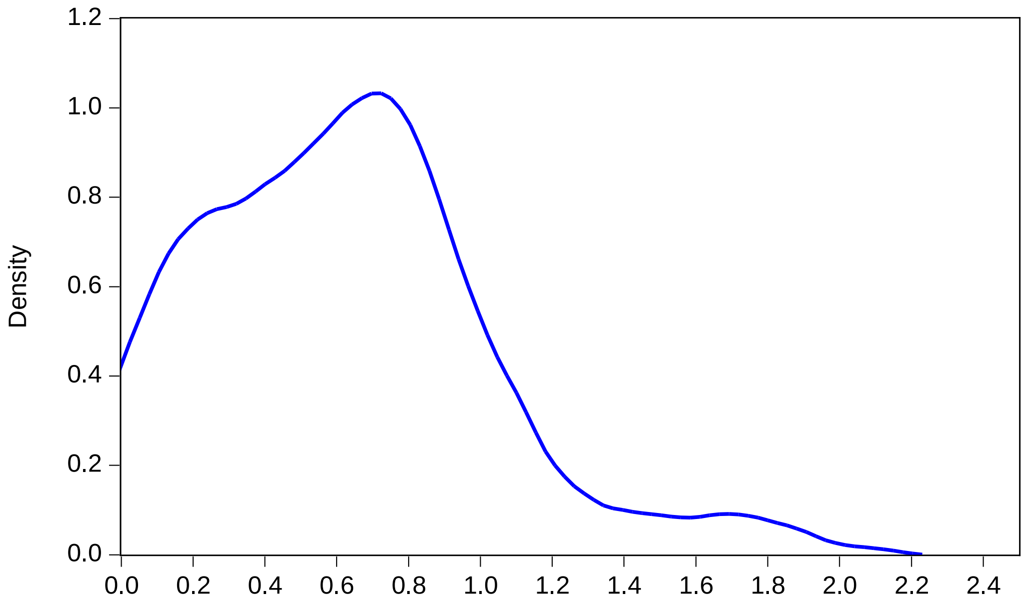
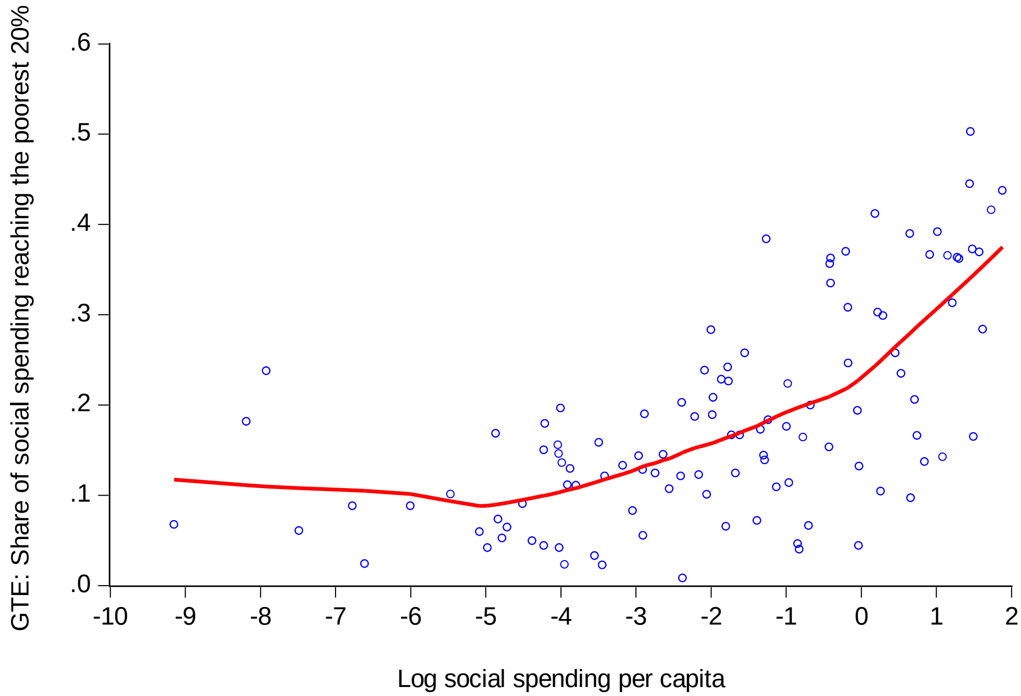


Figure 5: Transfer efficiency plotted against aggregate transfers per capita

(a) Gap transfer efficiency



(b) Floor transfer efficiency

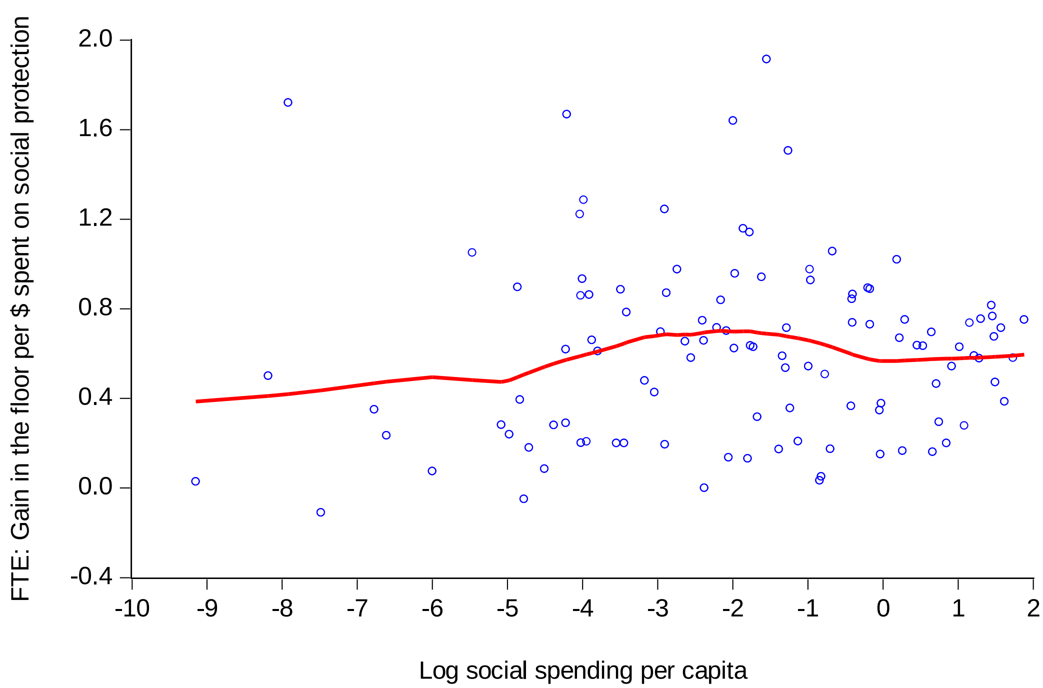


Figure 6: Log median plotted against log mean across countries

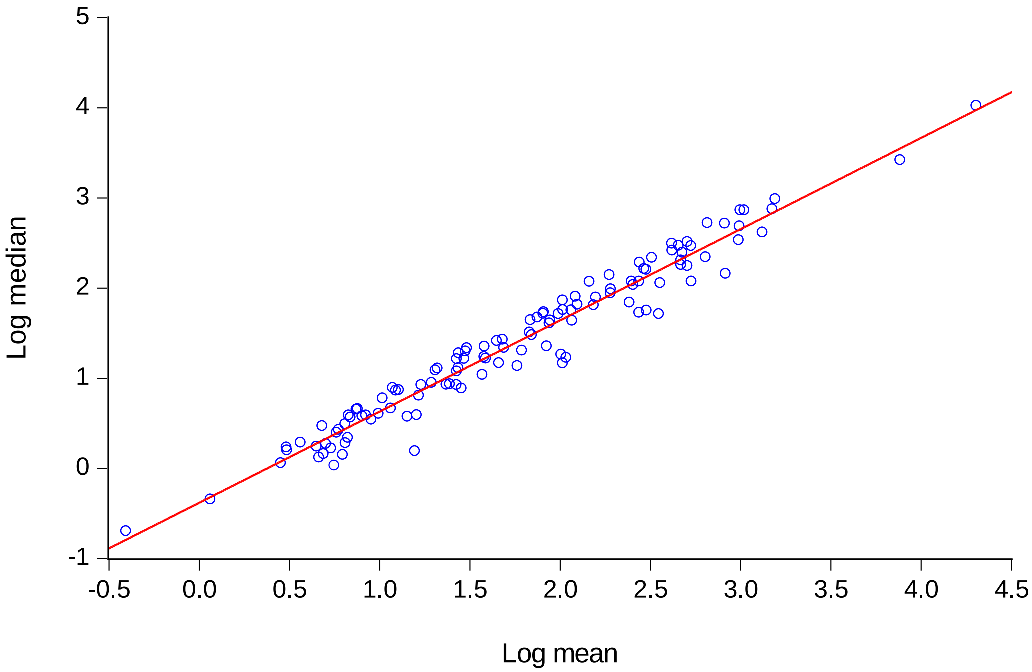


Figure 7: Average transfer plotted against survey median across countries

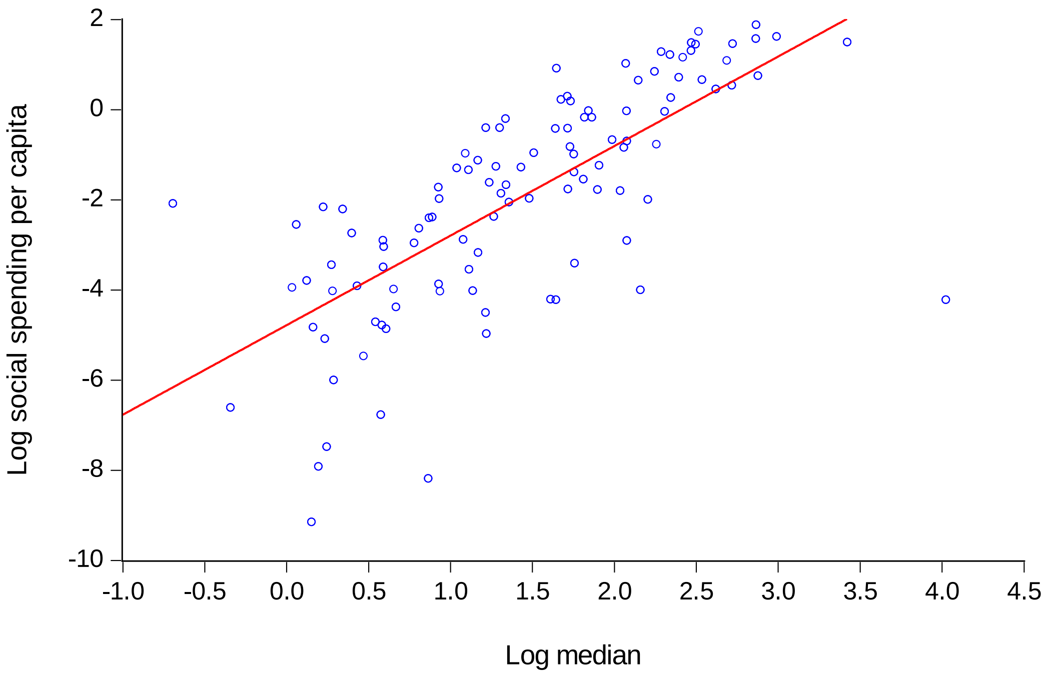


Figure 8: Richer countries have a higher floor

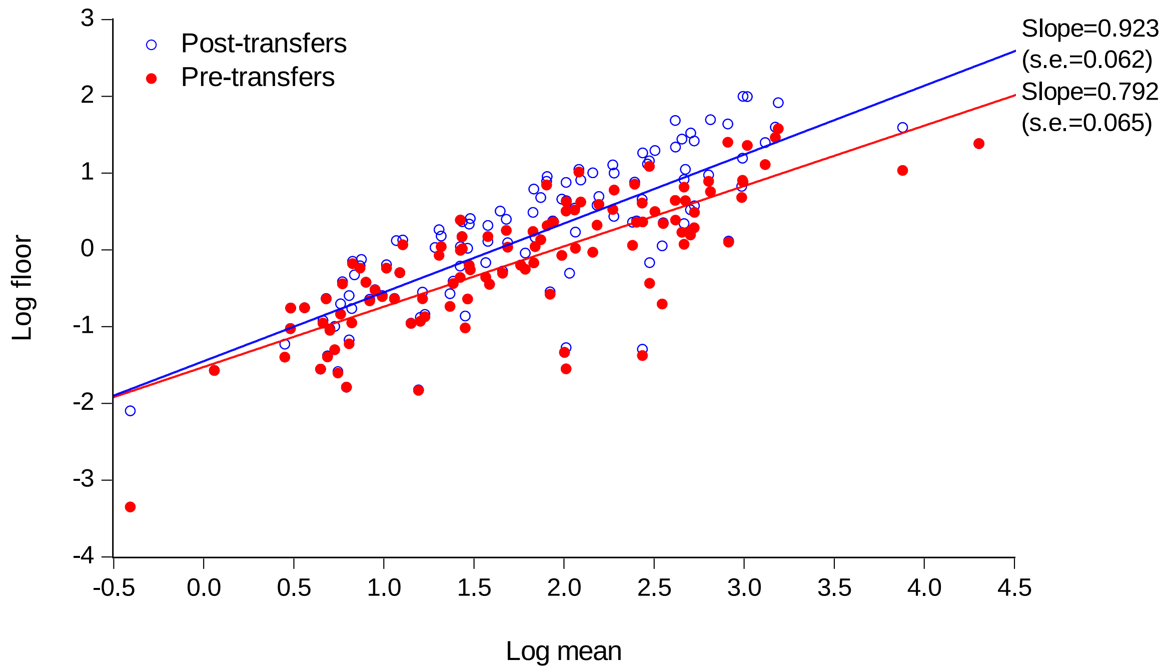


Figure 9: Log pre-transfer floor controlling for the mean plotted against log SP spending per capita

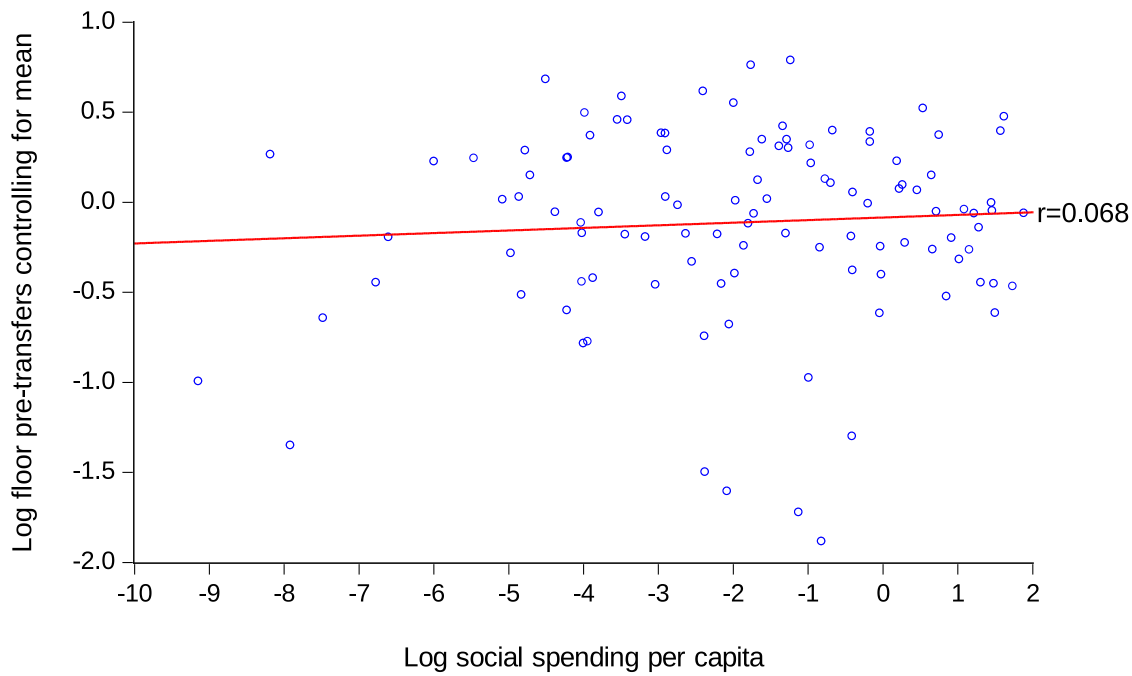
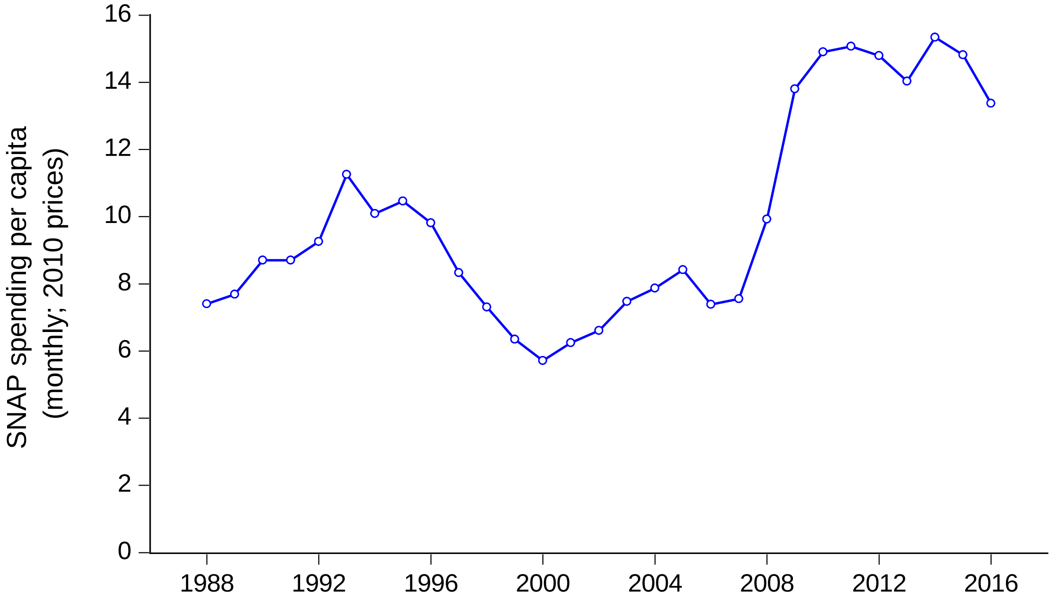


Figure 10: Public spending on SNAP and participation rates

(a) Spending per capita of US population



(b) Spending per recipient and participation rates

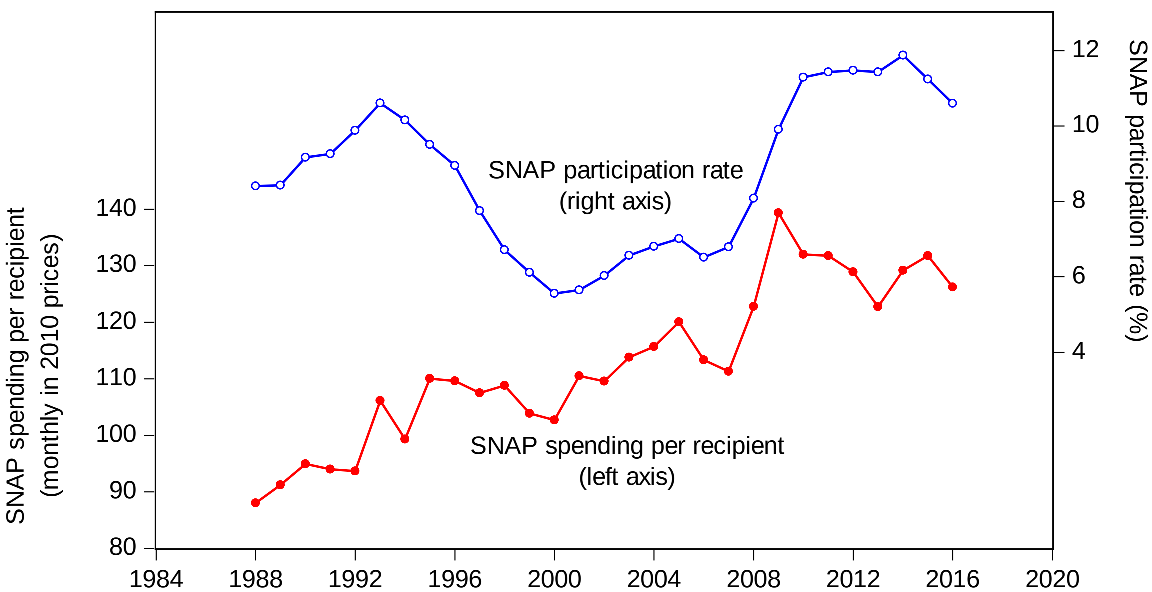
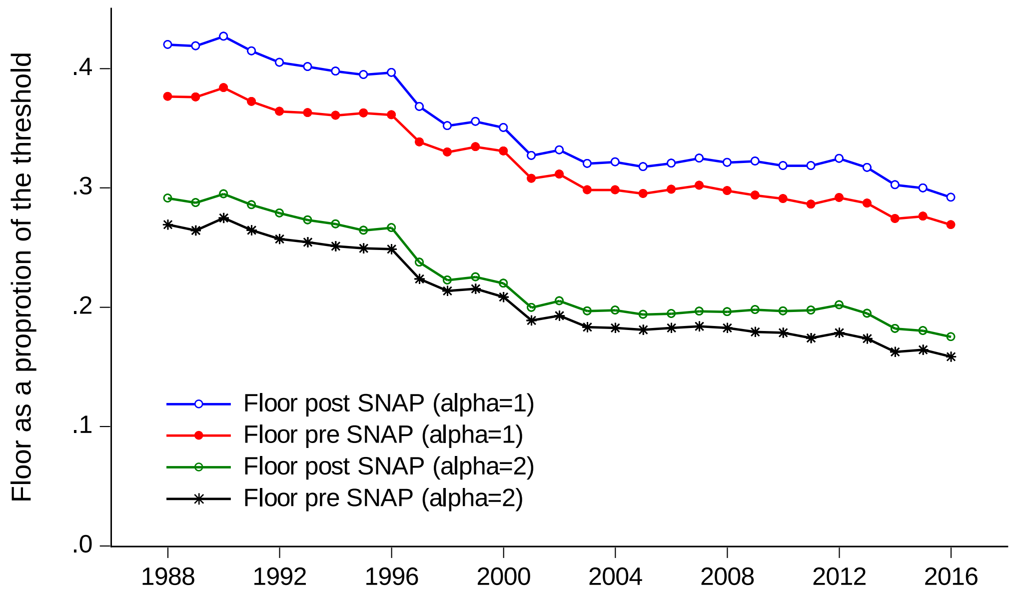


Figure 11: Floor in the United States 1988-2016 before and after food stamps

(a) Official poverty threshold



(b) Quantile of 20th percentile as threshold

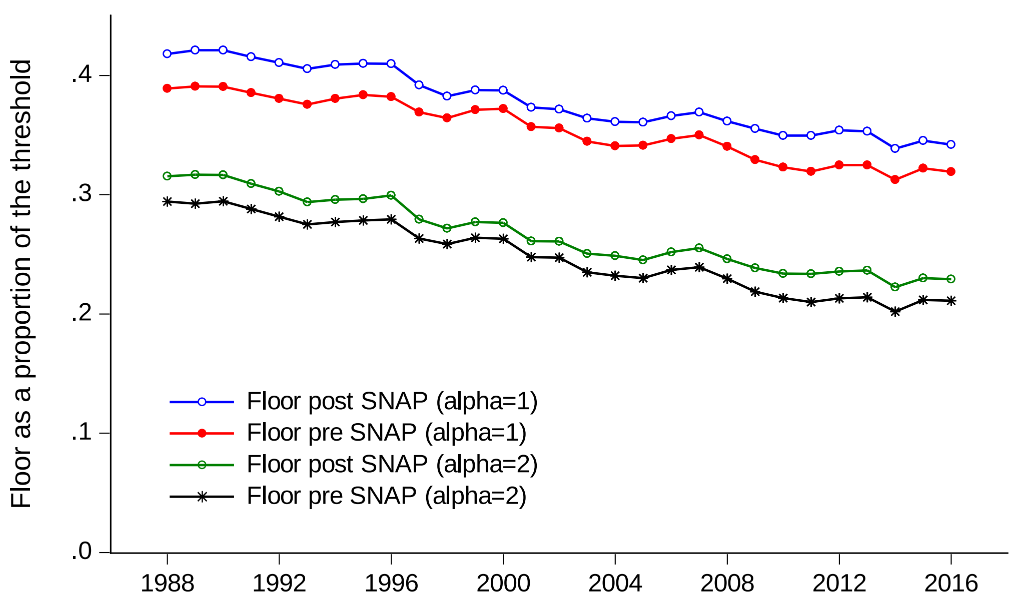
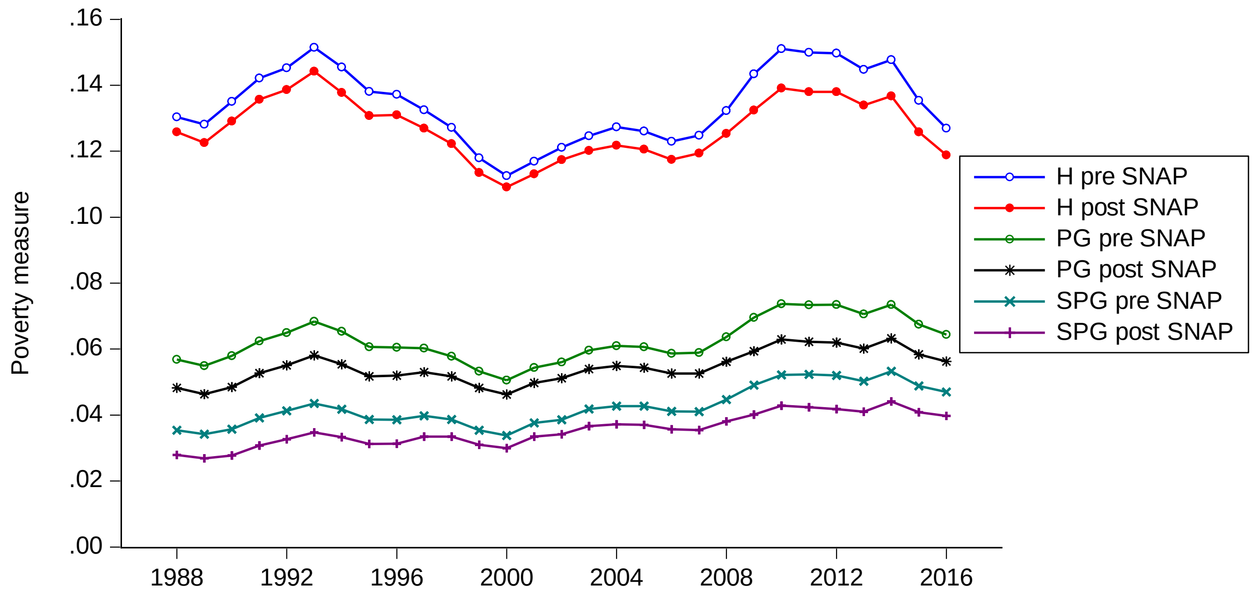


Figure 12: Poverty measures for the US



Note: H: head count index; PG=poverty gap index; SPG=squared poverty gap index

Figure 13: Floor relative to overall mean and various quantiles

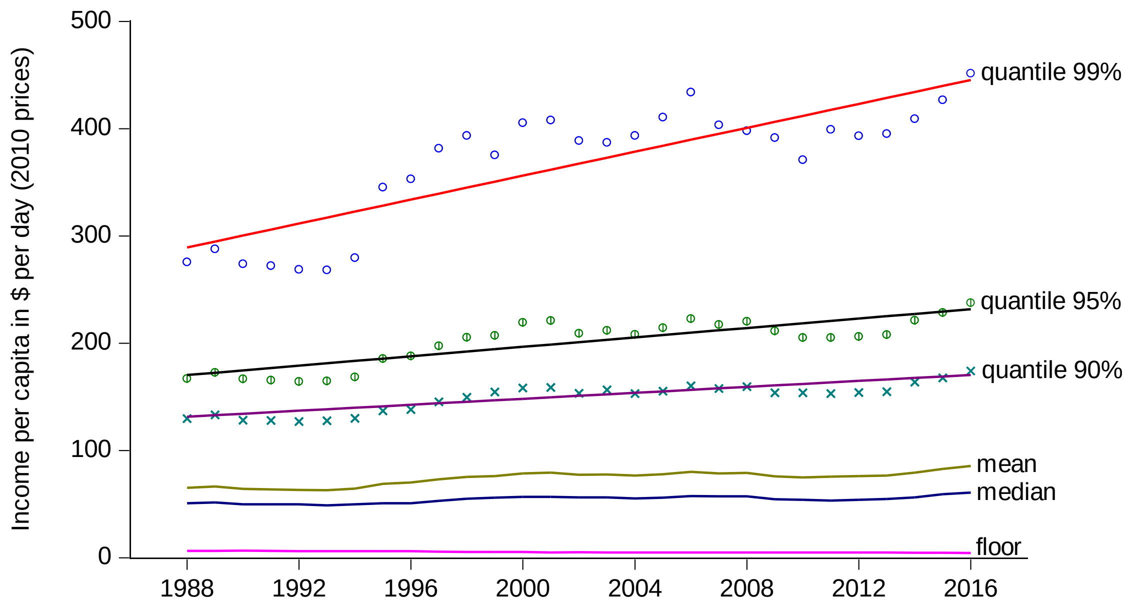
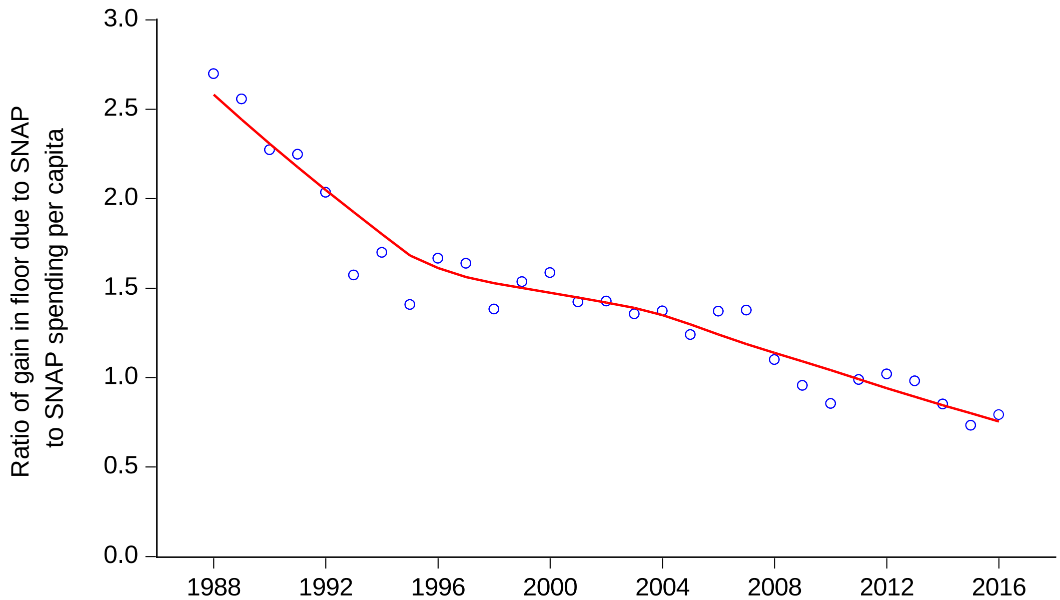


Figure 14: Floor transfer efficiency for SNAP



Note: FTE calculated for a family of four, two adults, two children, i.e., using a poverty threshold of $15.15 a day in 2010 prices.

Table 1: Summary statistics for cross-country data set for developing countries

	N	Mean	St. dev.	Median	Min	Max
Survey mean (m)	121	8.413	9.033	5.973	0.67	74.05
Survey median (y^{med})	121	6.014	6.677	3.817	0.5	55.97
Threshold (z)	121	2.979	2.895	1.963	0.20	17.48
Mean social protection spending (τ)	111	0.876	1.437	0.171	0.00	6.56
Mean contributory pensions	116	0.667	1.195	0.109	0.00	5.51
Mean social assistance	93	0.187	0.321	0.062	0.00	1.92
Floor post transfers ($\hat{y}_{min}^{*post}$)	121	1.693	1.547	1.184	0.12	7.34
Floor post transfers as share of threshold	121	0.580	0.095	0.594	0.228	0.729
Floor pre all transfers ($\hat{y}_{min}^{*pre}$)	111	1.210	0.954	1.009	0.03	4.82
Floor pre transfers as share of threshold	111	0.463	0.139	0.487	0.174	0.728
Floor pre contributory pensions only	116	1.308	1.010	1.101	0.11	5.70
Floor pre social assistance only	93	1.678	1.442	1.184	0.04	6.42
Headcount index pre all transfers (%)	111	26.802	9.531	22.390	20.00	56.83
Headcount index pre contributory pensions (%)	116	24.612	7.586	20.830	18.96	49.25
Headcount index pre social assistance alone (%)	93	22.236	3.913	21.000	19.80	49.00
Poverty gap index post transfers (%)	121	5.744	1.547	5.531	3.51	11.19
Poverty gap index pre transfers (%)	111	10.813	7.251	7.780	3.56	36.90
Squared poverty gap index post transfers (x100)	121	2.556	1.384	2.254	0.952	8.594
Squared poverty gap index pre transfers (x100)	111	6.680	6.246	4.011	0.967	30.489

Note: All values displayed above are in daily per capita US$ units, in 2005 prices (at PPP) unless noted otherwise. SP spending comprises all social insurance, social assistance and labor market programs (see text). The number of countries can vary depending on data availability, as indicated.

Table 2: Regressions for SP spending, cross-country data set

	(1)	(2)	(3)	(4)
	Log total SP transfers per capita ($\ln \tau$)		Log social assistance transfers per capita	
Log median ($\ln y^{med}$)	3.315***	2.226***	1.891*	1.829***
	(1.077)	(0.385)	(1.130)	(0.470)
Log mean ($\ln m$)	-1.156		-0.066	
	(1.285)		(1.396)	
Constant	-4.759***	-5.276***	-6.195***	-6.225***
	(1.083)	(0.663)	(1.311)	(0.843)
R^2	0.427	0.423	0.309	0.308
N	111	111	93	93

Note: OLS regressions. Robust standard errors in parentheses. ***: 1% significance;**: 5%; *10%.

Table 3: Regressions for log floor, cross-country data set

	(1)	(2)	(3)	(4)	(5)	(6)
	Log floor, pre-transfers ($\ln(y_{min}^{*pre})$)		Gain in the floor due to SP spending ($\ln(y_{min}^{*post}/y_{min}^{*pre})$)			
Log SP transfers per capita ($\ln \tau$)	0.027		0.118***	0.095***	0.058**	0.244***
	(0.033)		(0.018)	(0.012)	(0.027)	(0.043)
Log mean income ($\ln m$)	0.740***	0.792***	-0.099*		-0.030	-0.347**
	(0.105)	(0.065)	(0.052)		(0.066)	(0.155)
Interaction effect ($\ln \tau . \ln m$)					0.035**	0.025*
					(0.014)	(0.013)
Log Gap Transfer Efficiency ($\ln GTE$)						0.664***
						(0.138)
Interaction effect ($\ln \tau . \ln GTE$)						0.104***
						(0.014)
Interaction effect ($\ln m . \ln GTE$)						-0.151**
						(0.064)
Constant	-1.399***	-1.544***	0.661***	0.439***	0.497***	1.701***
	(0.249)	(0.131)	(0.128)	(0.039)	(0.152)	(0.340)
R^2	0.629	0.626	0.495	0.462	0.536	0.800
Total effect evaluated at mean points						
$\ln \tau$					0.121***	0.088***
					(0.020)	(0.016)
$\ln m$					-0.098	-0.101**
					(0.064)	(0.051)
$\ln GTE$						0.191***
						(0.029)

Note: OLS regressions. Robust standard errors in parentheses. N=110. ***: 1% significance;**: 5%; *10%.

Table 4: Data and summary statistics for the US

	Mean	Median	Gini index	SNAP spending per capita	SNAP participa-tion (%)	Poverty rate (pre-SNAP; %)	Poverty rate (post-SNAP; %)	Floor ratio (pre-SNAP)	Floor ratio (post-SNAP)
1988	65.23	50.83	0.404	7.40	8.41	13.04	12.58	0.377	0.420
1989	66.44	51.44	0.406	7.68	8.43	12.82	12.25	0.376	0.419
1990	64.25	49.82	0.404	8.70	9.17	13.51	12.91	0.384	0.427
1991	63.63	49.84	0.407	8.71	9.26	14.22	13.57	0.372	0.415
1992	63.25	49.72	0.410	9.25	9.88	14.52	13.86	0.364	0.405
1993	62.98	48.87	0.417	11.26	10.61	15.14	14.42	0.363	0.401
1994	64.28	49.82	0.416	10.09	10.15	14.55	13.78	0.361	0.398
1995	68.84	50.78	0.444	10.47	9.51	13.81	13.07	0.363	0.395
1996	69.99	50.74	0.448	9.81	8.95	13.72	13.10	0.361	0.396
1997	73.00	52.91	0.450	8.33	7.75	13.25	12.69	0.338	0.368
1998	75.38	54.96	0.448	7.30	6.71	12.72	12.22	0.330	0.352
1999	75.98	55.99	0.438	6.34	6.11	11.79	11.35	0.334	0.356
2000	78.54	56.77	0.451	5.71	5.56	11.25	10.91	0.331	0.351
2001	79.18	56.68	0.457	6.24	5.65	11.69	11.31	0.308	0.327
2002	77.32	56.10	0.456	6.60	6.03	12.12	11.74	0.311	0.332
2003	77.53	56.21	0.458	7.48	6.57	12.46	12.02	0.298	0.320
2004	76.59	55.30	0.460	7.87	6.80	12.73	12.18	0.298	0.322
2005	77.90	55.94	0.463	8.42	7.01	12.60	12.05	0.295	0.318
2006	80.08	57.40	0.466	7.39	6.51	12.30	11.75	0.299	0.321
2007	78.52	57.18	0.457	7.56	6.79	12.48	11.94	0.302	0.325
2008	78.93	57.13	0.461	9.93	8.08	13.23	12.54	0.298	0.321
2009	75.88	54.50	0.468	13.80	9.91	14.34	13.24	0.294	0.322
2010	74.89	53.97	0.468	14.90	11.29	15.11	13.91	0.291	0.319
2011	75.50	53.22	0.476	15.06	11.43	14.99	13.80	0.286	0.318
2012	76.00	53.94	0.477	14.79	11.47	14.97	13.80	0.292	0.325
2013	76.55	54.64	0.475	14.03	11.43	14.48	13.39	0.287	0.317
2014	79.30	56.30	0.479	15.34	11.87	14.77	13.67	0.274	0.302
2015	82.80	59.27	0.476	14.81	11.24	13.54	12.58	0.276	0.300
2016	85.40	60.59	0.478	13.37	10.60	12.70	11.88	0.269	0.292
Mean	73.94	54.17	0.449	9.95	8.73	13.41	12.71	0.322	0.351
St. dev.	6.39	3.15	0.025	3.15	2.04	1.13	0.92	0.036	0.043

Note: Monetary values in real 2010 $US per day except SNAP is per month. Gini index is for families (as usually calculated for the US). Poverty rate as % of population. Floor as a proportion of the threshold using $\alpha = 1$ and official poverty threshold. Authors' calculations based on CPS micro data.

Table 5: Regressions for the US floor 1988-2016 before and after SNAP

	(1)	(2)	(3)	(4)	(5)	(6)
	Using official poverty threshold to define the reference group		Using the poorest 20% as the reference group			
	Log floor pre-SNAP $(\ln y_{min}^{*pre})$	Gain in the floor due to SNAP $(\ln(y_{min}^{*post}/y_{min}^{*pre}))$	Log floor pre-SNAP $(\ln y_{min}^{*pre})$	Gain in the floor ratio due to SNAP $(\ln(y_{min}^{*post}/y_{min}^{*pre}))$	Impact on log quantile for p=0.20 $(\ln q^{post}(0.2)/q^{pre}(0.2))$	
Lagged dep.var.	0.741***	0.324**	0.696***	0.223	0.119	
	(0.084)	(0.120)	(0.130)	(0.113)	(0.104)	
$\ln \tau$	-0.019	0.029***	-0.036	0.034***	0.031***	0.028***
	(0.019)	(0.006)	(0.025)	(0.005)	(0.004)	(0.005)
$\ln m$	-0.244**	-0.065***	-0.154**	-0.043**	0.017	
	(0.090)	(0.021)	(0.099)	(0.014)	(0.020)	
Control for inflation rate	0.904**	0.287***	0.456**	0.183**	0.109	
	(0.354)	(0.096)	(0.190)	(0.067)	(0.134)	
Constant	0.763	0.265***	0.413	0.153**	-0.123	-0.038***
	(0.340)	(0.093)	(0.223)	(0.064)	(0.092)	(0.011)
R^2	0.972	0.898	0.951	0.931	0.775	0.674
DW	2.450	1.978	1.717	1.615	1.710	1.443

Note: N=28 (1988 lost due to lag). HAC standard errors in parentheses. ***: 1% significance;**: 5%; *10%. Floor ratio is used. Monetary variables are daily per capita. The control for inflation is the first difference in the log CPI.