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POUR OBTENIR LE GRADE DE

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SPÉCIALITÉ SCIENCES ÉCONOMIQUES

Par **Ulugbek AMINJONOV**

**Essais en santé sur la pandémie de COVID-19 et
en économie du développement**

**Essays in Health on the COVID-19 Pandemic and
Development Economics**

Sous la direction de : **Olivier BARGAIN**

Soutenue le 29 juin 2023

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Essais en santé sur la pandémie de COVID-19 et en économie du développement

Résumé : Cette thèse vise à aborder deux grands thèmes de l'économie de la santé et du développement. Les deux premiers chapitres se concentrent sur les facteurs à l'origine des différences de comportement en matière de distanciation sociale pendant la pandémie de COVID-19. Le chapitre 1 étudie le rôle de la confiance politique dans le respect des règles de santé publique dans les pays européens. Nous constatons qu'un degré plus élevé de confiance dans le gouvernement est associé à une diminution plus importante de la mobilité humaine en réponse aux politiques de confinement. Le chapitre 2 étend cette question au contexte des pays en développement en montrant qu'une incidence plus élevée de la pauvreté entraîne une baisse moins importante de la mobilité liée au travail, dont une partie est atténuée grâce aux programmes de soutien au revenu. La deuxième partie de la thèse applique les connaissances des modèles collectifs de ménage pour examiner les modèles d'inégalité intra-ménage dans le partage des ressources. Le chapitre 3 fournit une estimation globale du partage des ressources dans le cadre de la pauvreté des ménages et des individus pour 32 pays à revenu faible et intermédiaire, en mettant particulièrement l'accent sur les schémas liés au genre et aux enfants. Cette étude est complétée au chapitre 4 par une évaluation de l'impact des chocs pluviométriques sur la manière dont les ressources sont allouées au sein des ménages et ses implications pour la pauvreté individuelle au Malawi. Les résultats montrent que l'exposition à une anomalie pluviométrique entraîne une redistribution des ressources des femmes vers les hommes, ce qui fait peser sur les femmes la charge la plus lourde au sein du ménage.

Mots-clés : COVID-19, Conformité, Mobilité, Confiance politique, Pauvreté, Enfermement, Protection sociale, Modèle collectif, Règle de partage, Pauvreté individuelle, Inégalité intra-ménage, Choc climatique, Précipitations.

Essays in Health on the COVID-19 Pandemic and Development Economics

Abstract: This thesis aims to address two broad topics in health and development economics. The first two chapters focus on factors driving the differences in social distancing behavior during the COVID-19 pandemic. Chapter 1 investigates the role of political trust in compliance with public health rules in European countries. We find that a higher degree of trust in government is associated with a larger decrease in human mobility in response to lockdown policies. Chapter 2 extends this question to the context of developing countries by documenting that a higher incidence of poverty leads to a smaller decline in work-related mobility, a part of which is mitigated thanks to income support programs. The second part of the dissertation applies the insights from collective household models to examine the patterns of intra-household inequality in resource sharing. Chapter 3 provides a global estimates of resource sharing within household and individual poverty for 32 low- and middle-income countries, with a particular focus on gender and child-related patterns. This study is further complemented in Chapter 4 by assessing the impact of rainfall shocks on how resources are allocated within household and its implications for individual poverty in Malawi. The results show that exposure to a rainfall anomaly leads to a redistribution of resources from women to men, hence putting the heaviest burden within household on women.

Keywords: COVID-19, Compliance, Mobility, Political trust, Poverty, Lockdown, Social protection, Collective model, Sharing rule, Individual poverty, Intra-household inequality, Climate shock, Rainfall.

To my son Akmal

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Introduction

The COVID-19 pandemic has brought unprecedented challenges to societies around the world, affecting virtually every aspect of people's lives. One of the most significant problems has been the necessity to promote public compliance with social-distancing measures, which is critical to mitigate the spread of the virus. At the same time, the pandemic has highlighted and exacerbated longstanding issues related to poverty and inequality. This in turn has raised an urgency to refine targeting strategy in redistributive policies to effectively reach the poorest and the most vulnerable parts of population, especially in times of crisis. Hence, by examining these two distinct but still interconnected questions, this dissertation seeks to shed light on (i) the factors driving compliance attitudes towards public health rules during the COVID-19 pandemic, and (ii) the patterns of intra-household resource allocation that ultimately allow measuring poverty at the individual level (for men, women and children), as opposed to standard poverty indicators based on household-level information.

Compliance during the COVID-19 Pandemic

In early 2020, the global outbreak of COVID-19 triggered the enactment of shelter-in-place and social distancing policies throughout the world. Ranging from nationwide lockdowns to self-compliance recommendations, these measures were employed as a key means to contain the transmission of the novel coronavirus, especially in the absence of a vaccine at the early stages of the pandemic. Many studies have shown that strict containment policies significantly contributed to slowing down the spread of the virus and reducing death toll ([Chinazzi et al., 2020](#); [Hsiang et al., 2020](#); [Aubert and Augeraud-Véron, 2021](#)). However, their effectiveness, as determined by the level of public compliance, did vary in different parts of the globe. In general, how people comply with public health rules may depend on many factors associated with local circumstances or individual preferences, including civic culture, political beliefs, or welfare. Given its immense importance for future policy decisions, understanding the roots of heterogeneous response to anti-contagion measures became one of the crucial tasks at the onset of the COVID-19 pandemic.¹ Along this line, the first two chapters of this thesis focus on factors driving the disparities in social distancing behavior upon the introduction of COVID-19 containment policies.

¹See the survey by [Brodeur, Gray, Islam and Bhuiyan \(2021\)](#) on COVID-19 related studies.

Undoubtedly, one of the foremost determinants of compliance with public rules is civic values of citizens. Stronger civic culture, built around trust in government, is often suggested to improve citizens' involvement and cooperation in government policies (Helliwell and Putnam, 1995; LaPorta et al., 1997; Knack and Keefer, 1997; Scholz and Lubell, 1998; Knack, 2002). Before the COVID-19 pandemic, little was known about the effects of trust, or civic norms in general, on compliance with public health rules.² Chapter 1 is one of the first studies to address this question in the context of the COVID-19 pandemic. In this paper, which is a joint work with Olivier Bargain, we examine whether pre-pandemic level of trust in authorities affects the level of compliance to lockdown policies. Using mobility data at subnational level in Europe, we show that districts with a higher level of political trust exhibit a larger reduction in non-essential human mobility following the implementation of containment policies in March 2020. We interpret this effect as a relatively higher level of compliance with containment rules in high-trust regions. Additionally, we find that the effectiveness of policy stringency in terms of mobility reduction significantly increases with trust.³

In fact, the role of civic norms during the COVID-19 pandemic have been discussed predominantly in the context of developed economies, rather than developing countries. Indeed, civic qualities or trust might have played little role in defining the response of citizens to lockdown policies in low-income settings (Egger et al., 2020). Poor households usually have limited financial buffer and food stocks, depend on daily earnings, and in most cases cannot work remotely (Saltiel, 2020; Garrote Sanchez et al., 2021). In this sense, they simply could not afford to stay at home when COVID-19 mobility restrictions were in place (Durizzo et al., 2021; Carlitz and Makhura, 2020; Bennett, 2021; Bargain and Aminjonov, 2021), as they have to seek out income generating activities for daily livelihoods. Hence, poverty was not only increased by the pandemic (Decerf et al., 2020; Sumner et al., 2020; Egger et al., 2021; Gutierrez-Romero and Ahamed, 2021), but also might have fueled its growth by limiting the ability of people to comply with stay-at-home orders. On the other hand, income support programs, which were initiated by governments to preserve livelihoods and avoid sharp increases in extreme poverty during the COVID-19 pandemic, might have also allowed the poor comply with mobility restrictions. In Chapter 2 of this dissertation, together with Olivier Bargain and Tanguy Bernard, we provide a global evidence on the role of income support in reducing human mobility and thus, generating positive health externalities among the poor during the COVID-19 pandemic. We mobilize information on poverty and work-related mobility across 729 subnational regions of 43 low- and middle-income countries and exploit time and spatial variation in stay-at-home orders and income support policies in 2020. We find that a poverty gap in mobility reduction during lockdowns was partially mitigated by income support programs, thus, implicitly facilitating compliance with mobility restrictions and containing the spread of COVID-19.

²Only exception is Blair et al. (2017) showing that distrust in government deteriorated compliance with containment rules during the Ebola epidemic.

³In the context of the COVID-19 pandemic, several papers find similar results by focusing on country-specific contexts (e.g. Durante et al., 2021 in Italy, and Brodeur, Grigoryeva and Kattan, 2021 in the US) or using different proxies of civic capital, such as electoral participation (Barrios et al., 2020; Bartscher et al., 2020) or political beliefs (Engle et al., 2020).

The first two chapters bring several contributions to the literature as well as policy. First, we illustrate that regional differences in attitudes towards government are relevant for the effectiveness of nationwide emergency policies, hence should be taken into account in policy design. This also adds to a relatively understudied branch of literature on the role of political trust in times of public health crisis. Second, we provide a large-scale evidence supporting the use of income support programs as a part of policy response during an epidemic, not only to preserve livelihoods of the poor, but also to improve their ability to comply with strict containment rules that are inevitable to stop the outbreak. This analysis at the global scale complements a few existing country-specific studies on health benefits of social transfers in times of a pandemic (e.g. [Brooks et al., 2020](#); [Banerjee et al., 2020](#)).

Intra-household Inequality and Individual Well-being

The previous chapter highlights the utmost importance of social safety nets for the poorest, especially in times of crisis. While these policies ultimately aim at targeting poor *individuals*, measuring poverty at the individual level and identifying poor individuals within families (or extended families) is complicated. Traditionally, welfare analysis is based on household-level information and ignores the extent of intra-household inequalities, failing to address the needs of poor individuals living in non-poor households (see evidence in [Haddad and Kanbur, 1990a](#); [Brown et al., 2019](#); [Klasen and Lahoti, 2021](#)). Data on individual consumption is costly or sometimes impossible to obtain; some aspects are also not directly measurable, such as economies of scale from consuming goods jointly in the household (i.e. goods with a degree of ‘publicness’). A promising research avenue is the use of behavioral models of household decision-making that allow modeling household choices (in terms of consumption, saving, labor supply, etc.). In fact, the latest developments in the collective household models allow extracting the shares of resources allocated to each family member, along with the identification of economies of scale, from standard household survey data and, hence, measuring poverty at the individual level ([Browning et al., 2013](#); [Bargain and Donni, 2012](#); [Dunbar et al., 2013](#); [Bargain, Donni and Hentati, 2022](#)). This approach is rarely operationalized and there is still room for many extensions which could contribute to (i) measuring child poverty and gender-based poverty (using individual resources rather than standard definitions) more systematically and at a large scale, and (ii) understanding intra-household response to various shocks which would allow identifying the most ‘affected’ or vulnerable individuals. Thus, the second part of this thesis aims at addressing these two questions.

In Chapter 3, which is a joint work with Olivier Bargain and Maira Colacce, we employ the collective household framework to provide a global mapping of gender gap in consumption and child poverty. We mobilize microdata for 33 low- and middle income countries to estimate the share of household resources allocated to each member systematically at a global scale. The estimates reveal a consistent pattern of within-household gender inequality in resource sharing. Men’s and children’s resource shares tend to exhibit a Kuznets pattern of intra-household

inequality, with children’s resources increasing and then decreasing with living standards - symmetrically to men’s resource shares. In addition, using cross-validation with nutrition data both at macro and micro level, we show that child undernutrition is likely driven both by low living conditions and intra-household inequalities.

A global estimation of how household resources are allocated among its members – as suggested in the previous chapter – is already a big step forward in measuring intra-household inequalities and individual poverty. The next question that policymakers might be interested in is whether income shocks affect the way resources are allocated among household members, in other words, the income ‘gradient’ of resource sharing. There is some evidence on how positive income shocks such as cash transfers change intra-household allocation of resources (Bobonis and Finan, 2009; Braido et al., 2012; Bergolo and Galván, 2018; Tommasi, 2019), especially in the case of transfers whose labelling or ‘framing’ is targeted at certain persons in the households, or transfers that are given to specific members (e.g. women). At the same time, gender-differentiated negative income shocks, for example, affecting mostly men during the Great Recession (i.e. ‘manceSSION’) can increase the share of resources accruing to women, mainly due to the improvement in their relative labor market status within household (Bargain and Martinoty, 2019). In the same spirit, the final chapter of this dissertation investigates the intra-household response, in terms of resource sharing, to climate shocks that are on the rise in all parts of the world. By nesting rainfall anomalies, as random negative income shocks, into the household resource sharing model in the context of Malawi, I show that exposure to climate shock is associated with a lower share of resources allocated to women. This pattern of intra-household redistribution aggravates the negative income effect of the shock for women. The effect is more pronounced when employment gap between men and women is larger, suggesting that the observed change in resource sharing due to the shock is likely motivated by ‘life-boat’ ethics, that is, nourishing the members with higher marginal productivity and potential to bring money to the household in times of hardship.

Several contributions emerge from this part of the thesis. Our global estimation of resource sharing is a one of the rare studies that unveil, at a large scale, the systematic presence of intra-household inequality in resource sharing and subsequent patterns of gender-based and child poverty. This is complemented by an original illustration of how households may revise their sharing rule when affected by a climate shock, resulting in unequal impacts of climate change within household. Importantly, findings of both studies contribute to identifying and targeting poor as well as the most affected *individuals* by taking into account intra-household inequalities (or intra-household response to a shock) in welfare analyses. Lastly, by employing this methodology to as many countries as possible, we illustrate the scope of its application and cross-validation of resource share estimates against a directly observed measure of well-being.

Part I

COVID-19: Trust, Poverty and Social Distancing

Chapter 1

Trust and Compliance to Public Health Policies in Times of COVID-19

This chapter is based on a joint work with Olivier Bargain and published in Journal of Public Economics.

1.1 Introduction

The COVID-19 pandemic has spread rapidly and globally since February-March 2020. Shelter-in-place and social distancing measures have been enacted or recommended all over the world to slow down transmission and reduce both the load on the healthcare system and overall mortality. In this context, the compliance to health policy rules is crucial and may vary with the local context so that policy measures may not be equally effective in different parts of the globe. In particular, the way people abide to containment measures may depend on the degree of confidence in the authorities. Yet little is known about the effect of trust on compliance to health and safety rules. Trust has received a lot of attention in the economic literature (see the survey by [Algan and Cahuc, 2014](#)) and beyond (e.g., [Fukuyama, 1995](#)). Specific forms of trust are investigated, notably citizens' trust in institutions and decision-makers, which are shown to improve regulation efficiency and voluntary compliance to rules and laws.¹ Recent social movements in France (yellow jackets) and elsewhere have also reminded us that a spreading distrust in institutions can harm social cohesion and economic stability. There are very few

¹This literature highlights the relationship between general trust and many outcomes such as trade or economic development. Political trust and civic norms in particular allow cooperation when large-scale collective action is needed. They improve citizen involvement and governmental performance ([Knack, 2002](#); [Helliwell and Putnam, 1995](#); [LaPorta et al., 1997](#); [Knack and Keefer, 1997](#)), tax compliance (e.g. [Knack and Keefer, 1997](#); [Scholz and Lubell, 1998](#)) or the decision to report crimes ([Tyler, 2006](#)).

studies investigating the role of trust and compliance in the face of a massive pandemic.²

Against this background, we exploit regional variation in political trust throughout Europe to test whether confidence in authorities prior to the crisis affects the compliance to lockdown policies, as measured by the change in human mobility. We first provide graphical evidence then adopt a double difference approach around the time of lockdown announcements. We also use the daily intensity of policy stringency as a more continuous source of variation in treatment, both over time and across countries. Most European countries have enacted measures of varying severity, from strict suppression methods (including generalized lockdown, enforced social distancing and the closure of school and non-essential economic activities) to milder mitigation approach (for instance in the UK at first, and in Sweden throughout the period). We check whether trust improves the efficiency of policy stringency. We combine three main data sources: COVID-19 mobility reports from Google, trust data from the European Social Survey (ESS) and policy stringency from the Oxford COVID-19 Government Response Tracker.³

We find that the decline in mobility around mid-March 2020 is significantly stronger in high-trust regions. We interpret it as the result of better compliance to national health policies in regions that demonstrated higher levels of trust in policy makers prior to the crisis. The effect is especially strong for non-necessary activities (recreation, work and transport) compared to going to the grocery or to the drugstore, i.e. essential activities allowed by most of the national shelter-in-place policies. The effect of trust is similar whether we adopt a simple difference over the lockdown period of March 2020 or a difference-in-difference approach, and whether we use the ESS data on trust in politicians or alternative measure (ESS satisfaction in governments or Eurobarometer trust in government). Next, we observe a significant impact of the stringency of lockdown measures on mobility in European regions but the diminishing effect is larger in high-trust regions. The overall effect of trust coincides on average with this mediating effect on the efficacy of policy stringency. Using a continuous measure of stringency allows detecting nonlinearities: the effect of trust increases with the degree of stringency and we find no evidence of a sign reversal at very low stringency levels (i.e. as would happen if low-trust regions self-isolated more than the rest because they doubt the ability of the government to

²Some studies examine trust in the health system (Ozawa and Sripad, 2013) and how it affects vaccine hesitancy or the use of healthcare (e.g. Woskie and Fallah, 2019). Blair et al. (2017) provides an original account of how people who distrusted government were less compliant with Ebola control policies.

³Focusing on Europe already yields a large enough sample of regions (and exploitable variation in trust and mobility) while it also provides a relatively homogeneous ground to study the impact of civic values. Several papers follow a similar logic by exploiting county variation in the US. Brodeur, Grigoryeva and Kattan (2021) proceed as we do with trust data and Google mobility reports: they find that stay-at-home orders reduce mobility more in high-trust counties. Similar results are found in studies using different notions of civic values: Barrios et al. (2020) focus on electoral participation to proxy civic capital (stating that voting is the ultimate example of civic duty) while Engle et al. (2020) show more response to local restriction orders in counties that did not support the Republicans during the last presidential elections. Another paper examines mobility variation in Europe, yet using only broad country variation and proxying civic values by voter turnout at European elections (Bartscher et al., 2020). Other papers use disaggregated variation within specific countries, such as recent evidence on the ‘willingness to distance’ in Denmark (Olsen and Hjorth, 2020) or variation in civic capital in Italy, also shown to mediate the social distancing process (Durante et al., 2021). Finally, several papers provide cross-country evidence on how lockdown policies can curb the epidemic using mobility patterns (Hale et al., 2020; Askitas et al., 2020).

respond appropriately). Finally, we assess how the impact of trust on mobility translates in terms of death growth rate.

1.2 Data Sources

To analyze the impact of trust on mobility and, subsequently, on mortality, we mobilize several types of data: the Google mobility index, trust from various sources, the Oxford measure of policy stringency, official information on COVID-related deaths and control variables.

1.2.1 Mobility

We use the human mobility index by [Chan et al. \(2020\)](#), constructed from the Google COVID-19 mobility reports. These reports aggregate anonymized sets of data from users' mobile device Location History. The mobility index measures how visits to, or length of stay at, different types of location change over time compared to a baseline period corresponding to January 3-February 6, 2020.⁴ There are six location categories: (i) retail and recreation, (ii) grocery and pharmacy, (iii) parks (public gardens, dog parks, beaches, etc.), (iv) transit stations (public transport hubs such as subway, bus, train stations), (v) workplaces and (vi) residential areas. For the first five categories, one can expect a significant drop in mobility during the COVID-19 pandemic while the index for private residence, i.e. the length of staying at home, is supposed to increase. Human mobility is tracked by Google daily and in a consistent manner across 131 countries for the period from February 16 to April 5, 2020. For a subset of countries, the information is provided at sub-national level and we combine it with trust data for most of the European regions.

Figure 1.1a reports mobility at the country level using the index for “retail and recreation”, but very similar patterns are obtained with the other activities. The horizontal axis represents the February 16 - April 5 period with March 1 taken as day 0. Early calls for self-isolation were made in Italy, the first European country affected by COVID-19, and we see a decline in mobility in the first days of March for this country. The first strict official lockdown was enacted on March 9 in Italy. Most European countries tend to follow, with a sharp drop in mobility around mid-March and a lower (containment-level) plateau reached within 10 days. There are a few exception (notably a long hesitation in the UK and the mild mitigation policy in Sweden throughout the period).

Arguably, these mobility patterns reflect both spontaneous behavioral responses to the local gravity of the pandemic and the way people understand, agree and comply with governmental messages and measures: this acceptance/compliance dimension is what may vary with trust levels and what we test hereafter. Finally, note in the graphs that the cross-country variance in mobility is relatively small before lockdown, and increases enormously afterwards due to the variety of country responses. Our approach based on policy stringency will account for such

⁴See <https://www.google.com/covid19/mobility/>

country heterogeneity when attempting to capture the effect of trust.

1.2.2 Trust and Policy Stringency

Trust. To measure trust at the regional level, we use the 8th wave of the European Social Survey (ESS). For the year 2016, it asked respondents about their trust in politicians in the country on a scale of 0 to 10 (with 0 meaning “No trust at all” and 10 “Complete trust”).⁵ For estimations, we aggregate this information at the regional level. A *continuous* measure of trust is calculated as the regional share of respondents whose score is above the country mean score.⁶ For convenient interpretations, we also use a *binary* trust measure, distinguishing regions with an average trust score above national average (indicated as the ‘trust’ group on the graphics presented below) or below (indicated as ‘distrust’).⁷ For robustness checks, we will also use a question from the ESS on individual satisfaction with the work of the national government, as well as the political trust question from the Eurobarometer (the Flash Eurobarometer 472 records the share of those who tend to trust their national government at the regional level).

Note that we use trust measures that are prior to the COVID crisis and hence not affected by the way different governments have managed this crisis.⁸ In that sense, we aim to grasp profound differences across European regions in terms of civic norms and trust in the political system. Growing evidence suggest that trust attitudes, like other cultural traits, can persist for surprisingly long periods of time at national and sub-national levels (Bjørnskov, 2007), with regional differences shaped by past political and social developments (Tabellini, 2010). At the same time, we use relatively recent data (2016 for ESS and 2018 for the Eurobarometer) since part of the answer on trust is context-dependent and reflect confidence in the recent governments.

Policy Stringency. We use data on policy stringency from the Oxford COVID-19 Government Response Tracker (OxCGRT). This tracker implemented by the University of Oxford’s Blavatnik School of Government systematically collects information on the measures taken by governments to tackle the pandemic since February 2020.⁹ OxCGRT is based on publicly available information on 13 indicators of government response (policies such as school closures, bans on public gatherings or travel, etc., and financial indicators such as fiscal or monetary measures). Each indicator is rescaled to get a score between 0 and 100 (100 representing the highest degree of strictness/restriction). The composite stringency index we use is the daily average value of

⁵The size of ESS datasets ranges from 880 observations (Iceland) to 2852 (Germany).

⁶Note that we could use a cutoff that is common to all the countries, such as a fixed score of 5 on the scale – but our aim is to capture regional variation within country especially, which would not be possible with a common threshold (for instance, most of the individual scores are above 5 in Scandinavian countries). Our conclusions are robust to alternative ways of aggregating trust at regional level, in particular when using the share of scores above the country median or directly the regional average trust score.

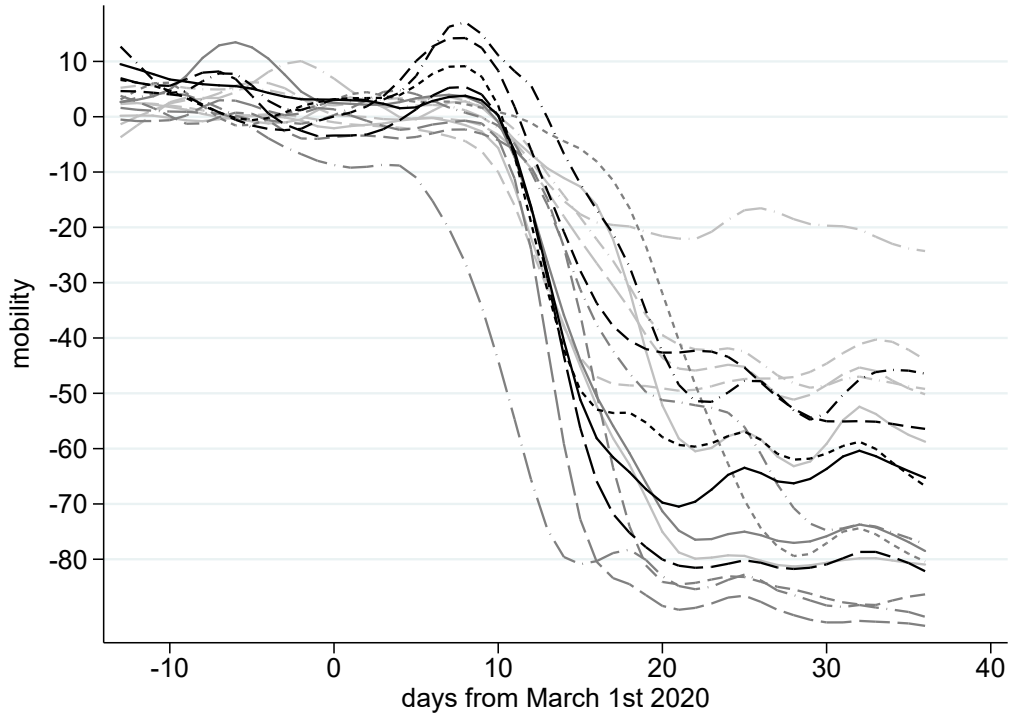
⁷Results are very similar whether we use national mean or median.

⁸Ongoing research aims to assess how citizens’ trust in the government respond to information about the policy response to the pandemic (Khan et al., 2020). Past studies show that effective public intervention to contain Ebola outbreaks might have increased trust in authorities (Flückiger et al., 2019).

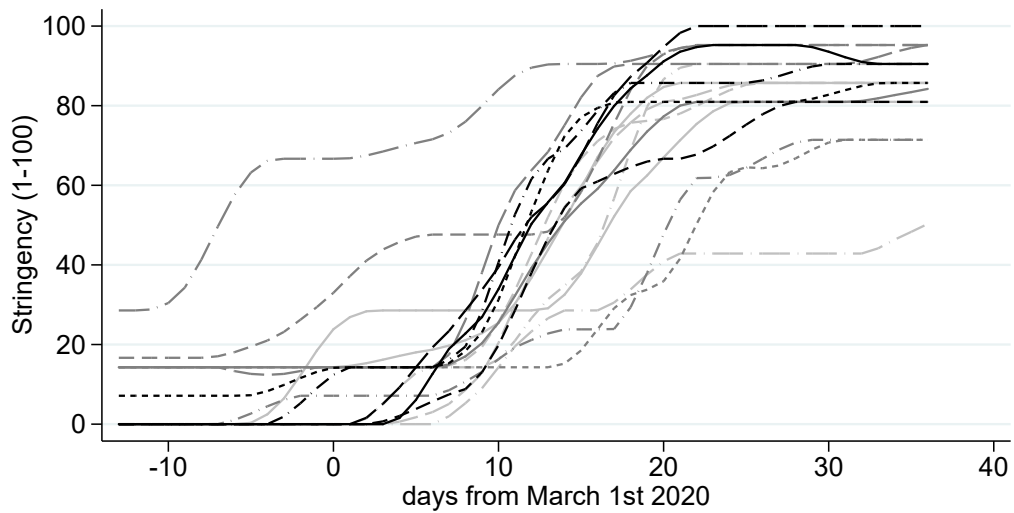
⁹<https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker>

Figure 1.1: Daily Mobility and Lockdown Stringency in Europe around March 2020.

(a) Mobility Trends



(b) Stringency Trends



Source: Google mobility data and stringency data from Oxford COVID-19 Government Response Tracker (OxCGRT).

these indices on a 0-100 scale. [Hale et al. \(2020\)](#) describe the data in detail.¹⁰ In Europe, stringency increases as the number of COVID-19 cases rises exponentially around mid-March. [Figure 1.1b](#) reports country-specific patterns, which mirror national mobility trends and hence indicate the effectiveness of policy measures overall.

1.2.3 COVID-related Deaths and Control Variables

After combining mobility, ESS trust data and policy stringency, our final sample (with non-missing values in key variables) includes 233 regions in 19 European countries over a period of 50 days starting from February 16, 2020.¹¹ Our estimations additionally control for the number of COVID-19 related deaths reported on the day before, at the country level, as this may alter individual mobility behavior. The data on COVID-19 deaths is obtained from the daily updates of the European Centre for Disease Prevention and Control (ECDC).¹² We also include regional characteristics, namely the 2019 unemployment rate (taken from EUROSTAT data) and the population density (number of people per square kilometer in the region, taken from EUROSTAT for 2018 and completed by 2016/2017 ESS data when missing).

1.3 Empirical Approaches and Results

We opt for a step-by-step presentation where we describe the empirical approach and directly provide the corresponding results. We start with the direct effect of trust on mobility, ultimately using the timing of lockdown policies for a difference-in-difference approach confronting high and low trust regions. We then use policy stringency as a more time-varying treatment variable to examine the effect of trust. While our main outcome is human mobility, we also provide suggestive evidence on the potential impact of trust on the mortality growth rate.

1.3.1 The Direct Effect of Political Trust on Mobility

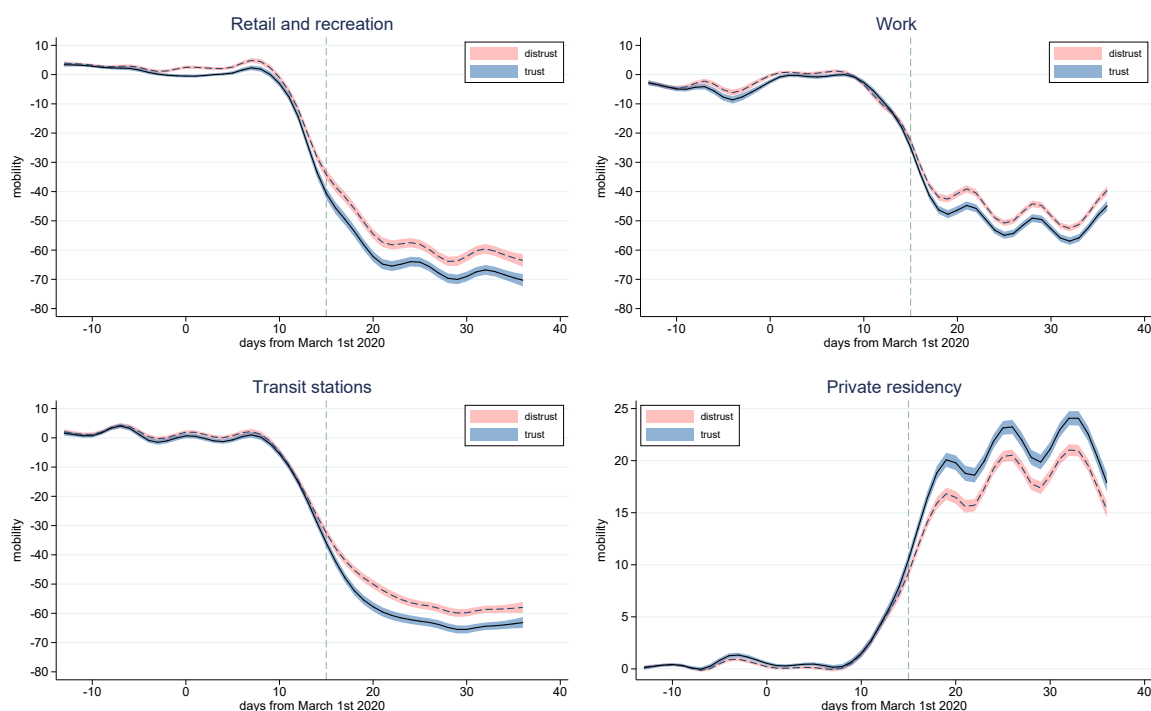
Graphical evidence. We first check the direct role of political trust as a shifter of the overall mobility of European citizens around March 2020. In [Figure 1.2](#), we begin with graphical evidence using regional mobility trends for non-essential activities – likely to be impacted by policy responses to the pandemic (recreation, work, transport) – or, symmetrically, the index of time spent at home. In each graph, we use a local polynomial fit of the daily variation across regions of Europe and its 95% confidence interval (CI). The horizontal axis represents dates with March 1 taken as day 0. The vertical dashed line represents the average lockdown date in Europe. Before that point, the variance in mobility across European regions is small while it increases much afterwards, reflecting the diversity of behavior and policy responses across Europe as depicted in [Figures 1.1a](#) and [1.1b](#).

¹⁰They show that the positive correlation between stringency and the reported number of COVID-19 cases in early March is driven by Asian countries and tends to disappear as many more countries get infected.

¹¹With Eurobarometer trust data, the sample is slightly different, with 171 regions in 18 European countries (it does not contain Estonia and Norway while the ESS data does not include Denmark and Romania).

¹²<https://www.ecdc.europa.eu/en/publications-data/download-todays-data-geographic-distribution-covid-19-cases-worldwide>

Figure 1.2: Daily Mobility and Political Trust (ESS):
Variation across European Regions (local polynomial fit)



Source: authors' calculations based on Google mobility data and ESS data on trust in politicians. Areas represent the 95% CI of average daily mobility across European regions, weighted by 1/number of regions in the corresponding country. Distrust indicates regions, within each country, with trust level below country average.

We see that the relative mobility indices in late February and early March is close to zero, indicating no difference compared to the prior benchmark period (Jan. 3 - Feb. 6). Most importantly, low and high trust groups show very similar trends and only tiny differences in mobility levels at this early stage. We then observe the sharp reduction in mobility (or increase in time spent at home – last graph) following national lockdown measures or recommendations. This drop is more pronounced in the group of regions characterized by higher levels of political trust, and the difference persists until the end of the period of observation. It is also suggestive to see that this pattern mainly concerns non-necessary activities. Indeed, appendix Figure 1.A1a shows that for visits to the grocery or pharmacy, mobility declines as well, but not as much, and that there is logically less of a trust and compliance issue so that there is no observable difference between trust groups. Results for visits to the park or other outdoor places are more ambiguous.

A similar pattern is found in a majority of cases when looking at each country separately (see Figure 1.A2 in the appendix).¹³ It is also confirmed when using alternative measures of appreciation of the political system, including the ESS question on satisfaction with the work of the national government (Figure 1.A3) and the question on trust in the national government from the Eurobarometer (Figure 1.A4).

¹³What the comparison between Figures 1.2 and 1.A2 reveals is that regional variation within a country does not necessarily provides enough power to detect the effect under study: our main, global effect is based on the variation across regions within but also between countries.

Table 1.1: Effect of Trust on Mobility

	Binary trust, basic DD (using average regional mobility before and after lockdown)		Binary trust, panel DD (using daily regional mobility)				Continuous trust, panel DD (using daily regional mobility)			
	(i)	(ii)	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
Post x Trust	-5.766** (2.338)	-5.083** (2.231)	-5.570*** (2.082)	-4.940** (1.966)	-5.560*** (2.080)	-4.925** (1.963)	-19.973** (8.863)	-18.753** (9.062)	-19.910** (8.854)	-18.671** (9.050)
# daily deaths (t-1)	-0.067*** (0.003)	-0.063*** (0.003)	-0.238*** (0.007)	-0.239*** (0.007)	-0.238*** (0.007)	-0.239*** (0.007)	-0.240*** (0.007)	-0.241*** (0.007)	-0.240*** (0.007)	-0.241*** (0.007)
Observations	440	440	7,899	7,899	7,899	7,899	7,899	7,899	7,899	7,899
R-squared	0.840	0.845	0.888	0.891	0.897	0.900	0.888	0.891	0.896	0.899
Country FE	-	-	Yes	Yes	No	No	Yes	Yes	No	No
Region FE	-	-	No	No	Yes	Yes	No	No	Yes	Yes
Region reweighting	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Elasticities with respect to (continuous) trust										
Mobility							-0.219	-0.206	-0.218	-0.205
Death growth rate							-0.263	-0.231	-0.262	-0.230

Note: authors' difference-in-difference (DD) estimation of Google mobility index (retail and recreation) on trust data (ESS) using regional variation for the period from March 1 to April 5, using either binary trust (1 if regional trust measure above international median, 0 otherwise) or continuous trust (regional trust measure, calculated as the proportion of people with trust scores above national average). *Post* is a dummy indicating the average lockdown date (mid-March 2020). The first column reports DD estimates using only average regional information before and after the lockdown time cutoff. Columns A to H are based on estimations using daily regional information. Estimations include the lagged daily number of COVID-19 fatalities (cf. European Centre for Disease Prevention and Control), day dummies and regional control variables (unemployment and population density). As indicated, they also account for country fixed effect (in this case, we include both regional controls and their interaction with *Post*) or region fixed effect (in this case, we include only the interaction term). Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Robust standard errors in parentheses, cluster-bootstrapped at region level (1000 replications). Significance level: *** p<0.01, ** p<0.05, * p<0.1

Difference-in-difference estimations. We then proceed with difference-in-difference (DD) estimations, using regional data for the period March 1-April 5. The treatment variable is the regional trust level, denoted $Trust_i$ for region i and constructed as a binary or continuous measure as previously discussed. The treatment period is defined as $Post = 1(date > March15)$. National lockdown announcements have taken place in a narrow time window around March 15, as previously seen in Figures 1.1a and 1.1b.¹⁴ In a classic DD specification ignoring the panel dimension, we regress the mobility of region i at day t as follows:

$$Mobility_{it} = \alpha^T + \beta^T Post \times Trust_i + \rho^T Trust_i + \gamma^T Death_{it-1} + \delta^T X_i + \eta^T Post \times X_i + \theta_t^T + \mu_c^T + \varepsilon_{it} \quad (1.1)$$

The coefficient β^T is the double difference estimator while ρ^T represents the long-lasting differences (constant selection bias) between regions.¹⁵ We control for day dummies θ_t^T , which capture common time trends (for instance the information available to all European citizens on the pandemic situation at any point in time) and absorb $Post$. We also include country dummies μ_c^T , which account among other things for national differences in the overall contagion

¹⁴Note that our conclusions are unchanged if we rather adopt alternative cutoffs, for instance the pandemic WHO declaration period or country-specific lockdown dates. The latter are the times of official lockdown enactment, when available, or the date of national lockdown recommendation (for Finland, Sweden, Netherlands, Hungary), as reported at: www.bbc.com/news/world-52103747. Below, we show time-heterogeneous effects and indicate that the trust effect becomes significant as soon as the second week of March.

¹⁵The parallel trend is verified informally by visual inspections of Figure 1.2 for the late February-early March period. Formal tests confirm it using placebo regressions carried out over the whole sample of regions or for each country separately.

level (e.g. an early start in Italy), for different national healthcare systems or for long-term trends in political trust at the country level (along other cultural differences). We add the number of people deceased from COVID-19 on the previous day, $Death_{it-1}$, which reflects the degree of exposure and the urgency to comply with containment measures.¹⁶ Finally, omitted variables may affect both trust and mobility. For instance, if citizens living in rural areas feel less threatened (and hence comply less) and are traditionally more in confidence with the political system, then our effect would be downward biased. To attenuate this concern, we introduce a vector X_i of local factors comprising the regional urban density (as per our example) and the regional unemployment rate (which mechanically impacts on work-related mobility and may also correlate with trust), as well as their interaction with $Post$. In a second specification, we acknowledge the panel nature of our sample of regions and replace μ_c^T by region fixed effects μ_i^T :

$$Mobility_{it} = \alpha^T + \beta^T Post \times Trust_i + \gamma^T Death_{it-1} + \delta^T X_i + \eta^T Post \times X_i + \theta_t^T + \mu_i^T + \varepsilon_{it} \quad (1.2)$$

Time-invariant characteristics of regions, including $Trust_i$ and X_i , are absorbed by these regional effects while $Post \times X_i$ remains in the model. Region fixed effects now capture all the local characteristics that may explain long-lasting differences in both mobility and trust (beyond the mere economic and urban density aspects that we controlled for in the first model). Note that for both models, standard errors are cluster-bootstrapped at regional level to account for multiple observations of each region in the daily panel (1000 replications).

Results are reported in Table 1.1 for the “retail/recreation” mobility index. All models convey that the mobility of citizens living in high trust regions decreases more than in other regions ($\beta^T < 0$), which we interpret as a higher compliance with national policies encouraging self-isolation. Let us start with the *binary* trust measure. The first two columns (i and ii) present a basic DD estimation using the average regional mobility before and after the time cutoff. We see that high-trust regions decrease mobility more than low-trust region by around 5-6 points on the 100-mobility scale: this is close to what visual inspections of the main graphs convey (cf. Figure 1.2). Remark that in column (i), we are pooling regional information from many European countries while the number of regions varies by country. To avoid giving more weight to a country with numerous regions, a variant is suggested in column (ii) whereby each observation is reweighted by the inverse of the number of regions in the corresponding country. The trust effect is very similar in this case.

In the next two columns of Table 1.1, we move to the DD estimations using the whole data. Columns A and B report the results of equation 1.1, without and with reweighting respectively. Columns C and D show the panel DD estimates from equation 1.2, also without and with reweighting. In all cases, we confirm that high-trust regions decrease their mobility significantly

¹⁶Mortality figures are at country level. Data at regional level are not systematically available for all the countries. In alternative unreported estimations, we control for the intensity of Google search for “COVID+death” at regional level to proxy the local intensity of concern regarding the risks associated with the pandemic. Our main estimates are barely changed.

more than low-trust regions. The magnitude is similar to the basic DD estimates, with a trust effect around 4.9-5.6 on the mobility scale. This effect appears fairly large if compared to the average drop in (recreational) mobility of around 35 on the 100-scale during lockdown: it means that high-trust regions have decreased their mobility by 14%-16% more compared to low-trust regions.¹⁷

The last four columns present results based on the *continuous* measure of trust. We show estimates based on model (1) (columns E-F) and model (2) (columns G-H) while checking the role of reweighing. Results are consistent and indicate that a 0 to 1 variation in the trust measure (i.e. the proportion of people reporting above national average trust) leads to a reduction of around 18.6-20 on the 100-scale of mobility. An interesting benchmark is a standard deviation in trust (0.10), which represents around a quarter of the average trust level in Europe (.41).¹⁸ Increasing trust by a standard deviation leads to an extra reduction in mobility of 1.9-2, which represents between 5.3% and 5.7% the average change in mobility after lockdown. We can also derive mobility elasticities with respect to trust, which will be used in the last section to evaluate the impact of trust on the COVID growth rate. The elasticity is calculated as the change in mobility (relative to the mean mobility since March 1) for a 100% increase in mean trust. Reported in the penultimate row of Table 1.1, the elasticities range from -.19 to -.22 across specifications. With this definition, a one standard-deviation increase in regional trust leads to a 5%-5.3% decrease in mobility. While these instantaneous elasticities may seem modest, they can lead to substantial differences after several weeks because of the exponential process of virus diffusion, as illustrated in the last section.

The above results are obtained for mobility related to recreational activities. We replicate the main estimations – namely the panel DD using regional fixed effects – for the other mobility indices. Results in Table 1.A1 of the appendix convey very similar conclusions: the drop in mobility associated to other non-necessary activities (work and transport) is significantly larger in high-trust regions. The summary measure indicating an increase in time spent at home during lockdown also shows a significantly higher shift for these regions. In contrast, as expected, the trust effect regarding essential activities (visits to the grocery or pharmacy) is not statistically different from zero. Elasticities are of a similar order of magnitude as what we found for recreational activities (between .16 and .27 in absolute value) and the mobility effect for a standard deviation in trust ranges between 4% and 6.5% (in absolute value).

While these estimates capture the average effect of trust on mobility during lockdown, it is also possible to capture time-heterogeneous effects. Time variation is already visible by comparing the high and low trust curves of Figure 1.2 during lockdown, relative to their pre-lockdown trends. To elicit time patterns more precisely, we estimate a version of equation 1.2 where

¹⁷Other, more minor results are in line with intuition. In particular, the number of COVID-related deaths on the previous day is significantly associated with reduced mobility.

¹⁸Such a standard deviation across European regions is not overrepresenting trust variation within countries. Indeed, the within-country standard deviation is often large. It varies between .05 and .20 across countries, which is between 10% and 50% the national mean trust level. Note also that similar dispersions are obtained using regional trust scores rather than the share of citizens above country mean.

$Post \times Trust_i$ is replaced by interactions between trust and 5-day period dummies. Time-varying effects are interpreted relatively to the first period (March 1-5). These estimates and confidence intervals are reported in Figure 1.A5 for both binary and continuous trust. They confirm that the drop in mobility was more pronounced in high-trust regions and additionally show that the divergence between regions appeared during March 16-20 and became statistically significant during March 21-25. Note that in both these results and Figure 1.2, global estimates of the trust effect are derived at each point in time and, hence, should be less affected by region autocorrelation in the daily panel. This is illustrated in Figure 1.A5 where estimates depend very little on whether standard errors are clustered at region level.

1.3.2 Policy Stringency and Trust

Graphical evidence. We now explore a more time-continuous variation in the intensity of lockdown policies using daily stringency measures at country level. We start with graphical evidence. Figure 1.3 reports the negative relationship between mobility and policy stringency, derived from time and regional variation in Europe (as represented by 95% CI). It suggests that for all non-essential activities, stricter lockdown regulations have contributed to drastically reduce human movements and, hopefully, to limit contagion. In high-trust regions, the mobility trends are shifted downward by a significant margin while, symmetrical, time at home (last graph) is shifted upward. The role of trust is nonlinear: the gap between trust groups increase with the stringency degree.¹⁹ Finally, these patterns are not so pronounced for necessary activities (see Figure 1.A1b in the online Appendix), even though policy-defying attitudes by low-trust regions are detected also for these activities at very high stringency levels (especially for visits to the park, which are more restricted than grocery/drugstore visits in some countries).

Empirical approach and results. The double difference approach used pre and post-lockdown time variation and assumed an average policy pressure. We now exploit a time-continuous change in policies using the daily index of stringency, which also captures country heterogeneity in the strictness of lockdown measures across Europe. Estimations are carried out as before on daily regional mobility from March 1 to April 5 and using the same control variables. Different specifications are written as:

$$Mobility_{it} = \alpha^S + Z + \gamma^S Death_{it-1} + \delta^S X_i + \theta_t^S + \mu_c^S + \varepsilon_{it}$$

$$\text{with } Z = \beta_0^S Stringency_{it} \tag{1.3}$$

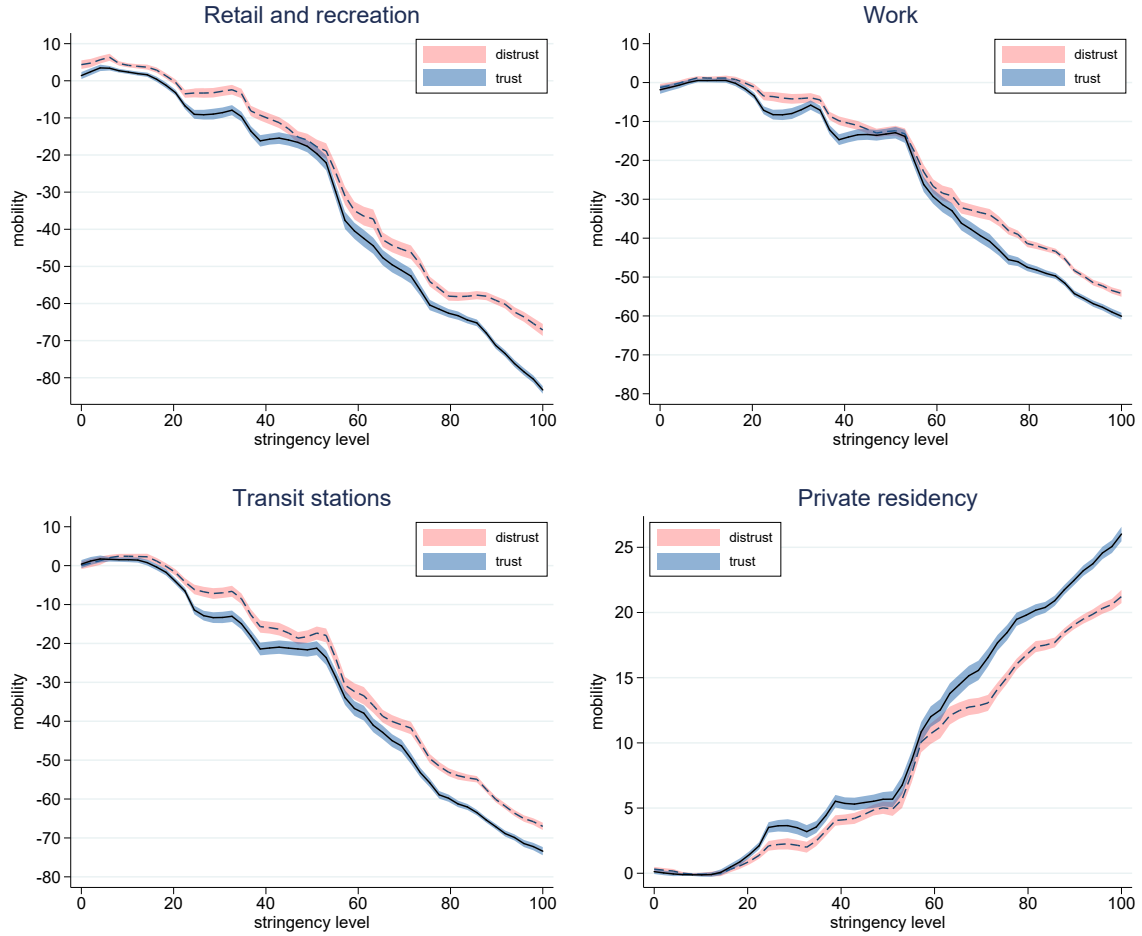
$$Z = (\beta_0^S + \beta_1^S Trust_i) Stringency_{it} + \beta_2^S Trust_i \tag{1.4}$$

$$Z = (\beta_0^S + \beta_1^S Trust_i) Stringency_{it} + \beta_2^S Trust_i \tag{1.5}$$

$$\text{with } \beta_{1i}^S = \beta_1^{HS} HighStringency_{it} + \beta_1^{LS} LowStringency_{it}.$$

¹⁹The recent literature on compliance disentangles the role of trust (which increases voluntary compliance) and that of power (which increases enforced compliance), while noting that they are not necessarily complementary (Batrancea et al., 2019).

Figure 1.3: Daily Mobility, Lockdown Stringency and Trust around March 2020



Source: authors' calculations based on Google mobility data, stringency level from Oxford COVID-19 Government Response Tracker (OxCGRT), and ESS data on trust in politicians. Areas represent the 95% CI of average daily mobility across European regions, weighted by $1/\text{number of regions in the corresponding country}$. Distrust indicates regions, within each country, with trust level below country average.

In all models, standard errors are cluster-bootstrapped at regional level. The first model, in equation 1.3, simply aims to gauge the average effect of stringency. Results are presented in Table 1.2. As expected, higher stringency is associated with less mobility (column a) and this result is not sensitive to region reweighting (column b).

Equation 1.4 captures how political trust may increase the stringency impact on mobility. Results in Table 1.2 go as follows. Trust significantly increases the diminishing effect of stringency (column c): high trust regions tend to comply more to policy stringency on average. This effect holds with region reweighting (column d). Replacing $Trust_i$ and country fixed effects μ_c^S by region fixed effects μ_i^S , the model leads to very similar results without or with reweighting (columns e and f respectively).

The elasticity of mobility with respect to trust, calculated around mean stringency and mean trust level, ranges between -.11 and -.13 across models (c)-(f).²⁰ We also replicate estimations

²⁰In the last specification, the average elasticity mediates around 70% of the direct effect of trust obtained by the corresponding DD model with region fixed effects and reweighting.

for all types of activities using the most complete model with region fixed effects. As can be seen in Table 1.A2, the mediating effect of trust on the efficacy of stringency is significant for the decrease in non-essential activities (recreation, work and transport), for the increase in time spent at home, but not for the shift in necessary activities (visits to grocery and pharmacy).

Finally, equation 1.5 aims to test the nonlinearity observed in Figure 1.3. In Table 1.2, specifications without or with region reweighting both convey that the impact of trust is larger at high stringency level (columns g and h respectively). Again, very similar results are obtained using region fixed effects (columns i and j). Equality tests reject the null with a p-value below 5% in models without reweighting. This result tend to confirm the increasing gap between high and low trust groups seen in Figure 1.3. We also formally test that there is no sign reversal at very low stringency levels. This could happen in situations where low-trust regions self-isolate more than the rest because they doubt the ability of the government to respond appropriately to the crisis.²¹

Potential limitations. A number of papers have studied the role of trust with respect to policy design and the degree of law-abidingness of the citizens (Algan and Cahuc, 2009). In our context, the endogeneity of policy stringency to the country level of political trust can be questioned. As a merely suggestive check, we regress stringency on trust and standard controls (unemployment, population density) at country level and find no effect of trust on stringency (p-value: 0.98). Most importantly, even if national policy stringency was endogenous to trust, our approach above relies primarily on region-time variation in trust (models c-d), with country fixed effect controlling for differences in overall levels of stringency and trust across countries, or just on time variation within regions (models e-f). Another potential limitation is the fact that stringency is measured at national level. Given the emergency, lockdown policies have been implemented nationwide in most countries, even in federal states such as Austria, Belgium or Germany. Stringency may however vary locally (e.g. severe restrictions in Bavaria). Further work could explore regional policy measures but more disaggregated trust data would be required for identification. Finally, endogeneous policy stringency may increase with the number of positive known cases on the days before, which also decreases individuals' mobility (fear factor). We have replicating our estimations using the number of positive cases rather than the number of death cases in t-1 but results were hardly changed.

²¹This interpretation is actually related to very recent studies on political orientation in the US, showing that Democrats tend not to follow the President's directive and exert more social distancing than Republicans (e.g. Allcott et al., 2020 or Painter and Qiu, 2020).

Table 1.2: Effect of Stringency and Trust on Mobility

	Effect of policy stringency on mobility		Mediating effect of continuous trust on the stringency effect				Mediating effect of continuous trust on the stringency effect, by stringency level			
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(j)
Stringency	-0.730*** (0.0344)	-0.715*** (0.0361)	-0.642*** (0.0543)	-0.620*** (0.0581)	-0.632*** (0.0552)	-0.610*** (0.0596)	-0.629*** (0.0520)	-0.613*** (0.0557)	-0.616*** (0.0527)	-0.600*** (0.0571)
Stringency x trust			-0.164** (0.0765)	-0.174** (0.0862)	-0.184** (0.0821)	-0.195** (0.0931)				
Stringency (high) x trust							-0.162** (0.0773)	-0.174** (0.0865)	-0.187** (0.0828)	-0.198** (0.0934)
Stringency (low) x trust							-0.0833 (0.0914)	-0.127 (0.100)	-0.0993 (0.0952)	-0.145 (0.105)
Observations	7,649	7,649	7,649	7,649	7,649	7,649	7,649	7,649	7,649	7,649
R-squared	0.920	0.920	0.928	0.927	0.936	0.936	0.928	0.927	0.936	0.936
Country FE	Yes	Yes	Yes	Yes	No	No	Yes	Yes	No	No
Region FE	No	No	No	No	Yes	Yes	No	No	Yes	Yes
Region reweighting	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Elasticities with respect to (continuous) trust:										
Mobility			-0.112	-0.119	-0.125	-0.133				
Death growth rate			-0.134	-0.142	-0.150	-0.159				

Note: authors' estimation of Google mobility index (retail and recreation) on stringency index (Oxford COVID-19 Government Response Tracker) and trust data (ESS) for the period from March 1 to April 5, 2020. Estimations include the lagged daily number of COVID-19 fatalities (cf. European Centre for Disease Prevention and Control), day dummies and regional control variables (unemployment and population density). As indicated, they also account for country fixed effect (in this case, we include both regional controls and their interaction with stringency) or region fixed effect (in this case, we include only the interaction term). Region reweighting: observations are weighted by $(1/\#)$ of regions in the corresponding country). Robust standard errors in parentheses, cluster-bootstrapped at region level (1000 replications). Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

1.3.3 Trust and COVID-19 Death Growth Rate

We provide suggestive evidence on how trust translates into a slower epidemic growth through mobility reduction. Clearly, it is not possible to find a relationship between current mobility and future deaths, as both are highly correlated with the current mortality level. However, it is possible to establish how the upcoming death growth rate responds to the instantaneous mobility index, reflecting the efficiency of lockdown policies. Note that other factors are excluded (in March-April, none of the European countries had reached a level of infection leading to collective immunization). Our calculations are purely indicative given the medical uncertainty on key parameters.

Using international data, [Soucy et al. \(2020\)](#) also point to an impact of the reduction in human mobility on the infection growth rate. They find that a 10% decrease in relative mobility in the second week of March was associated with a 11.8% relative decrease in the average daily death growth rate in the fourth week of March, i.e. an elasticity of 1.18. We obtain similar results when focusing on Europe. We also suggest an alternative calculation based on daily mobility data throughout March and until April 5, fully exploiting the variation in containment policies over time and across countries. For each day, we compare the current cumulated death toll attributed to COVID-19 to that of 2 weeks ahead, and divide the corresponding growth rate by 14 to obtain a daily upcoming death growth rate. This growth rate is regressed on the instantaneous mobility index, day fixed effects and country fixed effects.

We find a significant estimate of .021 (std. err. of .0016). It yields an elasticity of death growth rate with respect to mobility of 1.20, which is very similar to [Soucy et al. \(2020\)](#). We combine it with our previous estimates to compute an elasticity of death growth rate with respect to

trust, systematically reported in the last row of all the previous tables. Take for instance the DD approach with region fixed effects as baseline model. For recreational mobility, we find an elasticity of .218 in this case, i.e. doubling trust would lead to a 21.8% decrease in the mortality growth rate. This corresponds to a decrease from 39.1% to 28.9% in the median daily death growth rate, i.e. a doubling in the number of deaths in 3.5 days rather than 2.6 days. To get a notion of how it translates in terms of death toll, note that there was a total of 2,000 cumulated deaths mid-March in Europe and around 90,000 by mid-April (ECDC figures). Consider a benchmark variation of +25% in trust (1 standard deviation): with the baseline model, this leads to a 6.5% decrease in the mortality growth rate and around 10,000 less deaths by April 15. Robustness checks confirm these orders of magnitude.²²

1.4 Conclusion

Trust in governments is an important determinant of citizens' compliance with public health policies, especially in times of crisis. This relationship, rarely studied in the literature, deserves a particular attention in the present context of global pandemic. COVID-19 has forced governments to take drastic measures all over the world. Lockdown policies are often very constraining and must receive a large support by the population to be efficient – this support is not guaranteed and certainly not homogenous. Using mobility data at regional level in Europe, we show that higher political trust is associated with a larger reduction in non-essential mobility following the implementation of containment policies in March 2020. This effect is interpreted as a higher level of compliance to national directives in high-trust regions. It coincides in magnitude with the effect of trust on the efficacy of policy stringency.

Persistent differences in regional attitudes towards national policy makers are important and should be taken into account by authorities for policy design and especially for the implementation of nationwide emergency policies. This is relevant in the present context for both the enforcement of lockdown policies and the necessary roll back of these measures at the time we write these lines. Notice that regional diversity captures only one dimension of the heterogeneity in civic values within countries. Further research should attempt to exploit more local or individual data on mobility and compliance to health policies such as social distancing measures.²³ The fact that variation in trust at a broad regional level already yields significant differences in mobility responses to recent health policies is striking. New research could go further to identify relevant social groups and connect this issue with the work on conflicts. Recent episodes of social unrest (e.g. the yellow jackets in France) point to groups that show more socio-economic vulnerability and less adherence to the political system (Algan et al., 2019).

²²A two-week lag for the death growth rate calculation is the average known duration between infection and public report. Results are similar when using 1 or 3 weeks. Robustness checks also include taking out countries with less than 100 cumulated deaths at the end of the period.

²³The conclusion of the present paper is corroborated by a survey on Danish citizen showing that the 'willingness to distance' depends on political trust, among other determinants (Olsen and Hjorth, 2020).

The present paper also relies on policy stringency. Much remains to be known about the causes and consequences of the great diversity of national policy responses to the pandemic. All the more so as many governments will be accountable to their population regarding the management of this crisis and the chosen trade-off between death toll, economic downturn and other consequences of the lockdown in terms of health and mental health.

Appendix

Table 1.A1: Effect of Trust on Alternative Mobility Measures

Panel Difference-in-Difference estimates of Post x Trust	Retail and recreation	Work	Transit stations	Grocery stores and pharmacies	Private residence
	(i)	(ii)	(iii)	(iv)	(v)
Binary trust	-5.560*** (2.080)	-3.820** (1.737)	-3.387* (2.037)	-2.628 (1.724)	1.764** (0.752)
R-squared	0.897	0.917	0.928	0.840	0.921
Continuous trust	-19.910** (8.854)	-12.980* (7.086)	-14.429* (8.707)	-10.164 (7.639)	7.271** (3.002)
R-squared	0.896	0.916	0.928	0.839	0.921
Observations	7,899	7,899	7,899	7,899	7,899
Mean mobility index	-37.4	-27.6	-36.2		11.1
Elasticities with respect to (continuous) trust					
Elasticity mobility	-0.218	-0.193	-0.163		0.268
Elasticity death growth	-0.262	-0.231	-0.196		0.322

Note: authors' difference-in-difference (DD) estimation of Google mobility index (for different types of activity as indicated) or index of time spent in private residence on trust data (ESS) using daily regional variation for the period from March 1 to April 5, with either binary trust (1 if regional trust measure above international median, 0 otherwise) or continuous trust (regional trust measure, calculated as the proportion of people with trust scores above national average). We report the coefficient on Post x Trust, with Post a dummy indicating the average lockdown date (mid-March 2020). Estimations include the lagged daily number of COVID-19 fatalities (cf. European Centre for Disease Prevention and Control), day dummies, region fixed effects and Post interacted with regional control variables (unemployment and population density). Robust standard errors in parentheses, cluster-bootstrapped at region level (1000 replications). Significance level: *** p<0.01, ** p<0.05, * p<0.1.

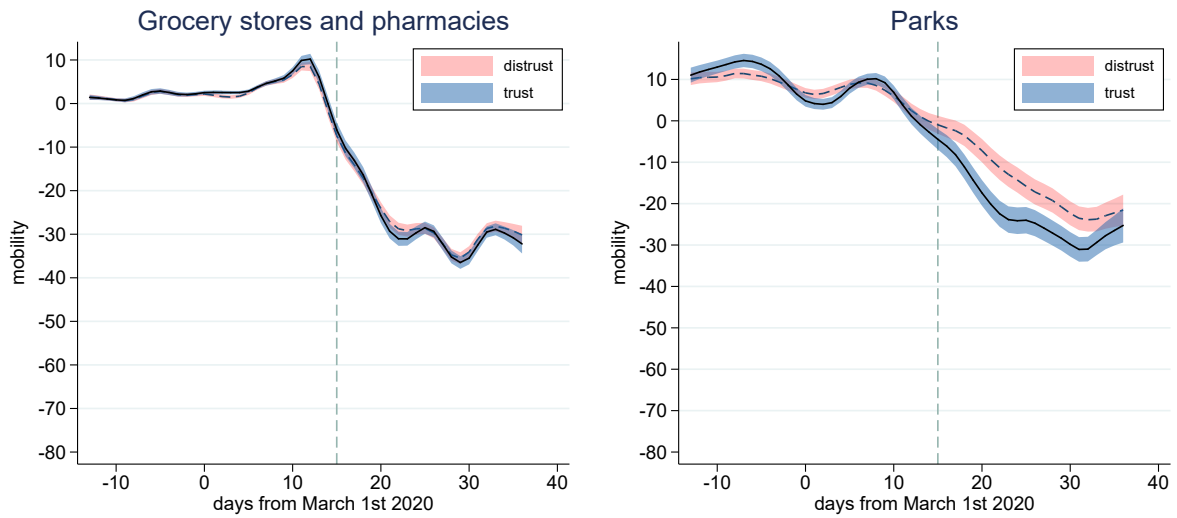
Table 1.A2: Effect of Stringency and Trust on Alternative Mobility Measures

	Retail and recreation	Work	Transit stations	Grocery stores and pharmacies	Private residence
	(i)	(ii)	(iii)	(iv)	(v)
Stringency	-0.632*** (0.0552)	-0.338*** (0.042)	-0.396*** (0.047)	-0.483*** (0.041)	0.131*** (0.018)
Stringency x trust	-0.184** (0.0821)	-0.113** (0.057)	-0.126* (0.075)	-0.060 (0.072)	0.056** (0.026)
Observations	7,649	7,649	7,649	7,649	7,649
R-squared	0.936	0.918	0.936	0.832	0.925
Mean mobility index	-37.4	-27.6	-36.2		11.1
Elasticity mobility/trust	-0.124	-0.103	-0.088		0.127
Elasticity death growth/trust	-0.149	-0.124	-0.105		0.153

Note: authors' estimations of Google mobility index (for different types of activity as indicated) or index of time spent in private residence on stringency index (Oxford COVID-19 Government Response Tracker) and trust data (ESS) for the period from March 1 to April 5, 2020. Estimations include the lagged daily number of COVID-19 fatalities (cf. European Centre for Disease Prevention and Control), day dummies, region fixed effects and stringency interacted with regional control variables (unemployment and population density). Robust standard errors in parentheses, cluster-bootstrapped at region level (1000 replications). Significance level: *** p<0.01, ** p<0.05, * p<0.1.

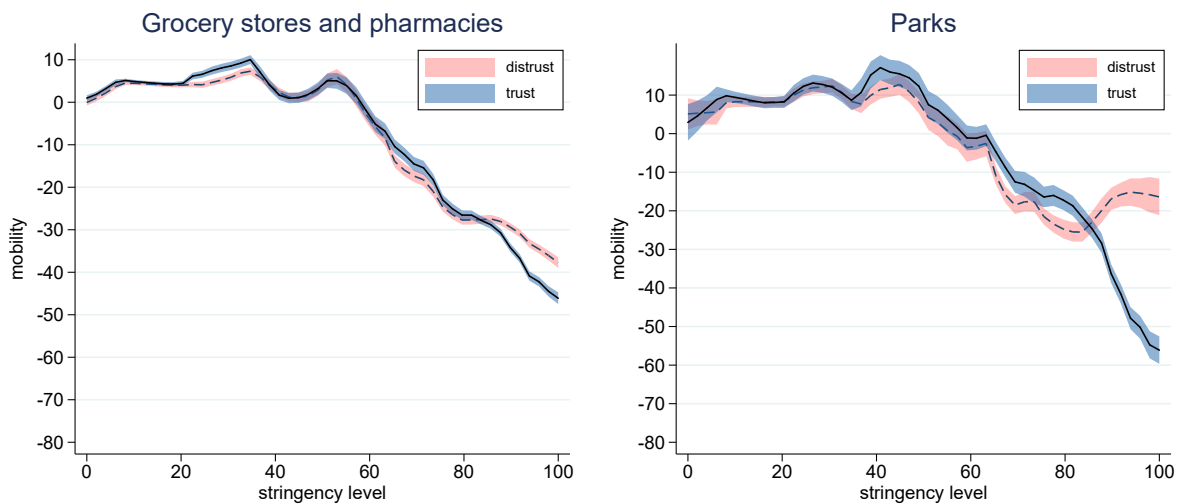
Figure 1.A1: Daily Mobility, Lockdown Stringency and Trust (Necessary Activities).

(a) Mobility Trends.



Source: authors' calculations based on Google mobility data and ESS data on trust in politicians. Areas represent the 95% CI of average daily mobility across European regions, weighted by 1/number of regions in the corresponding country. Distrust indicates regions, within each country, with trust level below country average.

(b) Stringency Trends.



Source: authors' calculations based on Google mobility data, stringency level from Oxford COVID-19 Government Response Tracker (OxCGRT), and ESS data on trust in politicians. Areas represent the 95% CI of average daily mobility across European regions, weighted by 1/number of regions in the corresponding country. Distrust indicates regions, within each country, with trust level below country average.

Figure 1.A2: Daily Mobility (Retail and Recreational) and Trust within Countries.

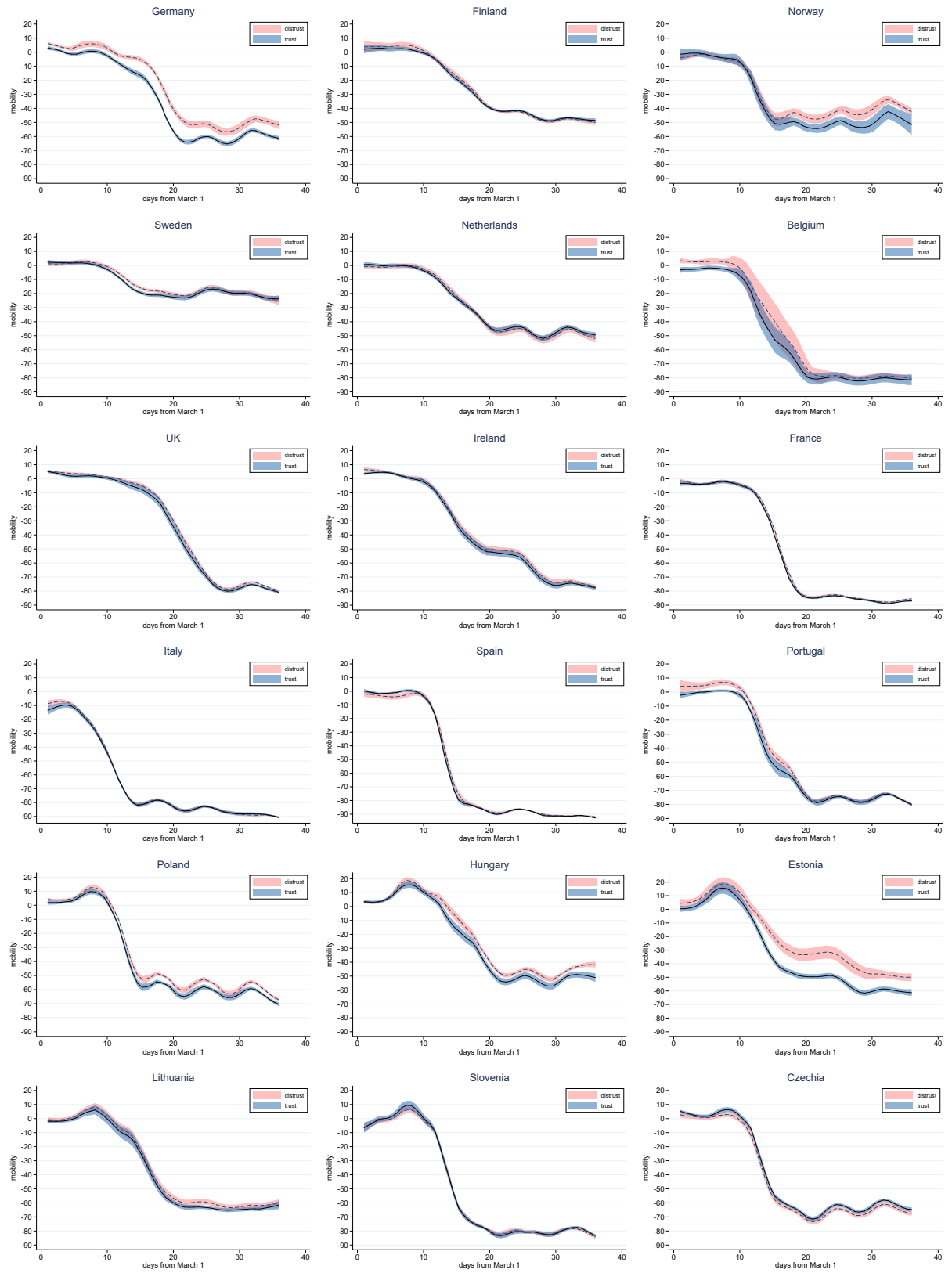
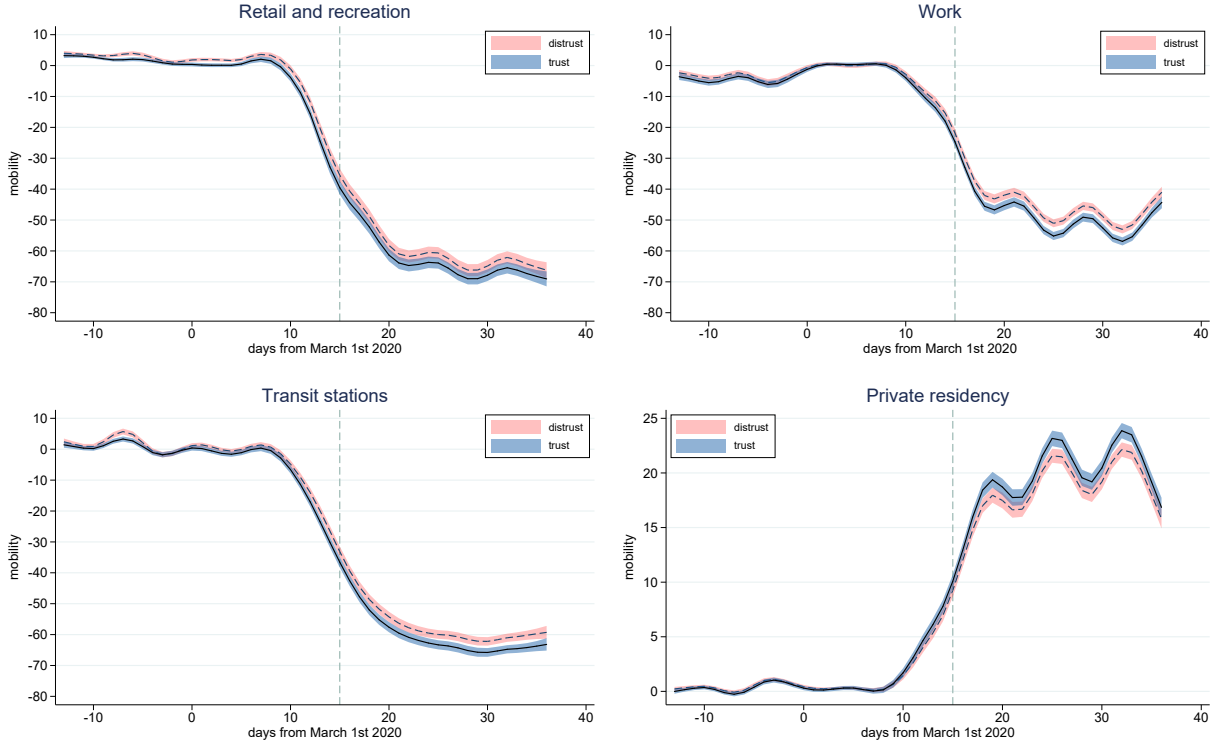
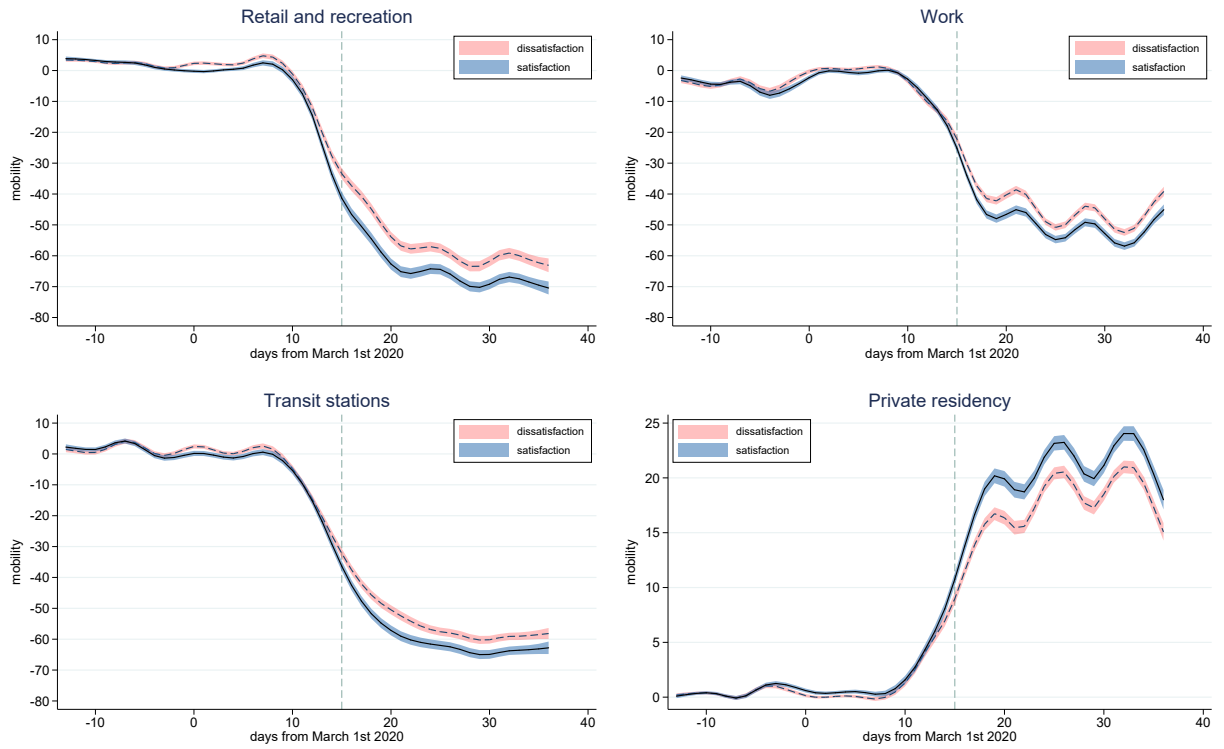


Figure 1.A3: Daily Mobility and Political Trust (Eurobarometer):
Variation across European Regions (local polynomial fit)



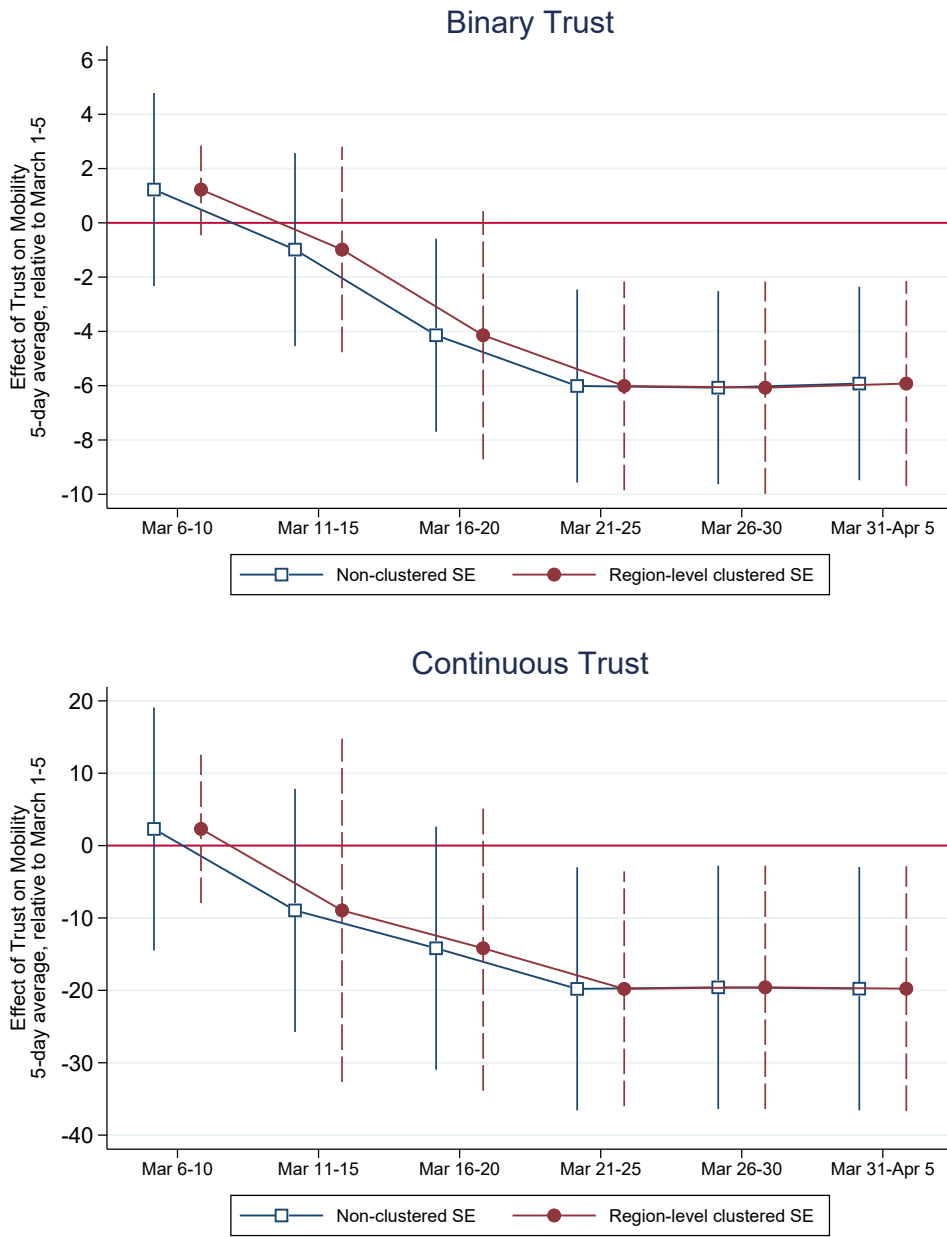
Source: authors' calculations based on Google mobility data and EUROBAROMETER data on trust in government. Areas represent the 95% CI of average daily mobility across European regions, weighted by 1/number of regions in the corresponding country. Distrust indicates regions, within each country, with trust level below country average.

Figure 1.A4: Daily Mobility and Satisfaction in Governments (ESS):
Variation across European Regions (local polynomial fit)



Source: authors' calculations based on Google mobility data and ESS data on satisfaction with government. Areas represent the 95% CI of average daily mobility across European regions, weighted by 1/number of regions in the corresponding country. Dissatisfaction indicates regions, within each country, with satisfaction level below country average.

Figure 1.A5: Time-heterogeneous Effects of Trust on Mobility



Source: author's estimations of 5-day average Google mobility index (retail and recreation) on trust data (ESS) for the period from March 1 to April 5, 2020. Estimations include lagged number of COVID-fatalities (cf. European Centre for Disease Prevention and Control) and 5-day period dummies. All estimations account for regional fixed effects. Confidence intervals are calculated using robust or region-level clustered standard errors.

Chapter 2

Gimme Shelter. Poverty, Social Distancing and Income Support in Times of Pandemic*

This chapter is based on a joint work with Olivier Bargain and Tanguy Bernard and published in European Economic Review.

2.1 Introduction

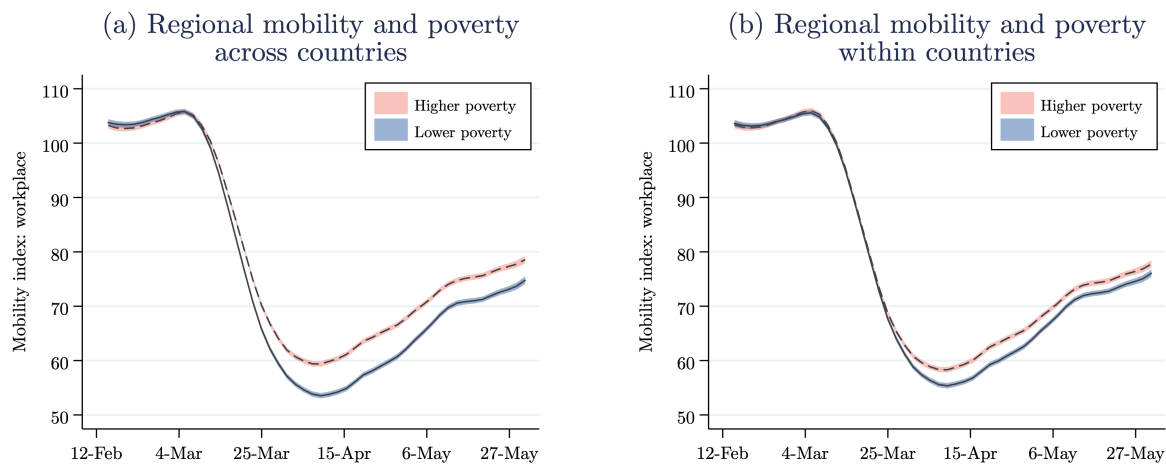
The COVID-19 pandemic has triggered unprecedented deployment of anti-contagion policies throughout the world, among which mobility restrictions and income support programs feature prominently. In terms of reducing mobility, by mid-April 2020, most countries had implemented stay-at-home orders (SHO) that greatly contributed to curbing the spread of the pandemic (see for example [Chinazzi et al., 2020](#); [Hsiang et al., 2020](#); [Aubert and Augeraud-Véron, 2021](#)).¹ However, these measures also triggered a major global economic crisis that resulted in a sharp increase in poverty (e.g. [Gutierrez-Romero and Ahamed, 2021](#); [Decerf et al., 2020](#); [Sumner et al., 2020](#); [Egger et al., 2021](#)). A critical aspect of containment policies is the fact that the poorest could not afford to stay home. Existing evidence focuses on geographically-specific cases, including Ghana, South Africa and Chile ([Durizzo et al., 2021](#); [Carlitz and Makhura, 2020](#); [Bennett, 2021](#)), or from regional variation within a small set of countries ([Bargain and Aminjonov, 2021](#)). These studies suggest that adherence to containment policies is limited amongst poor households, who typically face acute food shortage and must seek out income generating activities on a daily basis. Figure 2.1 illustrates that after lockdowns started around mid-March, the reduction in work-related mobility were smaller in poorer regions than in less

*This study has received financial support from the French State in the framework of the Investments for the Future programme IdEx Université de Bordeaux / GPR HOPE.

¹See <https://ourworldindata.org/> (notably ‘stay at home’ and ‘school closure’ graphs) for visual tracking of policy implementation over time.

poor regions, whether the relative poverty is defined across countries (a) or within countries (b). Thus, poverty is not only increased by the epidemic but can also contribute to fuel its growth.²

Figure 2.1: Work-related Mobility by Regional Poverty Levels



Source: author’s calculations based on Google mobility data (mobility for workplace) and poverty data from national statistics offices and authors’ estimations using household surveys. Local polynomial fit with 95% CI of daily mobility across districts, weighted by $(1/\#)$ of districts in the corresponding country). Poverty is measured as the share of people living below national or international poverty lines in a subnational district. In graph (a), poverty is defined as lower (higher) if district’s poverty rate is below (above) global median district-level poverty rate. In graph (b), poverty is defined as lower (higher) if district’s poverty rate is below (above) country’s median district-level poverty rate.

In this context, a new role has emerged for the other side of policy action: redistributive programs. Along with mobility restrictions, governments around the world have engaged in supplementary income support (IS) policies, expanding existing social transfers and/or setting up new ones. According to [Gentilini et al. \(2020\)](#), at least \$800 billion were invested in social protection in 2020 (around 1% of global GDP), amounting to more than 1,400 measures, of which about one-third took the form of cash transfers, reaching over 1.1 billion people. Initially motivated as a means of preserving livelihoods and avoiding sharp increases in extreme poverty, IS programs may have also helped poor populations comply with public health rules and thus contain COVID-19. Some of these programs are explicitly labelled in this way, such as the ‘Bogotá Solidaria en Casa’ in Colombia, or the ‘Quédate en Casa’ in Dominican Republic. Yet, to date, there is no global evidence about the extent to which IS has helped reduced mobility and, in this way, generated positive health externalities for the poor.

In this article, we exploit regional information on poverty and on work-related mobility across 729 subnational regions of 43 low- and middle-income countries, mainly in Africa and Latin America as well as in few countries in the Middle East and Asia. Using time and spatial variation in both SHO and IS policies throughout the year 2020, we emphasize the presence of a poverty gap in mobility reduction during lockdowns and on the cushioning effect of IS policies, measuring the extent to which these programs have implicitly facilitated compliance

²This observation also applies to rich countries. For the US, [Papageorge et al. \(2021\)](#) show that people with lower income and an inability to tele-work were less likely to engage in behaviors that limit the spread of the disease.

with mobility restrictions and contributed to slow down the spread of COVID-19.

Our approach mobilizes several types of openly available data. First, we collect recent pre-pandemic estimates of poverty incidence at regional level using World Bank data or, when unavailable, from our own computation based on microdata. We classify individual regions as being of higher (lower) poverty incidence if their poverty headcount is above (below) the median of the country. Second, we merge this data with daily regional mobility estimates from Google COVID-19 Mobility Reports, over 202 days (Feb. 15–Sept. 3, 2020). Third, we rely on the Oxford COVID-19 Government Response Tracker ([OxCGRT, 2020](#)), which records daily changes in state interventions during the COVID-19 pandemic. For each subnational region in our sample, we assign national-level changes in policies related to SHO and IS programs. We further check the robustness of our findings using regional information on urban density, informality, trust in government and mobile internet access.

Our estimates rest on panel specifications in which changes in work mobility are related to indicators of SHO measures and IS policies. We control for country-time fixed effects to account for all possible time-varying confounders at country level, including country differences regarding pandemic trends, economic trends or how health policies daily interact with state capacities and population characteristics. While these country-day dummies also absorb the average effect of the daily policy mix (e.g. SHO with or without IS) in a country, we focus on the *heterogeneous* effect of these policies across higher- vs lower-poverty regions within countries. In particular, we seek to estimate how the poverty-related mobility differences across regions is mitigated when IS programs are activated. Because SHO and especially IS are introduced at different points in time in the different countries, and to avoid difficulties due to multiple treatments and multiple periods, we complement our average estimates of the poverty gap in mobility by an event study approach that characterizes daily changes in the mobility gap for both policy combinations (SHO and SHO+IS).

Results of these different approaches converge to show that all the regions have greatly reduced workplace mobility in response to SHO but the magnitude of the drop was significantly smaller in regions with higher incidence of poverty (i.e. 20% less than in other regions on average). We interpret this difference as a higher propensity to continue labor activities in poorer regions. This interpretation is also consistent with the fact that the poverty gap in mobility is not found for ‘essential’ trips, such as going to the grocery/pharmacy. Importantly, when IS is provided in response to the pandemic, mobility is further decreased but more so in poorer areas, such that the mobility gap between higher and lower poverty regions drops from 20% to 7%. Event study results confirm the average estimate and offer a precise characterization of the daily variation in poverty gaps and how they are attenuated by IS programs. Back-of-the-envelope calculations indicate that IS policies have likely contributed to a slower spread of the virus through by further reducing work-related mobility in poorer regions.

Beyond these results, we see several contributions from this research. First, adding to the

literature on social protection in low and middle income countries, we provide a novel argument in support to the use of IS, i.e. as being part of the short-term policy response to curb a pandemic. Over the past two decades, IS policies have taken a predominant role in these countries,³ many studies pointing to their ability to preserve a minimum standard of living for the poor, help protect their asset base in the face of a negative income shock and avoid long-term poverty traps (see the meta-analysis of [Hidrobo et al., 2018](#)). In comparison, fewer studies have investigated the effect of IS policies on aggregate welfare.⁴ To our knowledge, this study is the first to examine the effect of IS policies on aggregate health outcomes in low- and middle-income countries in the context of a global pandemic. With the number of human infectious diseases constantly on the rise since the 1950s ([Smith et al., 2014](#)), our results offer further support to policies geared at protecting the income of the poor at the time that SHO are implemented.

Second, this contribution provides the ‘big picture’ by quantifying a global average effect of the poverty gap and the compensating effect of IS: these global estimates are complement the few existing studies focusing on specific policies in local contexts (for instance the health effect of a randomized cash transfer in Kenya, analyzed by [Brooks et al., 2020](#); [Banerjee et al., 2020](#)). Further work should attempt to connect both levels of analysis, possibly by using more disaggregated mobility data and providing more heterogeneous effects across contexts and policy types.

2.2 Data

In this section, we describe the four main types of data used in the analysis as well as data treatment and selection.⁵

2.2.1 Human Mobility during COVID-19 Pandemic

We use human mobility data from the Google Mobility Reports. The reports record daily changes in the number of visits to – or length of stay at – various locations before and during the COVID-19 pandemic. They are based on aggregated and anonymized data from users’ (Android operated) mobile device location history. The locations are grouped in several categories including mobility to workplaces (our key variable of interest) and mobility to grocery and pharmacy (used in placebo checks). The Google measures take into account the fact that the person is not at home during these activities: this is a key aspect of our demonstration

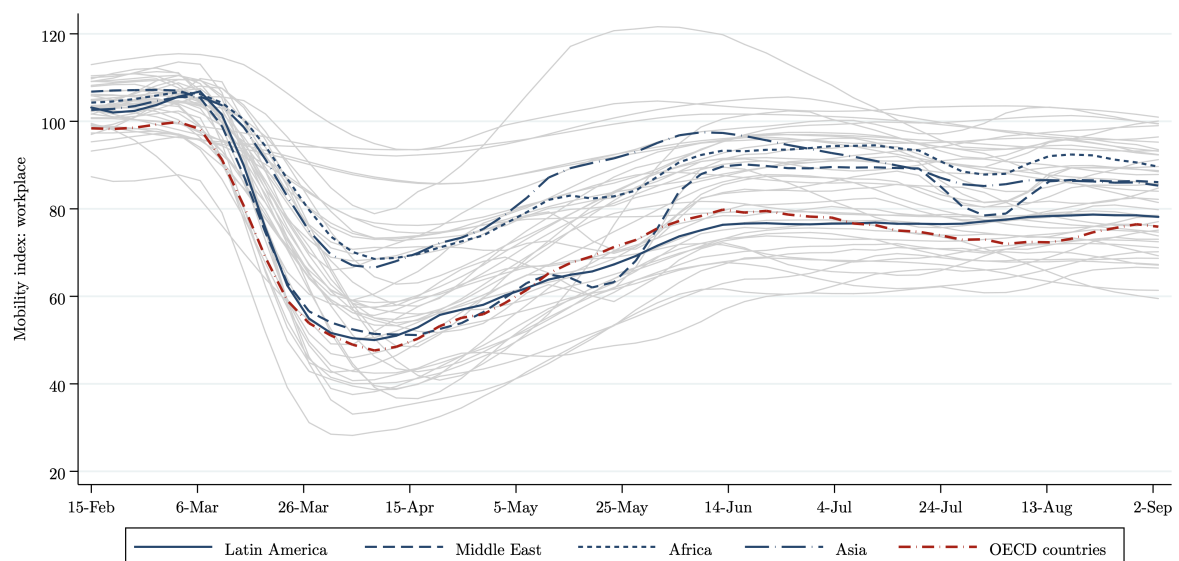
³In 2011, the UK Department for International Development estimated that social transfers in low- and middle-income countries reached between 0.75 and 1 billion people. As of 2017, cash transfer policies were on-going in 149 countries in the World ([World Bank, 2017](#)).

⁴The question is fundamental if one is to value the overall returns on investments in these policies. External effects were so far focused on economic outcomes: through reduced inequalities, increased social cohesion and enhanced human capital at the economy level, IS are shown to contribute to overall economic growth ([Alderman and Yemtsov, 2014](#)), generating positive spillovers ([Angelucci and De Giorgi, 2009](#); [Bobonis and Finan, 2009](#)) and large fiscal multipliers ([Egger et al., 2019](#)).

⁵A synthetic description of the variables used is provided in Table 2.A1 in appendix A.

regarding work-mobility, since it reveals a tension between health-related risks (i.e. leaving the home and being exposed to the virus) and income-related risks (i.e. the ability to generate livelihood, whatever the nature of the job: formal or informal, agricultural or not, etc.). For each type of mobility, daily measures are expressed as changes relative to the average level in a reference period of January 3 to February 6, 2020, normalized to 100 (see [Google, 2020](#) for more details). The data regularly tracks mobility across more than 130 countries since February 15, 2020, but we focus on a subset of low- and middle-income countries for which mobility data is available at the subnational level. As a result, our sample covers a panel of 729 subnational regions across 43 countries in Africa, Latin America, Middle East and Asia, observed for 202 days from February 15 to September 3, 2020.⁶

Figure 2.2: National Trends in Work-related Mobility.



Source: Google COVID-19 Mobility Reports.

Figure 2.2 illustrates the time trends of work-related mobility using national averages for all the countries in our sample (solid grey lines) as well as the summary trends for broad geographical groups of low- and middle-income countries (blue lines) and high-income countries (red line). The horizontal axis reflects our time coverage, from February 15 to September 2, 2020. The vertical axis represents the level of work mobility in reference to the value 100 for the reference period. For most of the countries, we observe limited changes in work-related mobility in February-early March 2020 (being close to the benchmark of 100 of January 6 - February 3), but a sharp drop in mobility in late March, which corresponds to the first round of physical distancing measures taken by most governments around the world in response to the rapid spread of COVID-19.

While the rates of change vary substantially across countries, it is striking to see that mobility restriction measures were implemented almost simultaneously at a global level and in particular

⁶The list of countries is reported in Figure 2.3 below.

in low- and middle-income countries.⁷ As for the disparities in mobility responses in our sample, it may be due to local policy stringency or different information about (and perception of) the risks, but also to other factors affecting behavioral responses such as poverty – our heterogeneity of interest in this paper. For instance, as depicted in Figure 2.2, the decline in work-related mobility was on average more pronounced in richer countries (OECD, Latin America or the Middle East) than in poorer regions of the world (Africa and Asia), which tends to give credit to our hypothesis, at least at the macro level. We follow this line of reasoning in a more disaggregated hereafter by exploiting differences in poverty at the regional level within countries.

2.2.2 Poverty

We measure poverty at the level of subnational regions using pre-pandemic poverty headcount ratios, i.e. the share of people in the region living below the poverty line. To cover as many countries as possible, we use the latest official poverty statistics, when provided at regional level, or our own poverty calculations based on recent household surveys. Regional poverty rates are calculated using per capita income or consumption and, for poverty lines, the standard World Bank international lines or national definitions based on the value of a basic bundle of goods.⁸ To make interpretation easier, we use a binary measure of poverty hereafter, i.e. a dummy indicating if the regional poverty headcount ratio is above (‘higher-poverty’) or below (‘lower-poverty’) the country-specific median.

2.2.3 Policy Information

Containment Policies. We exploit data from [OxCGRT \(2020\)](#), which records daily changes in national non-pharmaceutical interventions during the pandemic. Our main containment variable is a binary indicator of whether governments have enacted any SHO, either as requirement or recommendation, or not.⁹ In all the countries of our sample, SHO were imposed at some point during the period Feb. 15–Sept. 3, with an average duration of 155 days in total over the period. The daily variation in country-level policies is documented in Figure 2.3.

Income Support. Our key measure, also drawn from [OxCGRT \(2020\)](#), is an indicator tracking whether governments provided IS in form of direct in-cash/in-kind payments to those who lost their jobs or were not able to work due to the pandemic. Importantly, we focus only on ‘new’ transfers introduced in response to the COVID-19 pandemic: they may come as a completely new program, as increased benefits for current recipients (vertical expansion) or as an extension of existing programs towards new beneficiaries (horizontal expansion). In our estimation, we use a binary definition, i.e. whether any transfer was provided or not for a given country-day.¹⁰

⁷In low- and middle-income countries, the timing of implementation of the first SHO seems to be more influenced by the spread of COVID-19 in Europe and North America than to the actual local state of the epidemic.

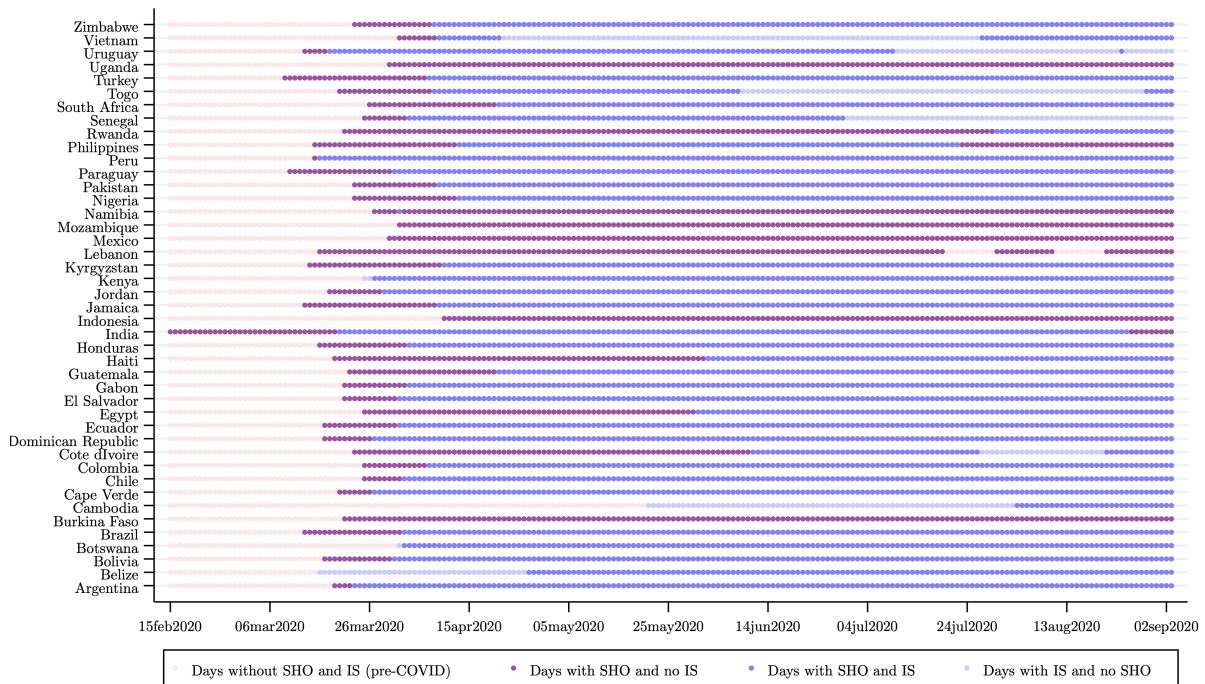
⁸A detailed description of sources of poverty data or household data used to calculate regional poverty is provided in Table 2.A2 in appendix A.

⁹We discuss additional approaches using more detailed information on degrees of SHO stringency in appendix C.

¹⁰We present additional approaches using more detailed IS levels in appendix C.

The variable is zero at the onset of the pandemic crisis (in February and early March 2020). In Figure 2.3, daily variation in policy options at country level shows when and where IS policies have been activated. In our sample, 37 out of 43 countries (86%) introduced IS programs during Feb. 15–Sept. 3, for did so for 120 days on average.

Figure 2.3: Daily Country-Level Policy Options including Stay Home Orders and Income Support



Source: Oxford COVID-19 Government Response Tracker.

2.2.4 Additional Data

Mobile Internet Access. Google mobility data is based on Google Location History (GLH) in users' mobile devices. Android devices are increasingly popular in low- and middle-income countries as an affordable way to access the internet. According to the [Pew Research Center \(2019\)](#), the average smartphone ownership rate in 2018 was around 45% in emerging economies (76% in advanced economies) with a rate of cellular subscriptions that has reached an average of 115 per 100 people. Yet, mobile internet might predominantly concern wealthier areas, where more people can afford smartphones ([Ballivian et al., 2015](#)). Even in this case, the effect of poverty on mobility may represent an interesting lower bound of the true effect if GLH information captures the mobility of the least poor within poorer regions, i.e. those who could reduce their mobility the most. To investigate this point further, we exploit two surveys, the Latinobarometer and the Afrobarometer, in which respondents are asked whether they own a smartphone or a mobile phone with internet. Individual measure of poverty are also provided so that we can check the relative poverty of mobile internet users across regions.

Population Density, Informality and Trust in Government. Poverty is often correlated with population density. Hence, as a sensitivity check, we mobilize the database ‘Gridded Population of the World’ by the CIESIN of Columbia University. The data records population count for 30 arc-second (about 1km on average) grid cells. While each subnational region has a varying number of cells, we use the average population count per cell in a given region. We split subnational regions into higher- and lower-population density groups based on whether regional density is above or below country-specific median level. Additionally, we check for the role of occupations in explaining potential differences across regions in mobility reduction. We use occupation data from the Afrobarometer, Latinobarometer and Arabarometer to construct a regional rate of informal employment – including agriculture, street vendor and small-firm workers – and define lower- and higher-informality regions using country-level median as cutoff. Finally, we test whether the effect of poverty and IS on mobility varies with the level of trust in the national government. We exploit data from the barometers surveys to measure regional level of trust in government before the pandemic. In each of the barometers, respondents were asked to rank their level of trust on a 0-4 scale (from “no trust” to “a lot of trust”). We calculate regional-level average of reported trust and allocate regions into lower- and higher-trust groups within countries using country-level median as cutoff. Descriptive statistics for these additional variables by level of regional poverty are reported in Table 2.A3 in appendix A.

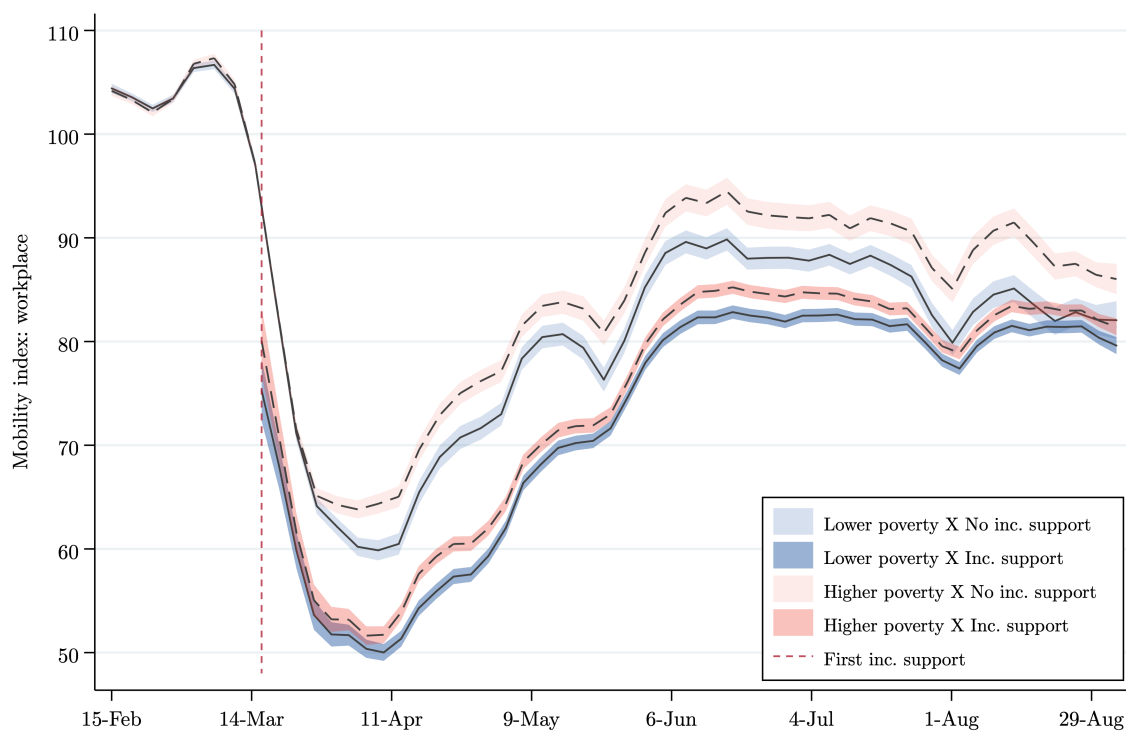
2.3 Suggestive Evidence from a Visualization of Mobility Patterns

Using these datasets, we first aim to measure the extent to which higher-poverty regions responded differently to SHO policies compared to lower-poverty ones, and then investigate whether IS helped mitigate the adverse effect of poverty on individuals’ capacity to reduce their exposure to the virus. We start with a graphical analysis of mobility patterns across subnational regions by local level of poverty.

Figure 2.4 depicts the daily average regional mobility to workplace for the period of Feb. 15 – Sept. 3, 2020, differentiating regional mobility patterns by pre-pandemic poverty incidence level and daily country-level IS status. We use all regions in our sample and a local polynomial fit with its 95% confidence interval (CI). Let us focus first on the average mobility of country-day cells without IS, the CI of which is depicted in light pink (blue) for lower (higher) poverty regions. Mobility is relatively constant in late February and early March, with small fluctuation around 100, i.e. similar levels of mobility as in the reference period. Around mid-March, many governments started to call for physical distancing, which leads to the sharp drop in work-related mobility illustrated in Figure 2.1 and 2.2. Before mid-March, SHO had not yet been implemented: lower-poverty and higher-poverty regions exhibit very similar mobility levels trends. More than a parallel trend verification, this observation tells us that in ‘normal times’, poverty does not lead to obvious differences in human mobility related to work.

By the end of March 2020, most countries in our sample had enacted SHO: mobility reaches a low peak at that time. After SHO are put in place, a marked difference appears between regions: those with lower poverty (light blue) reduced mobility significantly more than higher-poverty ones (light pink). These results are consistent with our initial hypothesis, namely that poverty contributed to push people out of their home to secure daily livelihoods despite the contamination risks. This poverty-related mobility gap becomes even slightly larger at the end of the period, when harsh economic conditions and depleted savings make it difficult for anyone – but perhaps more so for the poorest – to stay at home.

Figure 2.4: Work-related Mobility by Regional Poverty Levels with and without Income Support



Source: author’s calculations based on Google mobility data (mobility for workplace), poverty data from national statistics offices and authors’ estimations using household surveys, and OxCGRT data on COVID-19 income support. Local polynomial fit with 95% CI of daily mobility across districts, weighted by $(1/\#)$ of districts in the corresponding country). Poverty is measured as the share of people living below national or international poverty lines in a subnational district. Poverty is defined as lower (higher) if district’s poverty rate is below (above) country’s median district-level poverty rate. COVID-19 income support shows the daily status of whether government provides any income support to those who cannot work or lost their job due to the COVID-19 pandemic (country-day variation in income support).

Poverty hence undermines the efficacy of containment policies – or more generally the ability for people to self-isolate – but can be counteracted by IS programs such as those launched in Spring 2020. Many governments have introduced new or additional social protection programs to help people cope with lockdowns and income losses, as illustrated in Figure 2.3. As explained, we use the OxCGRT indicator, which records IS introduced in response to the COVID-19 pandemic either as a new program or as an extension of existing schemes. COVID-19 IS programs began as early as March 16th (in Belize and Peru), and by mid-April around 70% of the countries had a specific transfer in place. Figure 2.4 represents the average level of the mobility index in presence

of IS, starting from March 16th, for higher- (dark pink) and lower-poverty (dark blue) regions. This leads to the central (suggestive) results of this study. First, upon provision of IS, the level of mobility shifts downwards for both types of regions: pandemic-related IS schemes have seemingly helped improve compliance with containment rules and reduce work-place mobility. Second, the impact seems larger among higher-poverty regions: the difference between lower- and higher-poverty regions was larger in the absence of IS (light blue vs light pink) than when IS were in place (dark blue vs dark pink), and the difference tends to disappear in the latter case. This finding hints towards the health externalities of social protection for poor people who have to continue work-related mobility to maintain livelihoods during the pandemic. The rest of the paper aims to test this result while accounting for country-level unobserved confounders.

2.4 Estimations

2.4.1 Empirical Approach

Our empirical strategy puts some structure on the previous visualization of the results while allowing us to control for different sources of potentially confounding effects. Most importantly, we do not aim to identify the effect of SHO and IS policies on mobility patterns overall, since many country-level time-varying confounders affecting mobility trends hinder the identification of policy effects (e.g. differences regarding pandemic trends, economic trends, state capacities to enforce measures, the evolution of threat perceptions and citizen compliance, etc.). Our analysis focuses exclusively on the estimation of *within-country heterogeneity* in mobility patterns between higher- and lower-poverty regions at the time of implementation of SHO and IS policies. For this reason, the level of regional poverty is defined relative to the country-specific median. We select only country-day cells for which the policy mix is as follows: no policy, SHO only or both SHO and IS.¹¹

Mobility changes with respect to the pre-lockdown period (i.e. January 2020) in a subnational region i of country c on day t is estimated as:

$$Mobility_{it} = \alpha Poverty_i + \beta Poverty_i \times SHO_{c(i)t} + \gamma Poverty_i \times SHO_{c(i)t} \times IS_{c(i)t} + \theta_{c(i)t} + \varepsilon_{it} \quad (2.1)$$

For $Poverty_i$, we rely on the binary measure of regional poverty incidence before the pandemic, i.e. taking the value of one if regional poverty rate is above the country median (higher poverty) and zero otherwise (lower poverty). Our baseline indicator to define lockdown (income support) periods, $SHO_{c(i)t}$ ($IS_{c(i)t}$), is a dummy variable equal to one if SHO (IS programs) are enforced in country c on day t . All time-varying confounders are captured in country-day fixed effects $\theta_{c(i)t}$. As discussed, they possibly include country-specific trends in the pandemic, in the economic situation, in citizens' awareness about the virus, in SHO enforcement ability and in health service capacities, etc. The policy mix for country c on day t is also absorbed by these

¹¹Note that we ignore here and in our application the few days during which SHO are lifted while IS is still operational. It corresponds to the light blue dots in Figure 3, which represent very few cases (5.1% of the sample).

effects, but this is not an issue since we focus only on their relative effect in higher-poverty regions compared to lower-poverty ones. Comparing periods/regions without and with SHO identifies the coefficient β , i.e. the poverty gap in mobility patterns between the two types of regions, when only SHO are in place. Comparing periods/regions with SHO only and SHO+IS identifies coefficient γ , i.e. the additional shift in the poverty-induced mobility gap caused by the introduction of IS. In appendix B, we provide a more detailed account of this identification strategy by decomposing the different periods and show that the model of equation 2.1 captures the main variables of interest.

Note that estimates of interest are conditional on time-invariant differences between lower- and higher-poverty regions, captured by coefficient α . Also, we cluster standard errors at the country level over time to address both autocorrelation and the correlation of error terms between regions within countries, since countries are the level of policy decisions. We also reweight each observation by the inverse of the number of subnational regions in each country, in order to avoid over-representation of a country characterized by an administrative structure with numerous regions.

As we focus solely on within-country heterogeneity in mobility patterns during the COVID-19 policies, our approach slightly diverges from standard two-way fixed effect (TWFE) estimations, or more generally from difference-in-difference (DD) estimations, which usually aim at the identification of policy effects. Nevertheless, it still inherits similar features and identifying assumptions under which our estimates of β and γ are correctly identified. In particular, the fact that lower- and higher-poverty regions have the same level and trends of mobility before the pandemic, as seen above, is reassuring, even it has little external validity regarding whether these regions should behave similarly – at least in trend – in the presence of the pandemic but in absence of policies. In this respect, the fact that our demonstration focuses on regional heterogeneity during the pandemic and under different policy regimes is compelling but broadly suggestive.¹²

Further, the fact that COVID-19 policies started at different dates across countries may pose certain methodological issues. In recent developments of the DD literature, this ‘staggered’ nature of policy implementations implies that our coefficients of interest β and γ in equation 2.1 are weighted averages of classic ‘2x2’ DDs (cf. Goodman-Bacon and Marcus, 2020). In our specification, this induces a weighted average of the within-country heterogeneous effects. If these heterogeneous effects change over time, OLS estimates of β and γ might be biased and/or driven by specific weights.

To attempt to address these issues, we complement our estimation of the average effects with an event study design whereby we investigate the (daily) dynamics of within-country heterogeneity. With this approach, one can observe (i) whether there were any within-country heterogeneous

¹²We admit that the lack of more extensive data on regional characteristics prevents us to rely on conditional exogeneity assumption as an identification strategy (although we provide estimation results, as a robustness check, using available data on regional characteristics).

effects before SHO/SHO+IS, (ii) whether these effects have changed over time after policies were enacted, and (iii) whether there were any effects with reverse sign or outliers that could be affecting estimates of β and γ in conjunction with the implicit weights given by OLS estimates of equation 2.1. We combine both SHO and IS policies in one event study design,¹³ where we normalize dates to the first date of SHO for each country, and split each ‘lead’ day into ‘SHO only’ and ‘SHO+IS’ groups:

$$\begin{aligned}
Mobility_{it} = & \sum_{j=1}^J \alpha_j Poverty_i \times (Lag\ j)_{c(i)t} + \\
& \sum_{k=1}^K \beta_k Poverty_i \times (Lead\ k)_{c(i)t} \times 1 [IS_{c(i)k} = 0] + \\
& \sum_{k=1}^K \gamma_k Poverty_i \times (Lead\ k)_{c(i)t} \times 1 [IS_{c(i)k} = 1] + \theta_{c(i)t} + \varepsilon_{it}
\end{aligned} \tag{2.2}$$

with

$$\begin{aligned}
(Lag\ j)_{c(i)t} &= 1 [t = SHOstart_{c(i)} - j] \\
(Lead\ k)_{c(i)t} &= 1 [t = SHOstart_{c(i)} + k]
\end{aligned}$$

As before, $Poverty_i$ is the binary measure of regional poverty incidence before the pandemic and based on where a region is above or below the country median. $IS_{c(i)k}$ is a dummy variable equal to one if IS programs are enforced in country c on day k since the start of SHO. In this model, the absence of heterogeneity between lower-poverty and higher-poverty regions before SHO implementation, i.e. $\alpha_j = 0$ for $j = 1, 2, \dots, J$, would provide supporting evidence for our parallel trends assumption. β_k and γ_k are the estimates of mobility gaps when SHOs are in place without and with IS respectively. Note that the coefficient γ from equation 2.1 shows the *difference* in mobility changes across more/less poor regions between SHO only and SHO+IS scenarios, while coefficients γ_k here show the level of mobility changes when both SHO and IS are active.

2.4.2 Average Results based on Within-country Heterogeneity

Our main results are reported in Table 2.1. In the first row, the coefficient on poverty is rarely significant, indicating a mobility pattern in higher- and lower-poverty regions before the implementation of COVID-19 policies. Column (i) shows results for a model where regional controls μ_i include only the poverty status.¹⁴ According to the estimate of β in the second row, poorer regions experience a smaller reduction in mobility than higher-poverty regions during SHO periods. The differential of almost 5.2 points on the index is significant and substantial.

¹³Note that most countries introduced IS after the enactment of SHO, but a few countries started IS together with SHO. This allows us to estimate heterogeneity effects for SHO+IS policy combination from the first day of SHO in our event study design.

¹⁴Similar results are obtained when using the continuous measure of regional poverty, see results in Table 2.A4 in appendix C.

Indeed, it represents around 20% of the average drop in mobility (i.e. 25.8 points on our 0-100 scale) calculated among lower-poverty regions between the pre-lockdown period and the days with SHO.

Table 2.1: Effect of Stay-at-Home Orders and Income Support on the Differential Workplace Mobility of Poorer Regions

Dep. Var.: Mobility to Workplace	All countries	Africa	Latin America	Middle East & Asia	Low & lower-middle income	Upper-middle income
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Poverty	0.574* (0.317)	0.877 (0.527)	0.664 (0.545)	0.039 (0.614)	0.491 (0.448)	0.668 (0.460)
Poverty X SHO	5.167*** (1.166)	6.436*** (1.239)	1.273** (0.451)	6.466* (2.955)	5.544*** (1.303)	4.699** (2.088)
Poverty X SHO X IS	-3.320** (1.283)	-4.109** (1.650)	0.350 (1.111)	-4.754 (2.915)	-4.588*** (1.247)	-2.118 (2.376)
R-squared	0.841	0.834	0.883	0.733	0.802	0.880
Observations	132,639	35,163	60,214	37,262	61,272	71,367
Country X Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Region reweighting	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No

Source: authors' estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors' estimations) and information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region's poverty rate is below (above) country median poverty rate. Stay-at-Home indicates the period in which national stay-at-home orders, either recommendations or requirements, are imposed. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when stay-at-home orders are lifted, following the first lockdown, are excluded. Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Standard errors clustered at country level in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The third row reports the estimate of γ . It shows that the poverty gap in mobility trends is reduced by 3.3 points when IS are in place. That is, with IS, the poverty gap falls to 1.85 points only or 7.2% of the average mobility drop in lower-poverty areas. The rest of the table reports estimates for specific geographic areas. The within-country poverty gap is present everywhere but stronger on the African and Asian continents (cf. columns ii-iv). Consistently, it is larger in poorer countries (cf. comparing columns v and vi). The IS effect is also large and significant in these countries, yet it is null in Latin America, which conveys that there is much less within-country behavioral differences in this area.

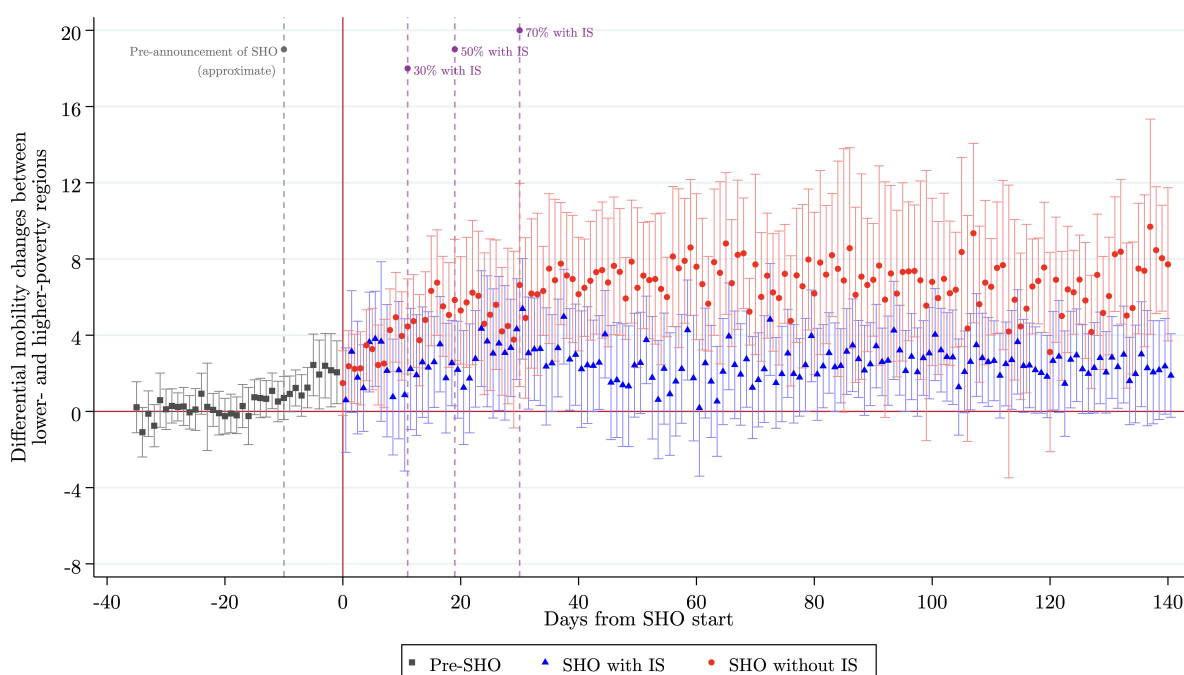
2.4.3 Results of the Event Study Analysis

Estimation results of our event study design, as defined in equation 2.2, are illustrated in Figure 5. Gray squares indicate the daily estimates of within-country heterogeneity before SHO, i.e. estimates of α_j in equation 2.2. Blue triangles and red circles depict the daily estimates for 'SHO only' and 'SHO+IS' cases respectively after the SHOs are enacted, i.e. estimates of β_k and γ_k from equation 2.2. Capped spikes indicate the 95% confidence interval for each point estimate.

From pre-SHO estimates, we confirm that there is no within-country heterogeneity in mobility levels before the pandemic or no marked differences in mobility drops in early pre-SHO days.

Arguably, we observe a small poverty gap just before SHO, which can be interpreted as an anticipation during the few days prior to lockdown. This is possibly due to the announcement of SHO (e.g. we illustrate the average period between announcements and the effective date of SHO, which turns out to be 10 days on average over all countries in the data). The small effect may also be interpreted as regional differences simply due to the ability of the less poor to isolate more even in the absence of formal orders. This effect does not seem to impair our main results, as the direction of this anticipation or spontaneous sheltering is positive, implying that our main estimates in Table 2.1 can still be interpreted as a lower bound.

Figure 2.5: Dynamic Effects of Stay-at-Home Orders and Income Support on the Differential Workplace Mobility of Poorer Regions



Source: authors' estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors' estimations) and information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region's poverty rate is below (above) country median poverty rate. Stay-at-Home indicates the period in which national stay-at-home orders, either recommendations or requirements, are imposed. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when Stay-at-Home orders are lifted, following the first lockdown, are excluded. The estimation is based on the equation (2). For each (daily) point estimate, capped spikes indicate 95% CI based on standard errors clustered at country level.

Moreover, whether poverty affects the ability to comply to SHO or the possibilities to spontaneously isolate, the most important finding is the reducing effect of IS policies. It can be interpreted as helping the poorer to stay at home *in general* as much as the rest of the population. The estimates of mobility gaps in Figure 2.5 illustrate these patterns. They show that the difference between higher- and lower-poverty regions in a context without IS (red circles) are smaller in the early days of SHO but oscillates between 4 and 8 points shortly after the start of SHO. When SHO is combined with IS (blue triangles), the poverty gap in mobility decreases to around 2-4 points. In almost all days reported in Figure 2.5, the mobility gap is larger when IS policies complement SHO, which reassures that our estimates in Table 2.1 are not driven by

few outlier cases or by weights, and rather correctly reflect the general pattern across all policy days. The averages of the daily effects are close to the averages estimate reported in Table 2.1 (around 6 points with SHO alone and down to 3 points on average when IS are in place).

2.4.4 Robustness Checks and Heterogeneity Analysis

Non-work-related Mobility as Placebo. Previous results are consistent with the conjecture that work-related mobility was less reduced among poor people because of the urgency to make ends meet. As a placebo test, we verify that the poverty gap is less pronounced when other types of mobility are considered. Table 2.2 reproduces baseline results for average effects in column (i). These results are confirmed when adding further regional controls, namely mobile internet access, population density and trust, in column (ii), even though the sample size is reduced by around half in this case. Estimates for mobility related to other essential activities, namely going to grocery/pharmacy, are reported in columns (iii) and (iv). The regional differences in grocery/pharmacy mobility are insignificant when adding region-level controls, supporting the assumption that non-compliance with mobility restrictions is mostly associated with work-related activities for the poorest.¹⁵ Similar patterns are also found in event study estimates reported in Figure 2.A1 in appendix C.

Table 2.2: Effect of Stay-at-Home Orders and Income Support on the Mobility of Poorer Regions : Placebo Test

	Workplace Mobility		Grocery/Pharmacy Mobility	
	(i)	(ii)	(iii)	(iv)
Poverty	0.574* (0.317)	-0.252 (0.511)	-0.428 (0.362)	-0.339 (0.470)
Poverty X SHO	5.167*** (1.166)	5.538*** (1.312)	3.175* (1.858)	0.800 (0.748)
Poverty X SHO X IS	-3.320** (1.283)	-3.348** (1.542)	-2.906 (2.022)	0.152 (1.271)
R-squared	0.841	0.899	0.764	0.872
Observations	132,639	68,615	132,639	68,615
Country X Day FE	Yes	Yes	Yes	Yes
Region reweighting	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Source: Source: authors' estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors' estimations) and the information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region's poverty rate is below (above) country median poverty rate. Stay-at-Home indicates the period in which national stay-at-home orders (recommendations or requirements) are imposed. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when stay-at-home orders are lifted, following the first lockdown, are excluded. Regional control variables include mobile internet access rate, regional population density (in log) and average regional score for trust in government (in log). The data on regional controls is available only for subset of our sample and we lose observations when we control for regional characteristics. Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Standard errors clustered at country level in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1

Mobile Internet Access. Using Google mobility reports in the context of poverty analysis raises a question of population representativeness. Mobility is indeed measured only for smart-

¹⁵Note that we impute few missing values of grocery/pharmacy mobility with 3-day, 5-day or 7-day moving averages at regional-level, or if needed, with daily national average to make the sample comparable across robustness checks (similar results are found without imputations).

phone users. If the poorest within poor regions have the least access mobile internet and are at the same time the least able to stay at home, our approach likely underestimates the effect of poverty on workplace mobility. Inversely, if poor regions are also the most unequal, so that average mobile internet users in these regions are in fact ‘richer’ than average mobile internet users in less poor regions, our conclusions would however be reversed. We first check this last critical point using individual measures of poverty provided in the three barometers surveys. We find that the average smartphone user in poor areas is poorer than the average user in less poor areas. We also confirm that our result is likely a lower bound, as nonusers are significantly poorer in poor regions than nonusers in less poor regions. Finally, as additional robustness checks, we replicate our estimations to capture heterogeneous effects depending on regional levels of mobile internet access. Table 2.3 reports estimates of a model where we interact the key variables of interest ($Poverty_i \times SHO_{ct}$ and $Poverty_i \times SHO_{ct} \times IS_{ct}$) with dichotomous regional characteristics.¹⁶ In columns i-ii, these characteristics are coded as a region above versus below the median mobile internet access rates. We see that results are qualitatively similar in both cases.¹⁷

Table 2.3: Effect of Stay-at-Home Orders and Income Support on Differential Workplace Mobility of Poorer Regions: Heterogeneity Effects¹⁸

	Mobile Internet Access		Population Density		Informal employment		Trust in Government		Stay-at-Home Targeting	
	Low	High	Low	High	Low	High	Low	High	Regional	National
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)
Poverty X SHO	4.566*** (1.021)	5.225** (2.182)	5.897*** (1.055)	3.906*** (1.310)	7.046*** (1.690)	4.574*** (1.390)	6.617*** (1.908)	5.826*** (1.313)	4.173* (2.158)	5.420*** (1.285)
Poverty X SHO X IS	-2.350* (1.335)	-4.749* (2.706)	-3.208** (1.252)	-3.121** (1.507)	-5.266** (2.042)	-2.717* (1.628)	-5.005** (2.119)	-3.546** (1.638)	-4.327** (2.017)	-2.963** (1.443)
R-squared	0.890		0.847		0.885		0.885		0.842	
Observations	69,955		132,639		78,455		77,515		132,639	
Country X Day FE	Yes		Yes		Yes		Yes		Yes	
Region reweighting	Yes		Yes		Yes		Yes		Yes	
Controls	Yes		Yes		Yes		Yes		Yes	

Source: authors’ estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors’ estimations) and the information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region’s poverty rate is below (above) country median poverty rate. Stay-at-Home indicates the period in which national stay-at-home orders (recommendations or requirements) are imposed. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when stay-at-home orders are lifted, following the first lockdown, are excluded. Not to loose too many observations, we limit the set of controls to the heterogeneity of interest (e.g. for results with heterogeneous effects by level of mobile internet access, we include a dummy for a lower level of mobile internet access rate). Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Standard errors clustered at country level in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1

Alternative Mechanisms: Population Density and Activity Types. We explore additional potential confounders, starting with the urban/rural divide measured as above/below median of population density. If poorer regions are also rural, our results may be affected by

¹⁶Note that here we report across columns ‘group’ coefficients, which are calculated using the coefficients from the single estimations with interacted terms.

¹⁷Note that this check is reassuring but only suggestive since it is based on a subset of countries for which mobile internet information is available (374 subnational regions across 27 countries in Africa and Latin America).

¹⁸We have here differences in sample size across columns because the data on regional controls is available only for subset of countries/regions and additional information tends to restrict sample size. We try to avoid unnecessary loss of observations by using maximum available non-missing data related to each heterogeneity check. If we impose subset that is common to all control variables, then sample size becomes too small.

other mechanisms including regional specificities in terms of labor market activities, urban/rural differences in mobile internet coverage, or potentially larger difficulties to capture workplace mobility (and its drop during SHO) in rural areas. We follow the same logic as above, i.e. interact key variables with regional heterogeneity: Table 2.3 (columns iii-iv) shows that the poverty gap in mobility changes is present and significant during SHO for both lower and higher levels of population density (although slightly smaller in more urban areas). The effect of IS is similar for both groups and comparable to baseline estimates. Similar concerns may arise if workplace mobility is not well capture for broader types of activities: agricultural work but also informal employment (for instance, work as informal vendors may be confounded with non-work activities).¹⁹ We thus complete our heterogeneity analysis by interacting key treatment variable with regional dummies for an above/below median informality rate. Results in Table 2.3 (columns v-vi) point to larger estimates in low-informality regions and slightly smaller, but of similar order of magnitude, in high-informality regions.

Alternative Mechanisms: Trust. Next, an alternative mechanism is the confidence in governments, which may affect our results if it is correlated with poverty. Several papers have documented that lower trust is associated with a lower degree of adherence to containment measures in the context of the COVID-19 pandemic (Bargain and Aminjonov, 2020; Brodeur, Grigoryeva and Kattan, 2021). Table 2.3 (columns vii-viii) points again to consistent results when differentiating poorer regions by level of trust in government. For both higher- and lower-trust levels, we find significant poverty gaps in mobility during SHO period. While IS corrects around 60% of the poverty effect in the baseline and in high-trust region, this correction reaches almost 76% in the case of low-trust regions, which might signal another positive externality of IS, i.e. the ability to “compensate” for lower trust in government in the poorer regions.

Policy Applicability within Countries. Finally, it is possible that policies differ across regions of a given country. This might be the case in federal systems for instance. To check this, we use information from OxCGR (2020) on whether containment policies during the first wave of SHO were applied nationally or were geographically targeted. We interact treatment variable with this country heterogeneity. Results in Table 2.3 (columns ix-x) show no difference between countries with uniform versus sub-national SHO. Regarding IS policies, we do not avail of the same type of disaggregated information but the degree of decentralization might be similar and hence captured in this estimation. Anyhow, if poorer regions are harder to reach and targeting was not optimal during the pandemic, or if there might have be within-country differences in local capacities and transfer provisions that may be related to poverty differences, our baseline estimations of the IS would be a lower bound.

Additional Results and Robustness Checks. We further check sensitivity of our results to methodological choices made in our main estimations. Results are reported in Table 2.A4 in appendix C. We show that the pattern of our baseline estimates hold without reweighing regions

¹⁹In this case, workplaces might be more ‘unique’ and identifiable in low-poverty regions, so that a drop in activity is also more easily identified in these areas, which would exacerbate our poverty gap in mobility reduction.

or with alternative weights. Similarly, when we apply continuous measures of containment policy stringency (using Policy Stringency Index from OxCGRT which combines all containment measures and their strictness level into a single index) or continuous measure of regional poverty (using poverty rate standardized with respect to country mean and standard deviation), the results are in line with baseline estimates. Additional findings related to the intensity of SHO strictness and IS programs are discussed in appendix C. In particular, we exploit the fact that OxCGRT (2020) reports information on IS levels, with high/low IS corresponding to transfers that cover more/less than half of the earnings losses due to the pandemic. We use this information instead of the binary IS for the only two countries in the sample that switched from low to high IS over the period (Chile and Uruguay). We find that more mobility reduction is achieved with higher IS levels. Although the exploitable time switch for causal inference is available for two countries only, this additional result supports our main interpretation that IS helps reduce the poverty-related gap in mobility.

Further Discussion. There might be other mechanisms that we do not cover in this sensitivity analysis. In that sense, our evidence is suggestive. Note however that the problem of time-varying confounders would be more acute if we compared regions of the world (rather than regions within countries). This was the approach followed in a previous version of the present paper (Aminjonov et al., 2021), defining poverty according to the global median of regional poverty and excluding country-day effects so that both between- and within-country variation were used. These results, summarized in appendix D, convey a similar message: the poverty gap in work-related mobility is significant during lockdowns but significantly lower when IS programs are switched on. This means that the pattern emphasized in the present paper is pervasive and found at different levels: when comparing regions within countries or when comparing countries or groups of countries by poverty levels.

Implication for Virus Diffusion. Since our findings highlight the importance of social assistance for the poor during a pandemic, not only in terms of securing livelihoods but also in reducing the risk of infection, we complete the analysis by examining the implications for the propagation of COVID-19. We combine the estimated poverty-elasticity of mobility with an estimate of the mobility-elasticity of virus diffusion. Detailed calculations are presented in appendix E. We find that IS policies have likely resulted in a slower spread of the virus through work-related mobility: switching from a lower- to a higher-poverty region within a country is associated with 51% more COVID-19 cases after five months when SHO operate without IS. This gap is reduced to only 26% additional cases when IS is provided.

2.5 Conclusion

The spread of COVID-19 and consequent restrictions on economic activity through containment policies has posed a serious threat to the livelihoods of many among poor regions of the world. Governments have responded to this with an unprecedented expansion of their social protection

programs and new transfers. Relative to pre-COVID levels, benefits have nearly doubled and coverage has grown by 240% on average ([Gentilini et al., 2020](#)). Admittedly, government assistance has been insufficient to sustain pre-crisis living standards and to prevent a sharp increase in food insecurity ([Egger et al., 2021](#)). Nevertheless, emergency support provided in response to the COVID-19 pandemic may have substantially helped to reduce the exposure of the poor to the virus.

We support this claim with new evidence exploiting spatial and time variation in patterns of human mobility across 729 regions of low- and middle-income countries. Regions with a higher incidence of poverty before the pandemic are concerned by a significantly lower reduction in work-related mobility, which we interpret as a lower ability to self-protect and comply with containment rules. Income support programs provide strong mitigating effects, allowing all regions to reduce mobility further but more so in poorer region, so the poverty gap in mobility partly disappears and the relative propensity of the poor to avoid infection increases. This conclusion stems from our global estimates comparing regions, but also at more aggregated levels, and is corroborated by event study analyses.

Our findings support the idea that poorer and more vulnerable groups should be targeted by substantial transfers in times of pandemic, as they allow governments to minimize the adverse welfare effects of containment policies and, critically, to maintain a higher level of adherence to these policies in poorer regions. This conclusion obtained from regional variation at a global scale completes the literature focuses on country-specific cases (e.g. [Banerjee et al., 2020](#)). It allows estimating positive health externalities of income assistance programs and the way they may have reduced the spread of the virus among poorer segments of the population.

Further research should provide more fine-grained information on policy options and their relative effectiveness. This includes how the nature of the transfers, and in particular cash versus in-kind benefits, affects livelihoods and health externalities during a pandemic. The mode of targeting could also be further explored, for instance the quality of pre-pandemic targeting strategies versus new proxy-mean tests or community-based targeting ([McBride and Nichols, 2018](#)), the use of universal transfers versus the horizontal scaling up of existing schemes (such as temporary removals of the conditionality of some CCT) or innovative ways to reach informal workers ([Carranza et al., 2020](#)).

Appendix

Appendix A: Variable Definitions and Poverty Measures

Table 2.A1: Variable Descriptions

Variable	Type	Level of Measurement	Description/Values	Source
Mobility Index: Workplace / Grocery & Pharmacy	Continuous	Regional	An index measuring daily changes in the number of visits or time spent at workplace / groceries or pharmacies. Measured on (0-100) scale, with 100 as the baseline or pre-pandemic level.	Google COVID-19 Mobility Reports
Poverty	Binary	Regional	A dummy variable taking the value 1 if regional poverty headcount ratio is above the global median of regional poverty rates.	Official poverty statistics, authors' calculations using household surveys
Poverty Headcount Ratio / Standardized Poverty Rate	Continuous	Regional	Share of regional population living below international or national poverty lines. Value range [0-100] / Poverty Headcount Ratio standardized with respect to its global mean and standard deviation.	Official poverty statistics, authors' calculations using household surveys
Stay-at-Home Orders	Binary	National	A dummy variable measuring daily changes in stay-at-home orders (recommendations or requirements) during the COVID-19 pandemic and taking the value 1 if stay-at-home orders are enforced.	OxCGRT, Hale et al. (2020)
Income Support	Binary	National	A dummy variable measuring daily changes in income support provided to those who lost their jobs or cannot work due to the COVID-19 pandemic and taking the value 1 if income support is provided.	OxCGRT, Hale et al. (2020)
Cumulative number of COVID-19 cases	Continuous	National	A variable measuring daily changes in the number of cumulative reported COVID-19 cases.	European Center for Disease Prevention and Control
Mobile Internet Access	Binary	Regional	A dummy variable taking the value 1 if the share of survey respondents who own a smartphone or a mobile phone with internet access is above the country-specific median.	Afrobarometer 2019, Latinobarometer 2018
Population Density	Binary	Regional	A dummy variable taking the value 1 if regional population density is above the country-specific median.	Center for International Earth Science Information Network (CIESIN), Columbia University
Trust	Binary	Regional	A dummy variable taking the value 1 if the regional-level average trust score is above the country-specific median.	Afrobarometer 2019, Latinobarometer 2018, and Arabarometer
Informality	Binary	Regional	A dummy variable taking the value 1 if the regional rate of informality is above the country-specific median.	Afrobarometer 2019, Latinobarometer 2018, and Arabarometer

Table 2.A2: Description of Poverty Data

Country	Poverty Line*	Welfare Measure	Data Source / Institute	Web-link / Web-page (report title)
Argentina	17.1	per capita income	Permanent Household Survey (EPH), 2019 / National Institute of Statistics and Census of Argentina (INDEC)	https://www.indec.gov.ar (Condiciones de vida Vol. 4, n° 4)
Belize	9.3	per capita consumption	Living Standard Measurements Survey, 2009 / Government of Belize and the Caribbean Development Bank, 2009	http://sib.org.bz/ (Country Poverty Assessment Report)
Bolivia	7.6	per capita income	Encuesta de Hogares, 2018 / National Institute of Statistics of Bolivia (INE)	https://www.ine.gov.bo (Pobreza y Desigualdad)
Botswana	2.2	per capita consumption	Botswana Multitopic Household Survey, 2015/2016 / Statistics Botswana	http://www.statsbots.org.bw (Poverty Stats Brief)
Brazil	5.5	per capita income	Continuous National Household Survey (PNAD Contínua), 2018 / Brazilian Institute of Geography and Statistics (IBGE)	https://www.ibge.gov.br/estatisticas/ (Síntese de Indicadores Sociais 2019)
Burkina Faso	1.8	per capita consumption	Enquête multisectorielle continue (EMC), 2014 / National Institute of Statistics and Demography (INSD)	http://www.insd.bf/ (Profil de pauvreté et d'inégalités)
Cambodia	2.5	indirect poverty estimation	The Commune Database Predictive Poverty, 2012 / Asian Development Bank, Ministry of Planning and United Nations Development Programme	https://www.adb.org/ (Country Poverty Analysis 2014)
Cape Verde	4.6	per capita consumption	III Inquerito as Despesas e Receitas Familiares, 2015 / Departamento das Estatísticas Demográficas e Sociais	http://ine.cv (Tables do Perfil da Pobreza)
Chile	11.6	per capita income	Encuesta Casen, 2017 / Ministerio de Desarrollo Social	http://www.desarrollosocialyfamilia.gob.cl/ (Informe Desarrollo Social 2019)
Colombia	5.3	per capita income	Integrated Household Survey (GEIH), 2018 / National Administrative Department of Statistics (DANE)	https://www.dane.gov.co/ (Condiciones vida, pobreza monetaria 18 departamentos)
Côte d'Ivoire	2.9	per capita consumption	Enquête sur le Niveau de Vie des Ménages, 2015 / Institut National de la Statistique de Côte d'Ivoire	http://www.ins.ci/ (Profil de pauvreté)
Dominican Republic	6.5	per capita income	Encuesta Nacional Continua de Fuerza de Trabajo (ENCFT), 2019 / Ministerio de Economía, Planificación y Desarrollo (MEPYD), Oficina Nacional de Estadística (ONE)	https://mepyd.gov.do (Boletín de estadísticas oficiales de pobreza monetaria en la República Dominicana año 5, no 7)
Ecuador	4.7	per capita consumption	Encuesta de Condiciones de Vida, 2013/2014 / Instituto de Estadística y Censos (INEC)	https://www.ecuadorencifras.gob.ec
Egypt	6.6	per capita consumption	Household Income, Expenditure and Consumption Survey (HIECS), 2015 / Central Agency for Public Mobilization and Statistics (CAPMAS)	https://www.capmas.gov.eg (Regional poverty calculated by El-Haity and Armanious (2018) based (HIECS))
El Salvador	5.1	per capita income	Encuesta de Hogares de Propósitos Múltiples (EHPM), 2018 / Dirección General de Estadística y Censos (DIGESTYC)	http://www.digestyc.gov.sv/ (Principales Resultados de la Encuesta de Hogares de Propósitos Múltiples)
Gabon	5.7	per capita consumption	Enquête Gabonaise pour l'Évaluation de la Pauvreté 2, 2017 / Direction Générale de la Statistique	https://www.statgabon.ga/ (Gabon : Profil de Pauvreté 2017)
Guatemala	6.4	per capita consumption	Encuesta Nacional de Condiciones de Vida, 2014 / Instituto Nacional de Estadística	https://www.ine.gov.gt/ (Encuesta Nacional de Condiciones de Vida 2014: Principales resultados)
Haiti	3.7	per capita consumption	Enquête Sur les Conditions de Vie des Ménages Après le Séisme (ECVMAS), 2012 / The World Bank, Observatoire National de la Pauvreté et de l'Exclusion Sociale (ONPES)	https://www.worldbank.org/ (Investing in people to fight poverty in Haiti: Reflections for evidence-based policy making, World Bank Report)
Honduras	4.9	per capita income	Encuesta Permanente de Hogares de Propósitos Múltiples, 2018 / Instituto Nacional de Estadística	https://www.ine.gov.hn (Pobreza Monetaria)
India	2	per capita consumption	SDG dashboard/NITI Aayog (Tendulkar Committee Estimates), 2012 / National Institution for Transforming India (NITI Aayog)	https://niti.gov.in/state-statistics https://sdgindiaindex.niti.gov.in (SDG Poverty Statistics)
Indonesia	2.7	per capita consumption	National Socioeconomic Survey - Survei Sosial Ekonomi Nasional (Susenas), 2019 / Badan Pusat Statistik / Statistics Indonesia	https://www.bps.go.id (Poverty Statistics)
Jamaica	5.8	per capita consumption	Jamaica Survey of Living Conditions, 2012 / Statistical Institute of Jamaica (STATIN)	https://statinja.gov.jm/ (Mapping Poverty Indicators, Consumption Based Poverty in Jamaica)
Jordan	7.1	per capita consumption	Household Income and Expenditure Survey, 2010 / United Nations Development Program (UNDP), Jordan Department of Statistics (DOS)	https://www.undp.org/ (Jordan Poverty Reduction Strategy Final Report 2013)
Kenya	3.2	per capita consumption	Kenya Integrated Household Budget Survey (KIHBS), 2015/2016 / Kenya National Bureau of Statistics	http://statistics.knbs.or.ke (Basic report on wellbeing in Kenya)
Kyrgyzstan	4.2	per capita consumption	Official Statistics on Living Standards, 2018 / National Statistical Committee of the Kyrgyz Republic	http://www.stat.kg
Lebanon	15.6	per capita consumption	Household Budget Survey, 2011 / Central Administration of Statistics (CAS) and the World Bank	https://www.worldbank.org/ (Measuring poverty in Lebanon using 2011 HBS Technical report)
Mexico	6.9	per capita income	National Survey of Household Income and Expenditure (ENIGH), 2018 / National Council for the Evaluation of Social Development Policy (CONEVAL)	https://www.coneval.org.mx/ (Medición de la Pobreza)
Mozambique	1.7	per capita consumption	Inquérito aos Orçamentos Familiares, 2014/2015 / The World Bank, National Institute of Statistics of Mozambique	https://www.worldbank.org/ (Strong but not Broadly Shared Growth. Mozambique Poverty Assessment)
Namibia	2.7	per capita consumption	Namibia Household Expenditure Survey, 2015/2016 / Namibia Statistics Agency	https://nsa.org.na/ (Namibia Household Income and Expenditure Survey (NHIES) 2015/2016 Report)
Nigeria	3.2	per capita consumption	Nigeria General Household Survey (NGHS), 2018/2019 / National Bureau of Statistics	http://www.nigerianstat.gov.ng (authors' calculation using ECMVA 2014)
Pakistan	2.9	per capita consumption	Household Income and Expenditure Survey, 2015/2016 / The World Bank	https://worldbank.org (Redaelli (2019), Pakistan at 100 : From Poverty to Equity)
Paraguay	6.8	per capita income	Encuesta Permanente de Hogares, 2017 / Dirección General de Estadística, Encuestas y Censos	https://www.dgeec.gov.py (Principales Resultados de Pobreza y Distribución del Ingreso)
Peru	5.8	per capita consumption	National Household Survey, 2018 / National Institute of Statistics and Informatics (INEI)	https://www.inei.gov.pe (Evolucion de la Pobreza Monetaria 2007-2018, Informe Tecnico)
Philippines	3.1	per capita income	Family Income and Expenditure Survey, 2018 / Philippine Statistics Authority	https://psa.gov.ph (Poverty Incidence and Magnitude of Poor Families with Measures of Precision, by Region and Province)
Rwanda	1.3	per capita consumption	The Fifth Integrated Household Living Conditions Survey (EICV5), 2016/2017 / National Institute of Statistics of Rwanda	https://www.statistics.gov.rw (Rwanda Poverty Profile Report)
Senegal	2.9	per capita consumption	Deuxième Enquête de Suivi de la Pauvreté au Sénégal, 2011 / Agence Nationale de la Statistique et de la Démographie	https://www.ansd.sn (Rapport Definitif)
South Africa	3.6	per capita consumption	South Africa Living Conditions Survey, 2014/2015 / Statistics South Africa	https://www.gov.za (authors' calculation using South Africa LCS 2014/2015)
Togo	3.8	per capita consumption	Questionnaire Unifié des Indicateurs de Base du Bien-être (QUIBB), 2015 / Institut National de la Statistique et des Etudes Economiques et Demographiques	https://inseed.tg/ (Cartographie de la pauvreté)
Turkey	12.1	per capita income	Income and Living Conditions Survey, 2018 / TurkStat	http://www.turkstat.gov.tr/ (Poverty Statistics)
Uganda	1.2	per capita consumption	Uganda National Household Survey, 2016/2017 / Uganda Bureau of Statistics	https://www.ubos.org/ (Poverty Maps of Uganda, Technical Report)
Uruguay	8.8	per capita income	Encuesta Continua de Hogares, 2019 / Observatorio Territorio Uruguay (OPP)	https://otu.opp.gub.uy/ (Poverty Statistics by Department)
Zimbabwe	4.2	per capita consumption	Poverty Income Consumption and Expenditure Survey (PICES), 2017 / Zimbabwe National Statistics Agency	http://www.zimstat.co.zw/ (Zimbabwe Poverty Report 2017)

*In 2011 PPP dollars per capita per day

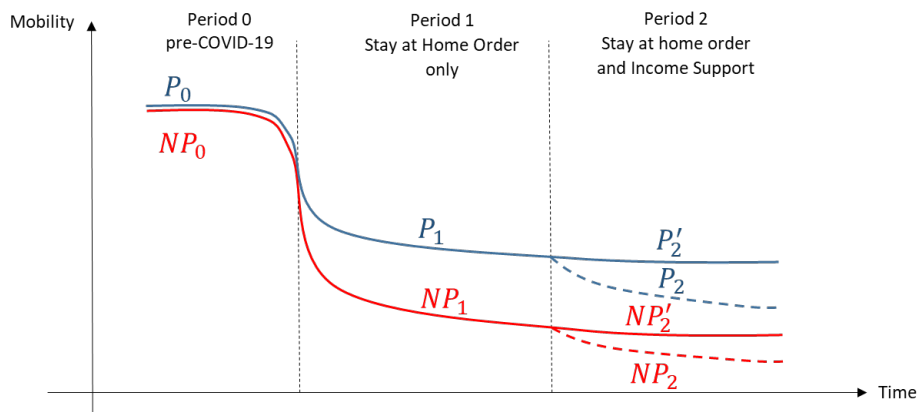
Table 2.A3: Descriptive Statistics for Region-level Control Variables

	Lower poverty	Higher poverty
	(i)	(ii)
Share of regions with higher population density	0.582 (0.494)	0.381 (0.486)
Share of regions with higher mobile internet access	0.543 (0.499)	0.314 (0.486)
Share of regions with higher trust in government	0.411 (0.493)	0.552 (0.499)
Share of regions with higher informality	0.405 (0.492)	0.566 (0.497)
Total number of subnational regions	380	349

Source: authors' calculations using the data on regional poverty rates (from national statistics or authors' estimations), Afrobarometer, Arabarometer, Latinobarometer and the database 'Gridded Population of the World' by the CIESIN of Columbia University. Standard deviations in parantheses. Note that statistics on mobile internet access, trust and informality are given for subsamples of regions due to data unavailability.

Appendix B. Main Approach: Detailed Description

We provide here a detailed explanation of our main empirical approach when decomposing the timeline into three distinct periods. Let us consider the simplified diagram below, representing mobility patterns over time for poor (P) and nonpoor (NP) regions. We distinguish three periods as follows: 0 corresponds to the pre-COVID-19 situation, 1 the period during which stay-at-home orders (SHO) are enforced but without income support (IS) program, 2 the days with both SHO and IS in place. This simplified sequence corresponds to the reality of most of the countries in our sample, if not all, and is sufficiently representative for our argument.



If we focus on periods 0 and 1, we may extract the differential effect of SHO between lower-poverty and higher-poverty regions by a difference-in-difference (DD) strategy where the average difference $P_1 - NP_1$ is corrected from the pre-lockdown difference $P_0 - NP_0$. Yet the analysis diverges from a classic DD. Indeed, the correction of the first $P_0 - NP_0$ difference is not very useful since regional differences in a "normal" situation are not very informative of the mobility paths that different regions may take when faced with a pandemic and an economic crisis.

Note that this would be even more an issue if we compared regions of different countries.²⁰ Thus, our main analysis focuses exclusively on the within-country heterogeneity between higher- and lower-poverty regions, with the regional poverty dummy $Poverty_i$ being defined relatively to the country-specific median (and all time-varying confounders being absorbed by country-day effects θ_{ct}). Hence, the approach retained here is not a standard DD analysis but rather a suggestive measure of the mobility gap between higher- and lower-poverty regions when SHO are in place. In a model applied to sub-periods 0 and 1, mobility in a subnational region i of country c on day t is written as:

$$Mobility_{ict} = \alpha + \beta Poverty_i \times SHO_{ct} + \theta_{ct} + \mu_i + \epsilon_{it} \quad (2.A1)$$

with SHO_{ct} a dummy variable indicating whether any type of SHO was enforced in this country on that day. Coefficient β captures the heterogeneity of interest, namely the poverty gap in mobility when SHO are introduced, which corresponds to a mix of the spontaneous self-isolation during period 1 and the reinforcing effects of SHO measures. We control in μ_i for the pre-crisis regional poverty status (variable $Poverty_i$) and additional regional characteristics such as population density and trust.²¹

Let us now consider period 2. On the diagram above, we represent mobility trends in the counterfactual situation without IS (NP'_2 and P'_2) and in the situation with IS (NP_2 and P_2), conjecturing that IS programs have succeeded in further reducing mobility. The effect of IS on the poverty gap in mobility trends is simply the actual poverty gap $\Delta_2 = P_2 - NP_2$ minus the counterfactual gap $\Delta'_2 = P'_2 - NP'_2$. A DD approach would consist in using $\Delta_1 = P_1 - NP_1$ in place of Δ'_2 for each country while netting out all time-varying country-specific confounders using country-time effects. The reasoning is now a bit different compared to periods 0 and 1: in contrast to Δ_0 , which we have described as providing little information, Δ_1 already gives an indication of regional responses (and their differences) to the health and economic shocks. Then, a model focusing on periods 1 (SHO alone) and 2 (SHO with IS) can be written as:

$$Mobility_{ict} = \alpha + \gamma Poverty_i \times IS_{ct} + \theta_{ct} + \mu_i + \epsilon_{it} \quad (2.A2)$$

with θ_{ct} absorbing time-varying confounders. The latter term also absorbs the overall effect of IS in each country, which is not a problem since we focus here on the heterogeneous effect γ , i.e. the poverty gap when IS programs are in place.

As suggested in the main text, we can estimate both equations 2.A1 and 2.A2 simultaneously

²⁰Implicitly in this case, we would face the problem of accounting for country differences in several factors (i.e. differences regarding pandemic trends, economic trends, state capacities to enforce measures, the evolution of threat perceptions and citizen compliance, etc.). This issue applies to the alternative approach presented in appendix D below and to the previous version of this paper (Aminjonov et al., 2021) It is also related to criticisms of COVID-19 analyses of DD analyses based on country variation (cf. Goodman-Bacon and Marcus, 2020).

²¹These variables are also interacted with SHO or IS dummies in our robustness checks in order to test alternative mechanisms beyond our work-related interpretation of the poverty gap.

by considering all three periods and the model:

$$Mobility_{ict} = \alpha + \beta Poverty_i \times SHO_{ct} + \gamma Poverty_i \times SHO_{ct} \times IS_{ct} + \theta_{ct} + \mu_i + \epsilon_{it} \quad (2.A3)$$

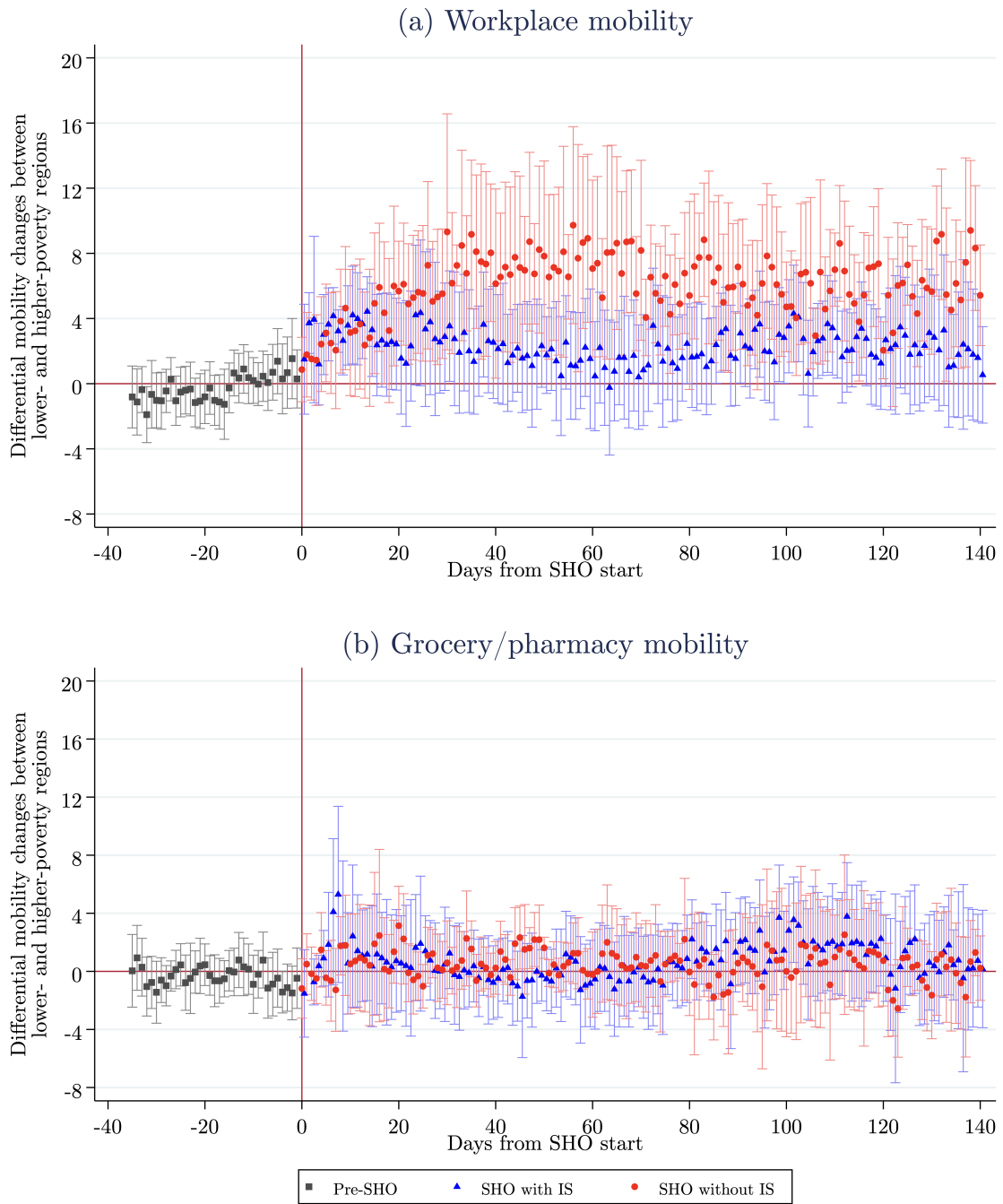
Country-day effects θ_{ct} absorb all country-specific time variation including the policy mix at each point in time (SHO, IS or both) and the underlying country trends in mobility due to other factors (country-specific evolutions of the pandemic, of the economic situation, of compliance, of health coverage, etc.). Coefficient β captures the poverty gap when only SHO are enforced; γ captures the correction effect of IS, i.e. a catching-up in mobility reduction by the poor thanks to IS.²² Note that by construction, we ignore here and in the empirical application the few days during which SHO are lifted while IS is still operational. This is relatively innocuous and concerns a marginal share of our observations (5.1% of days on average per country).

Appendix C. Additional Results and Robustness Checks

Figure A1 reproduces daily differential changes in mobility between lower- and higher-poverty regions for different types of mobility – here when including region-level controls. It confirms the marked poverty gap in mobility for work-related movements (graph a) and the absence of poverty gap in mobility for other activities for which poverty is not expected to matter (graph b).

²²Admittedly, from the description above, it is tempting to interpret the relative effect $-\gamma/\beta$ as the percentage reduction in the poverty gap due to IS. This is necessarily an approximation: for instance, the relative effect may be overstated if the counterfactual Δ'_2 was effectively larger than Δ_1 . Ideally, one would use observation just around the time when IS is introduced (i.e. around the second vertical dashed line on the diagram) to limit the influence of time changes. Yet, we observe in the data that in most countries, there were no sharp changes in mobility just after the introduction of IS, but rather a gradual change (as illustrated by the red and blue dashed lines on the diagram above). Using observations around the cutoff would not capture the full effect of IS, which certainly took time to materialize.

Figure 2.A1: Dynamic Effects of Stay-at-Home Orders and Income Support on Differential Mobility of Poorer Regions: Essential vs Work-related Mobility



Source: authors' estimation using Google reports for workplace and grocery/pharmacy mobility, regional poverty rates (from national statistics or authors' estimations) and information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region's poverty rate is below (above) country median poverty rate. Stay-at-Home indicates the period in which national stay-at-home orders, either recommendations or requirements, are imposed. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when Stay-at-Home orders are lifted, following the first lockdown, are excluded. The estimation is based on the equation (2). For each (daily) point estimate, capped spikes indicate 95% CI based on standard errors clustered at country level. Following regional-level controls are included in the estimation: mobile internet access rate, regional population density (in log) and average regional score for trust in government (in log).

Additional Results: Robustness Checks on Methodological Choices. We test the sensitivity of our main results to the reweighting process (i.e. reweighting aimed to reflect country differences in the number of regions) and to the discretization of key variables in our model. Results are reported in Table 2.A4 with the baseline reproduced in column i. First, in columns ii-iii, we show that even without reweighting or with alternative reweighting schemes (i.e. with smaller weights than in the baseline), the pattern of results is similar to the baseline estimates: SHO combined with IS is associated with a smaller mobility gap between lower- and higher-poverty regions, than SHO only.

Table 2.A4: Robustness Checks on Methodological Choices

Dep. Var.: Mobility to Workplace	Region reweighting			Variable Discretization	
	Baseline	No reweighting	Alternative reweighting by $1/(\# \text{ of regions})^{0.5}$	Continuous SHO: Policy Stringency Index	Continuous Poverty: Standardized Poverty Rate
	(i)	(ii)	(iii)	(iv)	(v)
Poverty	0.574* (0.317)	0.647 (0.418)	0.659* (0.344)	-0.447 (0.673)	0.488** (0.186)
Poverty X SHO	5.167*** (1.166)	3.343*** (1.040)	4.164*** (1.075)	0.078*** (0.018)	3.341*** (0.589)
Poverty X SHO X IS	-3.320** (1.283)	-1.633* (0.964)	-2.357** (1.098)	-0.042** (0.016)	-1.217* (0.721)
R-squared	0.841	0.801	0.823	0.841	0.849
Observations	132,639	132,639	132,639	132,639	132,639
Country X Day FE	Yes	Yes	Yes	Yes	Yes
Region reweighting	Yes	No	Yes	Yes	Yes
Controls	No	No	No	No	No

Source: authors' estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors' estimations) and information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Poverty is defined as lower (higher) if region's poverty rate is below (above) country median poverty rate. Standardized poverty rate is poverty rate standardized with respect to country-specific mean and country-specific standard deviation. Stay-at-Home indicates the period in which national stay-at-home orders, either recommendations or requirements, are imposed. Policy stringency index indicates overall strictness of all containment measures put in place on a given day in 0-100 scale, with 100 being the most stringent containment policies. Income support indicates the period in which government provides income support to those who cannot work or lost their job due to the COVID-19 pandemic. Days when stay-at-home orders are lifted, following the first lockdown, are excluded. Regional control variables include mobile internet access rate, regional population density (in log) and average regional score for trust in government (in log). Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Standard errors clustered at country level in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1

For variable discretization, we use the Policy Stringency Index from OxCGRT (2020) as a continuous measure of SHO. It combines all COVID-19 containment policies into a single index measuring the stringency of these interventions (on a 0-100 scale, with 100 being the strictest policies). The estimates in column iv show that the presence of IS at each level of policy stringency may help to reduce poverty-induced gaps in mobility. Further, we apply a continuous measure of regional poverty, i.e. the regional poverty rate standardized with respect to country-specific mean and standard deviation, instead of the binary indicator of poverty used in the baseline estimations. We find that a higher regional poverty is associated with a larger mobility gap when SHO is enacted, and at the same time, the mobility-gap reduction effect of IS is stronger at a higher level of regional poverty (cf. column v).

Additional Results: Varying SHO Stringency. Regarding the different stringency levels of SHO, we use [OxCGRT \(2020\)](#)'s information on the degree of strictness of social distancing policies. The original variable differentiates four levels of SHO by increasing degree of strictness: (a) no stay-at-home orders, (b) recommended staying at home, (c) required staying at home with exceptions for “essential” trips, and (d) required staying at home with minimum exceptions. If we use a discrete variable reflecting these variations instead of the binary variable for SHO, the main finding is a smaller poverty gap in mobility when stringency is high, simply because people had to comply more due to police controls. This unreported result, available from the authors, is not very central and, hence, is not discussed in the main text.

Additional Results: Varying IS Intensity. Regarding the intensity of IS programs, [OxCGRT \(2020\)](#) reports only broad information on IS levels, with high/low IS corresponding to transfers that cover more/less than half of the earnings losses due to the pandemic. Only few countries are concerned by high IS programs: three of them have implemented high IS directly (Gabon, Honduras, Cambodia, Turkey) while two countries have first activated low-level IS then eventually raised the transfers to the high level (Chile, Uruguay). We could reproduce our estimations using ternary groups (no IS, low IS, high IS) rather than the binary IS variable. Yet, we would infer an intensive-margin effect mainly from the comparison of low-IS countries with high-IS countries. In a more robust way, we can compare country-day cells around the switch from low to high IS within a country. For Chile and Uruguay, the two countries in which this switch is observed, the expected pattern is indeed found: more mobility reduction is achieved when IS transfers become larger. Precisely, the poverty gap during period 1 (days with SHO but no IS) is 6.4**, the reduction of this gap due to *low* IS is -4.8 (insignificant) and the reduction due to *high* IS is -7.1*** (the sample size is only 18% of the initial sample). This result is mentioned in the main text and, even if concerning only two countries, supports our main interpretation that IS helps reduce the mobility poverty gap.

Additional Results: Heterogeneity across IS Policy Types. In the earlier version of this paper ([Aminjonov et al., 2021](#)), we explored the heterogeneity across different types of IS policies using [Gentilini et al. \(2020\)](#). It is important to know which types of social assistance program have been most effective in helping people to stay home. Yet we now refrain to pursue this investigation because the data – at least the version available at the time we wrote this paper – do not allow to conclude in a robust way. Indeed, with the data at hand, it is only possible to characterize whether a country has implemented horizontal expansions (increase in the coverage of existing programs or implementation of new schemes) or both horizontal and vertical expansions (the latter corresponding to an increase in value or duration of transfers for existing beneficiaries) *over the whole period*. It is not possible to know which specific policy option was used at a given point in time and hence use within-country time variation.

Appendix D: Alternative Approach: Global Poverty

We can choose an alternative definition of poverty, whereby higher- and lower-poverty regions are defined according to the global median of regional poverty rate. In this case, the model exploits time-varying changes in policy across regions of the world so that country-day fixed effects cannot be used anymore. This different perspective may be interesting for a sensitivity analysis of how the poverty gap in mobility changes with the introduction of IS. Yet, compared to the model described above and used in the main text, a two-way fixed effect estimator comparing regions globally is more vulnerable to the critiques made about DD approaches in the context of COVID-19 analyses (e.g. [Goodman-Bacon and Marcus, 2020](#)). In essence, the problem is when researchers attempt to compare the evolution of countries A (where a policy got implemented) and B (where it was not). Even if common trends in mobility are respected between A and B, they are not very informative about the way A may evolve – and evolve differently from B - in the absence of the policy. The same problem occurs here when using regions of the world.

The model is written as follows. Mobility in region i on day t is regressed as:

$$\begin{aligned} Mobility_{it} = & \alpha' + \delta SHO_{ct} + \beta' Poverty_i \times SHO_{ct} + \rho SHO_{ct} \times IS_{ct} + \\ & \gamma' Poverty_i \times SHO_{ct} \times IS_{ct} + \theta_t + \mu'_i + e_{it} \end{aligned} \quad (2.A4)$$

with SHO_{ct} and IS_{ct} the binary indicators of days with SHO and IS respectively. $Poverty_i$ is a binary measure of poverty indicating whether poverty rate in region i is above (higher poverty) the global median of regional poverty rates. The advantage of this formulation is that we can also measure the effect of SHO and IS in lower-poverty regions, i.e. δ and ρ respectively. Coefficients β' and γ' capture the additional effects of SHO and IS for higher-poverty region (the poverty gap in mobility). Day dummies θ_t capture flexible time trends that are common to all (for instance, global information on the pandemic at any point in time, specific announcements by the WHO regarding the virus or the use of masks, etc.). Since we compare regions globally, it might seem important to account for region FE, μ'_i . We do so but recall that these effects, identified on pre-pandemic days, are not very informative. In principle, one would need to control more explicitly for information on country/region heterogeneity (e.g. difference in local healthcare capacities, SHO enforcement capacities, etc.).

Estimation results corresponding to equation 2.A4 are reported in Table 2.A5. Column (i) shows basic estimates for all regions in the sample, with observations reweighted by the inverse of the number of regions per country (not to overweight regions with a large number of regions). For the estimates in column (ii), we additionally control for the lagged cumulative COVID-19 cases. This variable provides additional time variation in the behavioral responses to the local pandemic situation.²³ As noted above, the fact that we ignore country-day effects makes that

²³Indeed, the pure fear response to the spread of the virus is already captured to a large extent by the SHO variable, since lockdowns were enacted at the time of exponential chances in contamination. Consequently, it

here, we can identify absolute SHO/IS effects (the 1st and 3rd rows of the table) and not just the relative effect for the poorest.

Results are as follows. Mobility decreases by around 11.9 points in lower-poverty regions when SHO are introduced. Yet, the poverty gap in mobility is 8.2, implying that mobility decreases by only 3.7 points in the higher-poverty regions, i.e. a 69% smaller reduction compared to other regions. IS contributes to a reduced mobility by 3.7 points in regions with lower poverty incidence, and by an additional 3.6 points in higher-poverty regions, decreasing the poverty gap in mobility by that much. The remaining gap is small, i.e. around 4.6 points (8.2-3.6) or 29% of the total mobility reduction in lower-poverty regions. In these regions, the total mobility drop (11.9+3.7=15.6) in periods combining SHO and IS is essentially due to lockdowns (they account for 3/4 of the effect versus 1/4 for IS transfers) while in higher-poverty regions, the total mobility reduction (15.6-4.6=11) is rather on account of IS policies (2/3 of the effect). The way higher-poverty regions catch up in terms of mobility reduction thanks to IS would be even more pronounced in these regions – corresponding mainly to poor countries in Africa – if they had more resources to support living standards.²⁴

Table 2.A5: Mobility Effect of Stay-at-Home Orders, Income Support and Poverty

Dep. Var.: Mobility to Workplace	All countries		Africa	Latin America	Middle East & Asia
	(i)	(ii)	(iii)	(iv)	(v)
SHO	-11.935*** (0.872)	-11.894*** (0.874)	-2.339 (1.874)	-7.245*** (1.296)	-16.595*** (1.017)
SHO X Poverty	8.214*** (1.235)	8.206*** (1.233)	7.755*** (2.274)	1.310 (1.848)	5.487*** (1.507)
IS	-3.702*** (0.702)	-3.660*** (0.703)	-5.167*** (1.596)	-3.055** (1.418)	-3.098*** (1.003)
IS X Poverty	-3.636*** (0.904)	-3.628*** (0.902)	2.618 (1.850)	-2.924* (1.596)	-3.877*** (1.201)
R-squared	0.742	0.742	0.713	0.776	0.764
Observations	142,601	142,601	36,462	61,452	44,687
Day FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Region reweighting	Yes	Yes	Yes	Yes	Yes
Lagged COVID-19 information	No	Yes	Yes	Yes	Yes

Source: authors' estimation using Google reports for workplace mobility, regional poverty rates (from national statistics or authors' estimations as described in Table A1) and the information on COVID-19 policy response from OxCGRT for the period February 15-September 3, 2020. Stay-at-Home is a dummy indicating period in which national stay-at-home orders (recommendations or requirements) are imposed. Income support is a dummy indicating period in which any type of income support was provided in response to COVID-19 pandemic. Poverty is defined as lower (higher) if region's poverty rate is below (above) median poverty rate based on the sample of all 729 subnational regions across 43 countries (columns i and ii) or the sample of regions within the group of countries being considered (column iii+). Robustness checks include the lagged cumulative number of COVID-19 cases as control (the data from the European Centre for Disease Prevention and Control). Region reweighting: observations are weighted by 1 over the # of regions in the corresponding country. Standard errors clustered at region level in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1

is not surprising that this additional variable does not affect results much. Note that we use one-day lagged nationwide COVID-19 cumulative cases drawn from the European Center for Disease and Control (ECDC, cf. <https://www.ecdc.europa.eu>). Information on the count of cases at the regional level is not systematically available. Additional estimations on the subset of countries where this information is available lead to very similar findings. Results are also unchanged when we use the number of fatalities rather than the number of cases.

²⁴The social protection coverage in Africa is overall weaker: new transfers reach less than 10% of the population in a majority of countries, and according to the data by Gentilini et al. (2020), the overall expenses on emergency social protection are small (\$8.3 billion, i.e. 0.4% of the African GDP, against 1.2% GDP in Latin America in 2020).

The rest of Table 2.A5 shows results for different geographical areas. The reduction in mobility is generally low in Africa, which is probably due to higher poverty than on other continents and the nature of labor markets. Mobility reduction is larger in Latin America and much larger in Asian countries. This monotonic pattern is consistent with differences in poverty rates across these three continents (44.6%, 33.4% and 15.1% respectively), which supports our poverty interpretation also at the international level. Differences in mobility might additionally reveal, to some extent, differences in both the prevalence of COVID-19 and the stringency of local measures.

Regarding the poverty gap in mobility, it is measured here across regions within each continent (higher-poverty regions are defined according to the median of each continent). It is larger in Africa, partly because the gap in poverty itself is very large there (the average poverty rate in lower-poverty regions of the continent is 23.6% vs. 65.6% in higher-poverty regions). This result is consistent with what we find in the main text but the reason is different: in the baseline model, it is due to the fact that marked regional differences in poverty are also observed *within* African countries.

The change in perspective, i.e. from a model exploiting regional variability within countries to one that exploits both within and between-country variability, is more visible when we consider the impact of IS. Baseline results, using *within-country* dynamics, indicated that especially in African countries, transfers helped poorer regions more than less poor regions. Here, we see in Table 2.A5 that when comparing regions globally, IS programs are very effective in Latin America and Asia, i.e. they help the poorest regions reduce their mobility relatively more, but not in Africa. Again, this result reflects the fact that, to a large extent, we are now implicitly comparing the relative performance of countries. In Africa, the lower-poverty regions are mainly those from Botswana, Namibia, Cape Verde, Gabon, Egypt and South Africa, where IS programs are most effective than in poorer regions of the continent. In Latin America, the lower-poverty regions are those for instance from Brazil and Mexico, countries where populist presidents have denied the seriousness of the pandemic (Blofield et al., 2020).

Appendix E: Implications for the Spread of COVID-19

We study the implications of our results for the spread of the virus. That is, we provide back-of-the-envelope calculations of how the poverty gap in mobility during SHO, and the dampening effect of IS, reflect on the spread of COVID-19 through the mobility channel.

First, we calculate the poverty gap in mobility during SHO period, and the effect of IS, as the percentage deviations from mean mobility in the corresponding periods, using our baseline estimates from Table 2.1. We find that the gap of 5.5 mobility points corresponds to a 6.9% deviation from mean mobility (during SHO days without IS). Similarly, the effect of IS (-3.3 points) corresponds to a -4.4% deviation from mean mobility (during SHO with IS days).

In the second step, we estimate a mobility-elasticity of the growth in COVID-19 cases us-

ing country-level data on COVID-19 cases from the European Center for Disease and Control (ECDC). We calculate the upcoming growth rate of COVID-19 cases by comparing the daily cumulative number of cases to that of two weeks ahead and divide the rate of change by 14 to obtain a daily average growth rate (the two-week lag is used to account for the average known duration between infection and public report). Then, we regress the growth rate on country-level mobility index, day dummies, country fixed effects and additional controls, separately for SHO days without and with IS. Estimates yield elasticities of 1.2 and 0.8 respectively, that is, a 10% increase in mobility leads to 12% and 8% increases in the upcoming growth rate of COVID-19 cases respectively.²⁵

Finally, we combine both types of elasticities. For SHO days, we find a combined elasticity of 8.3 (6.9×1.2), i.e. the within-country poverty gap in mobility is associated with a 8.3% higher growth rate of COVID-19 cases. When IS is enacted on top of SHO, this elasticity is reduced by 3.5 percentage points (4.4×0.8) and drops to 4.8%. We can provide an illustration of the magnitude of these effects. According to official figures, the average number of cumulative cases in our sample of 43 countries passed the threshold of 100 around March 20, reaching 4,600 cases after two months (mid-May) and 256,000 cases after five months (mid-August). Based on our estimated elasticities, the poverty gap in mobility between lower- and higher-poverty regions within a country would be associated with around 132,000 additional cases after five months if SHO were implemented without IS. With IS schemes in place, this poverty gap in terms of virus spread would be reduced to 68,000 cases (i.e. 48% fewer additional cases). We also run similar estimations using a continuous measure of poverty and find that a one standard deviation higher regional poverty rate, within country, would be associated to 85,000 more cases during SHO periods without IS and to 55,000 additional cases only when IS operates (i.e. 35% fewer additional cases).

²⁵We calculate the elasticity as a one percent deviation from the mean mobility. That is, we first multiply estimates by the mean mobility and divide by the mean daily growth rate of COVID-19. Our mobility-elasticity of cases growth are of a comparable order of magnitude as the recent literature (e.g. Soucy et al., 2020).

Part II

Gender, Intra-household Inequality and Individual Poverty

Chapter 3

Global Evidence on Gender Gaps and Child Poverty in Consumption*

This chapter is based on a joint work with Olivier Bargain and Maira Colacce.

3.1 Introduction

Global poverty estimates typically rely on per-capita measures of household welfare. Despite offering significant methodological advantages, such as simplicity, transparency and enhanced cross-country comparability (Chen and Ravallion, 2010; Ferreira et al., 2016), this approach essentially fails to account for important elements that shape how poverty is experienced individually (Lanjouw and Ravallion, 1995; Deaton and Zaidi, 2002). First, it overlooks differences in needs among household members, often because detailed information on family composition, and in particular the age of dependent household members, is not known in a reliable way in survey data for poor countries or by administrations in charge of implementing social programs.¹ Then, and most importantly, the per-capita approach does not capture within-household inequality. This is problematic given the growing evidence that disparities within households account for a substantial share of overall inequality.² Thus, ignoring intra-household inequalities may significantly impair the assessment of countries' relative achievements in terms of child- or gender-specific poverty.

This paper suggests addressing these issues by estimating the intra-household distribution of

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¹In addition, the per-capita approach does not consider economies of scale in multi-person households while the latter tend to change patterns of global poverty (Jolliffe and Tetteh-Baah, 2022; Batana et al., 2013; Newhouse et al., 2016). This aspect is not treated in the present paper but discussed at length.

²The evidence relied first on nutritional data because it allows comparison of the caloric intakes of different household members with their (age- and gender-specific) requirements. Intra-household inequality in nutrition has been observed in Haddad and Kanbur (1990a,b), Haddad and Hoddinott (1994), Hoddinott and Skoufias (2004), D'Souza and Tandon (2019) and Brown et al. (2019) for specific countries.

resources for a large number of low- and middle-income countries (plus a few richer countries for Latin America). The main motivation is the following: while there is some evidence that women often get less than an equal share of household resources (World Bank, 2018), very few studies have systematically investigated gender inequalities in individual consumption across many countries. Similarly, children may not receive their fair share of household resources and there is no global mapping of child poverty based on what is actually accruing to them. With the per-capita approach, children appear twice poorer than adults (World Bank and Unicef, 2020) but this is simply because they disproportionately live in poor households (Chen and Ravallion, 2010), i.e. poor households tend to have more children. This mechanical child-adult poverty gap partly disappears when difference in needs is taken into account (Batana et al., 2013). Yet, remaining differences might reflect consumption inequality, especially in contexts where the trade-off between quantity and quality of children works to their disadvantage.

To provide global patterns of gender inequality and child poverty, we mobilize expenditure surveys for a large number of countries and apply recent techniques aimed to elicit intra-household resource sharing (Bargain and Donni, 2012; Dunbar et al., 2013). We start with the examination of a very large number of existing datasets and select those in which the necessary information can be found in a reliable way. The main data requirement is that surveys must contain not only total household expenditure but also information on goods consumed exclusively by men, women or children. For that, we rely on clothing as an 'assignable' good, since standard expenditure surveys commonly report clothing expenditure for males, females and children separately. With assignable goods and simple restrictions on individual preferences, the resource sharing function can be recovered and, hence, the total level of resources accruing to men, women and children respectively. With these requirements, we obtain 33 expenditure surveys, for mostly low- and middle-income countries. These are used to estimate intra-household consumption allocation for each country and provide in fine a global mapping of gender inequality and child poverty that is homogeneous and comparable across countries.

This paper makes several contributions. *First*, international comparisons of gender inequality and child poverty usually focus on broad indicators at household levels, i.e. using the per-capita approach. Our study is, we believe, an original attempt to measure gender consumption gaps and individual poverty more systematically on the basis of individual resources and at a large scale.³ *Second*, the rare studies that focus like us on women's and children's welfare within households are typically based on specific measures readily available at individual levels, such as nutrition.⁴ We are mainly aware of Brown et al. (2019), who illustrate intra-household inequality using nutrition data for more than 30 African countries.⁵ In contrast, we provide

³Note that Lechene et al. (2022) suggest an estimation of resource shares for 6 countries but mainly to illustrate an approach based on a linearized collective model.

⁴Undernutrition can be due to insufficient caloric and protein intake or from illness, hence often serving as a measure of individual deprivation (Steckel, 1995; Sahn and Younger, 2009; Brown et al., 2019). In what follows, we shall use nutrition proxies based on weight and height, namely wasting and stunting in children.

⁵Public health studies also suggest global comparisons of child nutrition (such as Bredenkamp et al., 2014). In addition, several multi-country studies are based on individual deprivation, mainly measured using human capital variables, for instance Alkire et al. (2019). In particular for gender gaps, these studies focus on health (Kennedy

an original international comparison of child- and women-specific levels of consumption poverty based on large-scale estimates of resource sharing, but nutrition is also used for cross-validation purposes.⁶ *Third*, the wide international range of living standards for which we report results allows us to comment on the relationship between intra-household inequality and individual poverty across economies at different stages of development. We contribute to the discussion initiated by Jayachandran (2015) about whether intra-household inequality narrows as countries grow or whether it is the case that many countries that are poor today happen to have cultural norms that exacerbate favoritism toward males.⁷ We also provide interpretations of the patterns obtained in relation to demographic trends and a broader set of household decisions associated with intra-household inequality – namely fertility decisions and the child quantity-quality trade-off. *Fourth*, this paper provides a data-intensive contribution and makes an extensive use of the recent methods to identify and estimate resource sharing. In a way, this data work helps to assess how frequently, among all the expenditure surveys available for poor and middle-income countries, one can operationalize a collective model of consumption identified using assignable goods. *Finally*, we provide a cross-validation based on undernutrition as an alternative individual poverty index. Note that collective models seem to be the only available solution so far to evaluate intra-household consumption inequality (and individual poverty) broadly.⁸ However, we keep a critical eye since these models have rarely been validated. Thus, we interpret our results with caution, but also contribute to the recent validation effort. This endeavor has involved comparing resource share estimates with actual resource shares in rare surveys when consumption is fully individualized (see Bargain, Lacroix and Tiberti, 2022), or with nutrition data (as in Brown et al., 2021). We generalize the latter approach and suggest cross-validation with nutrition data at country levels and, when undernutrition measures are available in the surveys used, at the micro level.

The main results are as follows. Our estimates of intra-household distribution of resources show a frequent pattern of gender inequality within households, with women getting a significantly smaller of resources than men in a majority of countries and on average (-18%). Children’s resources tend to increase with living standards: this is shown to be consistent with a unfavorable

et al., 2020), education (with their consequences on macroeconomic performances, see Klasen and Lamanna, 2009) or on multidimensional indices combining health, education and other variables such as final say measures (see Alkire and Foster, 2011 and Klasen and Lahoti, 2021 for instance). These are important dimensions but quite different from monetary measures of individual poverty as derived here.

⁶Note that we focus on women’s and children’s intra-household *final* outcomes, not on their control over income and wealth sources through labor market opportunities or legal institutions. Regarding the control of resources within households, some international comparisons exist that use subjective information on decision power (see World Bank, 2012 or Alkire et al., 2013), as drawn from surveys such as the Demographic and Health Surveys (DHS). Yet, these empowerment proxies may reflect delegation more than power (see Baland and Ziparo, 2018) and, most importantly, do not allow quantifying individual poverty as we do.

⁷While this discussion is relatively focused on gender gaps, our results indicate more of cumulated effect of both factors regarding inequality between children and adults.

⁸Indeed individualized consumption is very rarely found in survey data, as it is costly and difficult to collect, hence rare. Among exceptions, individualized household expenditure is used in De Vreyer and Lambert (2021), Lambert et al. (2014), Bargain, Lacroix and Tiberti (2022) while individualized food expenditure is exploited in Brown et al. (2021). The full or partial observation of how resources are shared, a unique feature of these datasets, can actually be used for a comparison with estimated shares and, hence, provide a validation of the approach, as discussed below.

child quality-quantity trade-off in the poorest countries, i.e. households have more children and invest less in each of them. Translated in individual poverty terms, these results mean that child poverty declines more slowly than adult poverty as living standards begin to rise, so that the child-adult poverty gap is largest for countries in intermediate positions in our sample. An important aspect is that estimated child shares reflect both potential intra-household inequality and difference in needs between children and adults. Sensitivity analyses using alternative levels of differential needs tend to indicate that children do not receive their fair share in many poor countries of our sample - children's needs would have to be as low as 30 percent of adult needs for child and adult poverty to coincide.⁹ Finally, our cross-validation shows how child undernutrition and child poverty in consumption correlate across living standards. It also emphasizes the fact that child undernutrition is not only due to low consumption levels but also to the degree of intra-household inequality.

The rest of the paper is structured as follows. Section 2 describes the empirical strategy and the data used in the estimation of intra-household resource allocation. Section 3 presents our main results on resource shares and individual poverty. Section 4 provides the cross-validation of estimated resource shares and nutrition measures for children. Section 5 concludes.

3.2 Empirical Strategy

3.2.1 Identification of Individual Resource Shares

Collective Models and Resource Sharing. Our suggested approach belongs to the broad family of collective household models. These models, unlike the unitary approach, account for the bargaining process underlying household decisions (Chiappori and Bourguignon, 1992) and ultimately allow recovering the intra-household allocation of resources (Browning et al., 2013). Originally, this approach assumes that households make efficient decisions (Chiappori, 1992), which justifies the decentralization of the household decision process leading to a sharing rule interpretation (i.e. a direct application of the Second Welfare Theorem). That is, household decisions are as if total resources were first shared among household members and then each individual decides about her consumption bundle based on her resources and preferences. Recent studies have suggested a tractable and transparent framework to identify the allocation process using household-level consumption data, assignable goods and preference restrictions. Seminal contributions (Browning et al., 2013; Lewbel and Pendakur, 2008) have established the general framework for identifying resource sharing in childless couples that was then extended to the case of households with children (Bargain and Donni, 2012; Dunbar et al., 2013; Bargain, Donni and Hentati, 2022).

⁹Note that remaining within-household disparities are interpreted as inequality but could partly relate to 'lifeboat ethics' mechanisms, i.e. situations where breadwinners adults are prioritized in terms of nutrition to be able to generate resources for the other family members (Pitt et al., 1990).

Sharing Rule and Notations. As in related studies, we start by assuming that a sharing rule governs the distribution of resources in the household. Note that our set-up does not necessarily require the efficiency assumption, which remains debatable, especially in the context of poor countries (see [Baland and Ziparo 2018](#)).¹⁰ We only need to assume that total expenditure is shared among household members according to some rule, which we identify and estimate. Let us now introduce some notations. Denote x the log of total private expenditure and $\eta_{i,s}(z^r)$ the share of total private expenditure $\exp(x)$ accruing to each type of individuals $i = m, f, c$, i.e. men, women, and children, in a household of composition s . Household composition corresponds to the number of individuals in each of the three groups, which are denoted by s_m, s_f and s_c , respectively, and are stacked in the vector $s = (s_m, s_f, s_c)$. Resource shares depend on several factors in vector z^r including household demographic characteristics. We ignore dependence on price variation as our set-up is static so that all the households of a given country at a point in time are assumed to face the same price vector ([Lewbel and Pendakur, 2008](#)). With the sharing rule interpretation, each household member of type i in a family of composition s receives her own private resources $x_{i,s} = x + \ln \eta_{i,s} - \ln s_i$, written in log terms, which we later use to calculate individual poverty. From this expression, we make explicit the fact that we identify only the total resource share of each person type $i = m, f, c$, i.e. we do not retrieve the shares of specific individuals (e.g. girls) within a type (e.g. children). This is merely a data limitation but it means that we will slightly underestimate the scope of intra-household inequality.¹¹ This is not really an impediment as we focus mainly on gender gaps among adults and on overall child poverty. Moreover, by including the proportion of boys (among all children) in z^r , we can capture whether the resource shares for children are biased in favor of boys, i.e. a gender gap among children (see [Dunbar et al., 2013](#); [Bargain et al., 2018](#)).

Structural Engel Curves at Individual and Household Levels. We opt for a semi-parametric identification as in [Dunbar et al. \(2013\)](#). Assuming Piglog indirect utility functions for each individual (see [Deaton and Muellbauer 1980b](#)), we obtain *individual* Engel curves that are linear in the logarithm of individual resources. That is, the *individual budget share* for a good k consumed by individuals of type i in households of type s is written:

$$w_{i,s}^k = \alpha_{i,s}(z^p) + \beta_{i,s}(z^p) \cdot x_{i,s}(z^r), \quad (3.1)$$

with preference shifters z^p and sharing rule determinants z^r . The key data requirement for the identification of resource shares is the presence of exclusive goods, i.e. goods consumed only by one specific demographic group (ex: tobacco for adults), or assignable goods (ex: clothing, the consumption of which can often be distinguished between men, women and children). We index these exclusive/assignable goods k_m, k_f, k_c for men, women, and children, respectively. For

¹⁰Because of the collective model literature, the efficiency paradigm is the most commonly accepted way to justify decentralization, but probably not the only one that could explain a sharing process. [Lewbel and Pendakur \(2022\)](#) illustrate that a departure from efficiency leads to a relatively small variation in the resource share estimations

¹¹To derive resource sharing among siblings or among women of different age groups, one would need goods that are exclusive to these sub-groups of persons (for boys vs. girls, or young adult women vs. older adult women).

instance, if k_f corresponds to women's clothing, $w_{f,s}^{k_f}$ is the proportion of women's resources, $\exp(x_{f,s})$ that they spend on their own clothing. From the structure placed on individual demand in equation (3.1), we can derive *household* Engel curves. For example, in a nuclear household, if we multiply $w_{f,s}^{k_f}$ by $\eta_{f,s} = \exp(x_{f,s})/\exp(x_s)$, we obtain the level of spending on the woman's clothing as a fraction of total expenditure, i.e., the *family budget share* on that good, denoted $W_s^{k_f}$. If there are several adult women in the family, the latter represents the clothing share for all the women in the household. Thus, we can write a system of household budget shares for exclusive goods k_i , $i = m, f, c$:

$$\begin{aligned} W_s^{k_m} &= \eta_{m,s}(z^r) \cdot (\alpha_{m,s}(z^p) + \beta_{m,s}(z^p) \cdot (x + \ln \eta_{m,s}(z^r) - \ln s_m)) \\ W_s^{k_f} &= \eta_{f,s}(z^r) \cdot (\alpha_{f,s}(z^p) + \beta_{f,s}(z^p) \cdot (x + \ln \eta_{f,s}(z^r) - \ln s_f)) \\ W_s^{k_c} &= \eta_{c,s}(z^r) \cdot (\alpha_{c,s}(z^p) + \beta_{c,s}(z^p) \cdot (x + \ln \eta_{c,s}(z^r) - \ln s_c)) \end{aligned} \quad (3.2)$$

where the left-hand terms are observed.

Restrictions and Identification. We need to retrieve the key elements from the estimation of a reduced form of the above system, i.e., from the estimation of family budget shares of the exclusive good expenditure on log expenditure. Note that the men's resource share can be written as the residual to one of the women's and children's shares, i.e. $\eta_{m,s} = 1 - \eta_{f,s} - \eta_{c,s}$, and is automatically recovered once women's and children's shares are. Hence, the derivatives with respect to log expenditure of the system above yield:

$$\begin{aligned} \partial W_s^{k_f} / \partial x &= \eta_{f,s}(z^r) \cdot \beta_{f,s}(z^p) \\ \partial W_s^{k_c} / \partial x &= \eta_{c,s}(z^r) \cdot \beta_{c,s}(z^p) \\ \partial W_s^{k_m} / \partial x &= (1 - \eta_{f,s}(z^r) - \eta_{c,s}(z^r)) \cdot \beta_{m,s}(z^p) \end{aligned} \quad (3.3)$$

for each s out of a total of S different household types (family compositions). The left-hand derivatives are observed, at least when household Engel curves are not flat, which is an applicability condition that we check in the empirical analysis. The system above have $3S$ equations and $5S$ unknowns ($\eta_{f,s}, \eta_{c,s}, \beta_{m,s}, \beta_{f,s}$ and $\beta_{c,s}$ for each s). Here the identification of resource shares requires additional restrictions on the preference terms $\beta_{i,s}$. We rely on the Similarity Across People (SAP) assumption suggested by (Dunbar et al., 2013), which states that for exclusive goods, the shape of individual Engel curves is similar across person types $i = m, f, c$ of a given household type s . Formally, SAP is written as: $\beta_{m,s} = \beta_{f,s} = \beta_{c,s} = \beta_s$ for each s . It yields $3S$ unknowns in total ($\eta_{f,s}, \eta_{c,s}$ and β_s for each s) and, hence, an exact identification. Note that SAP is a commonly used preference restriction in the demand literature and a weaker version of shape-invariance defined by Lewbel (2010). It has been tested in recent studies. Using direct observations of resource shares in microdata for Bangladesh, Bargain, Lacroix and Tiberti (2022) tend not to reject SAP for clothing. Other tests hinge on indirect methods, i.e. start from alternative identification approaches that do not require SAP and test it as a restriction. This is notably the case using identification based on distribution factors in Dunbar

et al. (2021) and Brown et al. (2021), with a relative support for SAP using data from Malawi and Bangladesh respectively.

Specification and Estimation Method. The semi-parametric approach provides the log-linear specification of Engel curves derived from Piglog preferences, as written in equation (3.1). We model resource shares using logistic functions to guarantee that the shares are in range (0, 1) and sum up to 1. To estimate the model, we add error terms to household Engel curves for men’s, women’s, and children’s exclusive goods in the demand system (3.2) and impose the SAP condition. That is, we estimate the following system:¹²

$$\begin{aligned} W_s^{km} &= \eta_{m,s}(z^r) \cdot (\alpha_{m,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{m,s}(z^r) - \ln s_m)) + \epsilon_{m,s} \\ W_s^{kf} &= \eta_{f,s}(z^r) \cdot (\alpha_{f,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{f,s}(z^r) - \ln s_f)) + \epsilon_{f,s} \\ W_s^{kc} &= \eta_{c,s}(z^r) \cdot (\alpha_{c,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{c,s}(z^r) - \ln s_c)) + \epsilon_{c,s} \end{aligned} \quad (3.4)$$

with

$$\begin{aligned} \eta_{f,s} &= \exp(\gamma_f z^r) / D, \quad \eta_{c,s} = \exp(\gamma_c z^r) / D, \quad \eta_{m,s} = 1 / D \\ \text{and } D &= 1 + \exp(\gamma_f z^r) + \exp(\gamma_c z^r). \end{aligned}$$

To maximize the number of countries in our analysis and for the sake of comparability, we have kept the specification of the model parsimonious. Engel curve parameters $\alpha(z^p)$ and $\beta(z^p)$ vary with preference shifters z^p that include household composition (namely s_m, s_f, s_c) and a urban dummy. For the sharing rule, we specify the logistic form with a set z^r of variables equivalent to z^p plus other demographic characteristics, namely the average age of each person type and the proportion of boys among the children. We systematically apply this specification, with the same set of variables, across all countries in our database, with few exceptions.¹³

3.2.2 Household Expenditure Data and Key Variables

Country and Data Selection. As mentioned in the introduction, the selection of countries in our sample is exclusively based on the availability of household expenditure surveys that allow the estimation of the model described above. At the beginning of this project, we identified expenditure surveys for 56 low- and middle-income countries, plus a few richer countries in Latin America, for which the microdata and data documentation were accessible.¹⁴ A key element for

¹²Since the error terms of the model are likely to be correlated across equations, each system is estimated using Non-Linear Seemingly Unrelated Regressions. Details about the estimation procedure are explained in the Appendix.

¹³Specifically, for Argentina, Chile and Panama, the urban dummy is not included as the surveys are implemented only in urban areas.

¹⁴More countries may be available now so it is therefore possible to broaden the scope of analysis in future updates. We believe that our conclusions are nonetheless robustness, at least for international patterns for Africa and Latin America.

the identification of individual resource shares is the availability of expenditure data on goods that are exclusive to men, women, and children – specifically, the assignability of clothing expenditure, as further discussed below. We also need a parsimonious set of demographics (household composition, the age of household members and the urban/rural location). For each country, we choose the most recent year that satisfies the above criteria.¹⁵ It turns out that 12 countries do not have assignable clothing expenditure,¹⁶ an additional 9 have missing information,¹⁷ and 2 countries are excluded because of the poor quality of expenditure data (e.g., a high proportion of infrequency of clothing purchases). This leaves us with a final sample of 33 countries, mostly low- and middle-income countries (59% of the initial set), listed in Table 3.A1 with the year and the name of survey data. This data assessment and country selection demonstrate how often one can replicate resource-sharing estimations based on collective models identified using exclusive/assignable goods for low- and middle-income countries. Note that a majority of the surveys are LSMS while many others were collected nationally but often in a LSMS style. Global regions with better representation in our sample are Latin America, with 11 countries which represent 78% of the total population of this region, and Africa, with 16 countries representing 52% of the African population. We also consider four Asian and two middle-income European countries. The survey years range from 2002 to 2019, with 23 surveys dated after 2014 (70%).

Expenditure Data. All the surveys used in our analysis contain the required information on household consumption and demographic characteristics, as noted above. Specifically, we can construct total household expenditure, which aggregates spending on food and non-food items. We can also retrieve assignable goods, namely male, female and child clothing. The choice of clothing for the resource share identification is primarily practical. The set of exclusive goods available in standard surveys is extremely limited. A few types of exclusive goods have been suggested in the literature, most often some adult goods used to retrieve the cost of children with the Rothbarth approach. Adult goods comprise alcohol and tobacco for instance, which pose problems of misreporting (Deaton, 1997). Also, these goods generally do not allow distinguishing men and women. Children’s, men’s, and women’s clothing expenditures are often reported separately so that they can be used as exclusive goods. As seen above, assignable clothing is frequent in standard expenditure surveys (44 out of 56 countries in our selection). Moreover, contrary to other goods, clothing is not necessarily subject to large consumption externalities. For these reasons, this good has been extensively used to retrieve child resources (the cost of children) using the Rothbarth approach (Deaton, 1997), to test efficiency in early collective models of consumption (e.g., Browning et al., 1994 or Bourguignon et al., 2009, and Browning et al. 2014 for a survey) or to identify resource sharing in recent approaches (e.g. Browning et al., 2013, Bargain and Donni, 2012, Dunbar et al., 2013). The use of clothing for resource share identification is also supported by recent validation tests (Bargain, Lacroix and

¹⁵Note that further work could look at dynamic patterns, i.e. at time variation in resource shares, for the few countries for which older expenditure surveys are available.

¹⁶For 2 of them, adult and child clothing is reported but not the age cutoff above which an adolescent is defined as an adult regarding child vs. adult clothing.

¹⁷Total expenditure could not be recovered or key demographic variables are missing.

Tiberti, 2022).

Sample Selection. Our sample selection within each country follows the same logic as above and aims to pursue the individual poverty analysis in the broadest possible way. Hence, we impose as few exclusions as possible in each country and simply discard households for whom basic information, i.e. expenditure and demographics, is missing. To reduce measurement errors, we also eliminate the few observations corresponding to outliers in terms of total household expenditure and budget shares for clothing. As in recent contributions (Calvi, 2020; Bargain, Lacroix and Tiberti, 2022; Brown et al., 2021), we consider all types of households with at least a man, a woman and a child since the objective is to discuss the intra-household distribution between these three demographic groups and the implications in terms of individual poverty for each of them. As a result, samples include nuclear families but also more complex households with several adults of the same gender, which is often the case in poor countries. Our final sample size for each country is presented in Table 3.A1.

Summary Statistics. Table 3.A2 reports the mean and standard deviation of the demographic variables used in the estimation of the structural model, focusing on all types of households consisting of men, women, and children. The number of children in the household varies widely across countries, reflecting the different stages of demographic transition that countries are in. For instance, Latin American and European countries, Namibia, Mongolia, and South Africa have, on average, two children per household, while households in all other countries have an average of three or more children per household. As we shall see, this heterogeneity in fertility is likely to be an important correlate of country differences in distribution to children. The average number of men and women reflects the inclusion of non-nuclear households in the analysis. As expected, poorer countries, mainly in Africa, are also more rural. Table 3.A3 reports statistics on household expenditure. Annual total household expenditure ranges from just over 3,000 dollars (2011 PPP) in Malawi to almost 31,500 dollars in Chile, illustrating the wide range of living standards in our sample of countries. This allows us to discuss the relationship between intra-household inequality and individual poverty across different stages of economic development. In almost all countries, households systematically tend to spend, on average, 1-5% of household budget on clothing of each demographic type. The infrequency of clothing purchases is not an issue (see Dunbar et al. 2013). The proportion of households with zero clothing consumption is within reasonable bounds for all countries. Cross-country differences may be explained by data collection strategies (in particular, zero clothing expenditures are more frequent when the duration of the recall period for clothing expenditures is short, as seen in Table 3.A1).

3.3 Results

3.3.1 Correlates of the Sharing Rule

We first report the marginal effects of some of the relevant covariates on per-child and per-woman resource shares in Tables 3.A4 and 3.A5 respectively, namely the average age of person types,

the proportion of boys and being urban. The results are in line, and generalize, the findings of related studies (for instance [Dunbar et al., 2013](#) and [Penglase, 2021](#) for Malawi, [Bargain et al., 2014](#) for Ivory Coast or [Brown et al., 2021](#) and [Bargain, Lacroix and Tiberti, 2022](#) for Bangladesh). In particular, we find no systematic evidence of gender gaps between siblings: there is a significantly positive association between child shares and the proportion of boys in only 7 countries (and an opposite pattern in 3 countries). Note in particular that a significant gender gap among children is found in only 3 of the 16 African countries in our sample (Burkina Faso, Gambia and Kenya): such a limited gender bias in Africa is in line with past evidence ([Haddad and Hoddinott, 1994](#); [Bargain et al., 2014](#)).¹⁸ Children receive more resources in urban households but the pattern is not very marked (living in urban area is significantly associated with larger child shares in 7 countries while an opposite result appears in 3 countries). Relatively older women, with respect to men in the household, tend to receive less in a majority of countries but there are exceptions (Brazil, Ecuador, Paraguay, Sierra Leone). Note that the age-resource pattern does not need to be linear.¹⁹ Notice that women tend to receive less resources when children are mainly composed of boys. Other (unreported) estimates show that child shares increase with the number of children but at a decreasing rate, a pattern also found in related studies ([Bargain et al., 2014](#); [Dunbar et al., 2013](#)). There are several explanations among which the classic quality-quantity trade-off, which we explore through international comparisons later on, and the possibility of scale economies among siblings (as originally studied in [Calvi et al. 2023](#)).

3.3.2 Patterns of Intra-household Inequality

These estimations allow us to predict average individual resource shares and their standard errors for each country using sample mean covariates. Results are presented in [Table 3.1](#). In column (1) we provide a pre-test of the method applicability, i.e. we verify that the estimated slope β of clothing Engel curves is statistically different from zero for a large majority of households in each country.²⁰ Columns (2)-(4) report average per-person resource shares for men, women and children respectively. Overall, we find a frequent pattern of intra-household inequality in resource allocation: men tend to receive larger shares of resources than women, while children’s resource shares are considerably smaller than adults’. Each man consumes between 20-46% of household resources on average (with a global average of 30.3%) while women receive between 17-33% (a global average of 24.7%), and children between 3-22% (a global average of 8.4%). Resource shares for children partly reflect differences in needs and cannot directly be interpreted as inequality – we discuss this point extensively below. Focusing on the adult gender

¹⁸It is interpreted by [Deaton \(1997\)](#) as due to the relatively high rate of economically productive women in many African countries (so that girls are not seen as a burden by their parents).

¹⁹For Southern Asia, there is evidence that women’s health status deteriorates when they get older ([Anderson and Ray, 2010](#); [Calvi, 2020](#)). On the contrary, being relatively younger can be associated with early marriages and lower women’s empowerment ([Cameron et al., 2022](#)). Several authors have recognized that women’s power can actually increase over the life course ([Gupta, 1995](#)) while for Ghana, we show a U-shape pattern in past results ([Aminjonov et al., 2023](#)).

²⁰Recall that zero slopes, i.e. flat Engel curve, would prevent the identification of resource shares ([Dunbar et al., 2013](#)).

gap, we see that men get a larger share of household resources than women in most countries but the gap is statistically significant in only 17 countries. There are a few exceptions where the gender gap is reversed (it is significant in Mexico and Panama, and insignificant otherwise, for instance in Bulgaria). The average gender gap represents almost 6 points of percentage globally, as detailed in column (5) of Table 3.1, which means that women get on average 18.4% less than men. Similar results are found when using mean resource shares over all households in each country rather than predicted resource shares at sample means.

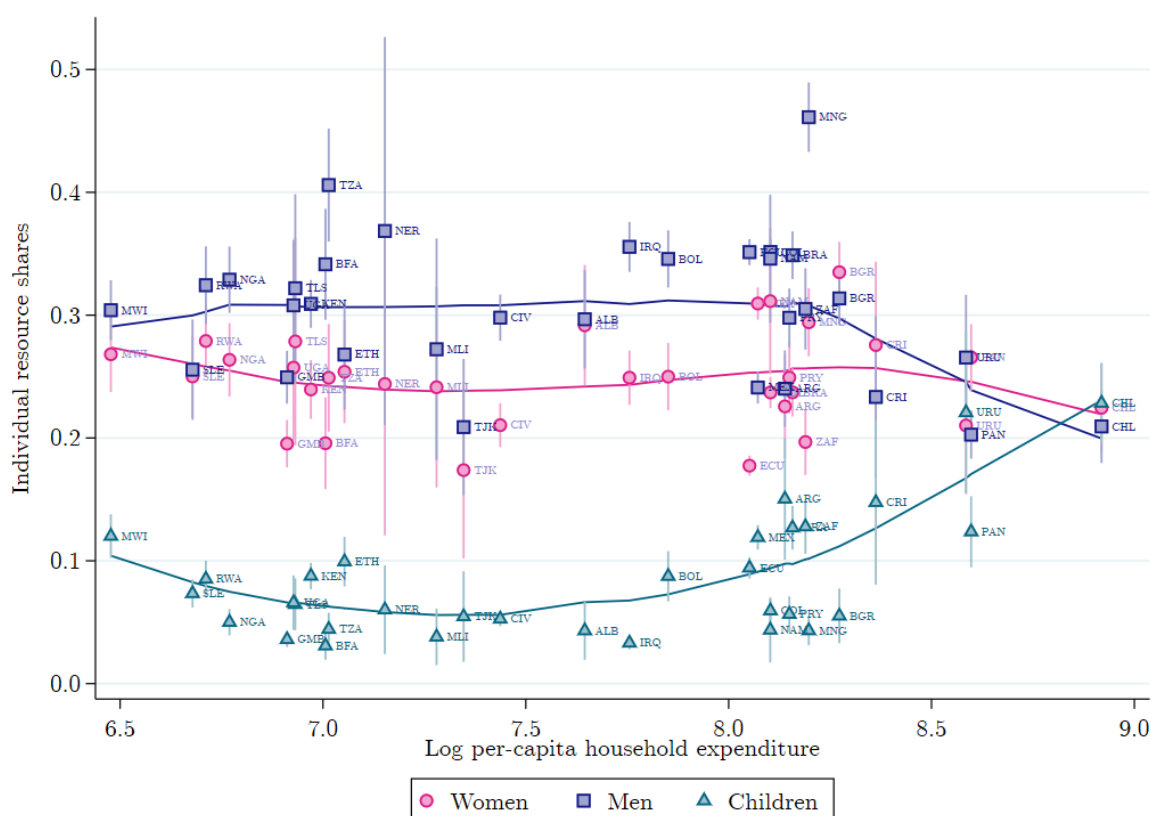
This global assessment of the gender gap in consumption based on resource-sharing estimates is, we believe, the first one at such a large scale in the literature. It is also consistent with existing evidence for single countries or specific groups of countries, as summarized in the review of Table 3.A6. Note that this review is merely indicative, since several studies cannot readily be compared to ours as they focus on nuclear households only. Yet, the literature converges on salient features, namely a frequent gender gap in consumption but also some exceptions such as Mexico and Bulgaria. For Mexico, past analyses point to a relatively large per-woman share, ranging from 0.29 to 0.38 across studies for nuclear household and 0.34 for complex households in Calvi et al. (2023). For Bulgaria, a reversed gap is also found in Lechene et al. (2022). Results for children are discussed below in relation to individual poverty, but we can stress here that our findings are in line with past estimates reported in Table 3.A6. For instance, we reach similar conclusions regarding salient results such as the very small level of resources accruing to children in Iraq, as reported in Lechene et al. (2022).

Table 3.1: Average per-person resource shares: men, women, children

Country	Year	% of households with non-flat Engel curve	Resource shares at mean			Gender gap at mean
			Per man	Per woman	Per child	
		(1)	(2)	(3)	(4)	(5)
Albania	2005	0.90	0.297 (0.020)	0.292 (0.025)	0.043 (0.012)	0.005 (0.043)
Argentina	2018	1.00	0.240 (0.016)	0.226 (0.021)	0.150 (0.025)	0.014 (0.029)
Bolivia	2019	1.00	0.346 (0.012)	0.250 (0.014)	0.087 (0.010)	0.096*** (0.022)
Brazil	2017	1.00	0.349 (0.010)	0.237 (0.010)	0.127 (0.009)	0.112*** (0.017)
Bulgaria	2007	1.00	0.314 (0.011)	0.335 (0.013)	0.055 (0.011)	-0.021 (0.021)
Burkina Faso	2014	1.00	0.341 (0.023)	0.196 (0.019)	0.031 (0.006)	0.146*** (0.040)
Chile	2017	0.91	0.209 (0.015)	0.225 (0.018)	0.228 (0.017)	-0.015 (0.027)
Colombia	2017	1.00	0.351 (0.005)	0.237 (0.006)	0.059 (0.005)	0.114*** (0.011)
Costa Rica	2018	0.87	0.233 (0.034)	0.276 (0.035)	0.148 (0.034)	-0.042 (0.058)
Cote d'Ivoire	2002	1.00	0.298 (0.010)	0.210 (0.009)	0.053 (0.003)	0.088*** (0.017)
Ecuador	2011	0.99	0.351 (0.005)	0.177 (0.004)	0.094 (0.004)	0.174*** (0.008)
Ethiopia	2015	0.78	0.268 (0.023)	0.254 (0.021)	0.099 (0.010)	0.014 (0.038)
Gambia	2015	0.90	0.249 (0.011)	0.195 (0.010)	0.036 (0.003)	0.054*** (0.019)
Ghana	2017	1.00	0.273 (0.014)	0.226 (0.014)	0.054 (0.009)	0.047* (0.024)
Iraq	2012	1.00	0.356 (0.010)	0.249 (0.011)	0.033 (0.003)	0.106*** (0.021)
Kenya	2015	1.00	0.309 (0.010)	0.239 (0.012)	0.088 (0.005)	0.070*** (0.018)
Malawi	2016	1.00	0.304 (0.012)	0.268 (0.016)	0.120 (0.009)	0.036* (0.020)
Mali	2014	0.80	0.272 (0.046)	0.241 (0.042)	0.038 (0.012)	0.031 (0.084)
Mexico	2018	1.00	0.241 (0.007)	0.309 (0.007)	0.119 (0.005)	-0.068*** (0.011)
Mongolia	2016	1.00	0.461 (0.014)	0.294 (0.014)	0.043 (0.006)	0.167*** (0.027)
Namibia	2015	1.00	0.346 (0.027)	0.311 (0.030)	0.044 (0.013)	0.035 (0.053)
Niger	2014	0.80	0.368 (0.081)	0.244 (0.063)	0.060 (0.018)	0.125 (0.135)
Nigeria	2019	0.98	0.329 (0.014)	0.264 (0.015)	0.050 (0.005)	0.065** (0.026)
Panama	2008	1.00	0.203 (0.010)	0.266 (0.014)	0.124 (0.015)	-0.063*** (0.019)
Paraguay	2011	1.00	0.298 (0.012)	0.249 (0.015)	0.056 (0.007)	0.049* (0.026)
Rwanda	2016	1.00	0.324 (0.016)	0.279 (0.015)	0.085 (0.008)	0.045* (0.026)
Sierra Leona	2011	1.00	0.256 (0.021)	0.250 (0.017)	0.073 (0.006)	0.006 (0.036)
South Africa	2014	0.86	0.305 (0.017)	0.197 (0.014)	0.128 (0.011)	0.108*** (0.027)
Tajikistan	2009	1.00	0.209 (0.028)	0.174 (0.037)	0.055 (0.019)	0.035 (0.061)
Tanzania	2014	0.97	0.406 (0.023)	0.249 (0.022)	0.044 (0.007)	0.157*** (0.043)
Timor Leste	2007	0.79	0.322 (0.039)	0.279 (0.043)	0.065 (0.011)	0.043 (0.079)
Uganda	2015	0.90	0.308 (0.027)	0.257 (0.030)	0.066 (0.011)	0.051 (0.051)
Uruguay	2016	0.81	0.265 (0.026)	0.210 (0.028)	0.221 (0.034)	0.055 (0.041)
# countries with significantly positive gender gaps						17
# countries with significantly negative gender gaps						2
International means			0.303	0.247	0.084	0.056

Source: authors' estimations using the data from household expenditure surveys for 33 countries. Notes: Table reports the per-man, per-woman, per-child resource shares and gender gap in resource shares predicted using *country-level mean household characteristics*. Mean household characteristics are based on the sample of households with men, women and children. Per-person shares do not add up to one due to the different number of members of each demographic group within households. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Figure 3.1: Individual resource shares by living standards (shares calculated at sample means)

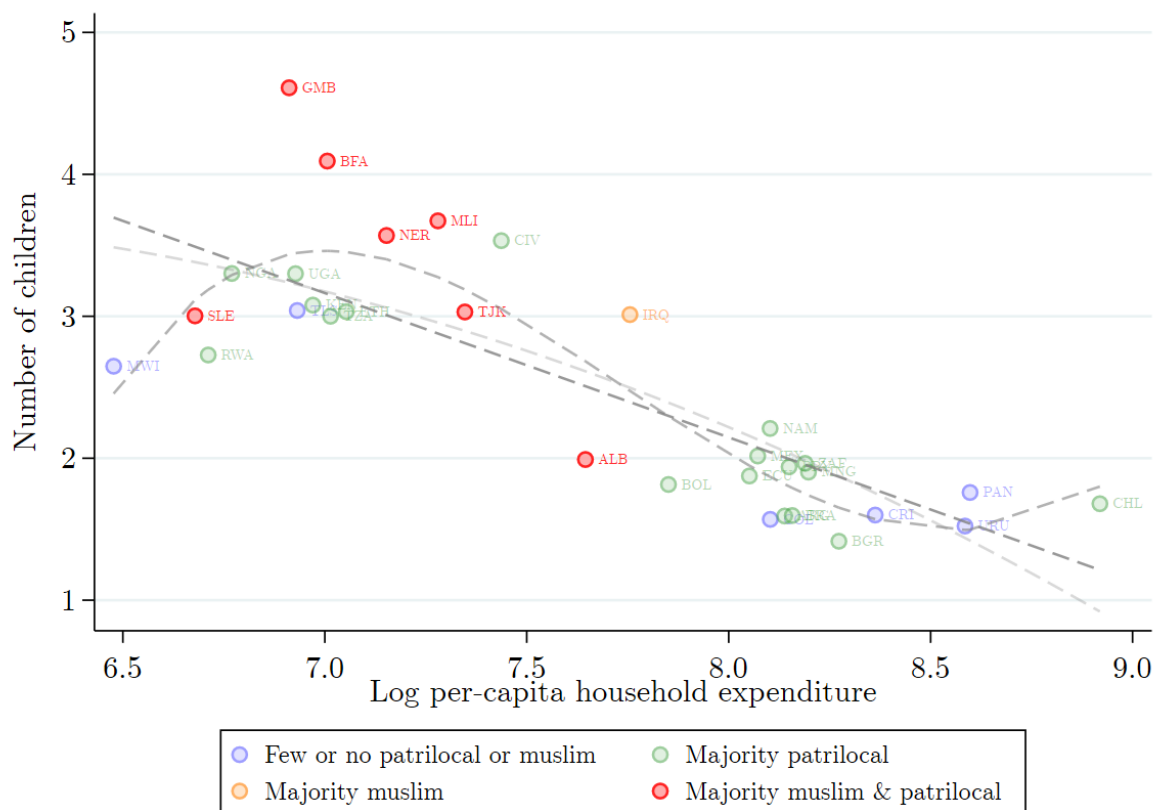


Source: authors' estimations using the data from household expenditure surveys of 33 countries. Notes: Figure illustrates per-mann, per-woman, and per-child resource shares predicted using *country-level mean household characteristics* by log total household expenditure. Mean household characteristics are based on the sample of households with men, women and children. Per-person shares do not add up to one due to the different number of members of each demographic group within households. Spike lines represent 95% CI. Smooth lines are based on locally weighted regressions of resource shares on log household expenditure.

Despite this general trend, the dispersion of estimated resource shares across countries is high, which could possibly be explained by the degree of development, or its correlates (e.g. gender roles, cultural norms, democracy etc.). Bringing country estimates together, we can visualize the extent of intra-household consumption inequality by levels of living standards in Figure 3.1. Precisely, we depict country resource shares, based on estimated shares evaluated at country sample means, against country per-capita household expenditure expressed in log. Overall, we find that intra-household inequality tends to increase with living standards. More specifically, gender inequality is prevalent, as noted before, but tends to disappear at higher development stages. More pronounced is the adult-child gap: child shares are much lower in poor countries and the gap tends to vanish for the high living standards in our sample. Note that Figure 3.1 is based on country-level resource shares predicted at the sample mean characteristics for each country. Alternatively, appendix Figure 3.A1 shows country resource shares calculated as the average predictions over all households in each country, i.e. using the full variation in household covariates. While these two approaches could yield different results, given the non-linearity of resource share functions, they reassuringly lead to very similar patterns.

These results could be interpreted very simply in terms of child quality-quantity tradeoffs (Becker, 1960), where child investments are expressed here in terms of consumption allocation. If fertility decreases with household wealth, children with many siblings should get a smaller share of total resources than children with few siblings, conditional on the budget constraint.²¹ Consistently with this interpretation, Figure 3.2 shows that in our global sample, the average number of children tends to decrease with average household resources. This explanation may be related to cultural factors such as more patriarchal behaviors in poorer countries, with men exerting more control over household decisions and being more inclined to have more children but also to redistribute less to each child. Note that cultural traits may also explain vertical heterogeneity in Figure 3.1, i.e. relatively lower resource shares for children and/or larger shares for men, at given levels of living standards, in Muslim countries such as Gambia, Burkina Faso, Mali, Niger and Iraq. This is consistent with higher fertility in these countries, conditional on living standard, as highlighted in Figure 3.2 (see also Heaton, 2011).

Figure 3.2: Average number of children by living standards



Source: authors' estimations using the data from household expenditure surveys for 33 countries. Notes: Figure illustrates the country-level average number of children in households with children, women and men, by log household expenditure. Smooth line is based on locally weighted regressions of the number of children on log household expenditure. Majority muslim/patrilocal indicates if the share of muslim/patrilocal households in a country is equal to or above 50%.

²¹A negative relation between income and fertility has been widely observed: see reviews by Doepke et al. (2022) and Guo et al. (2022).

3.3.3 Implication for Individual Poverty

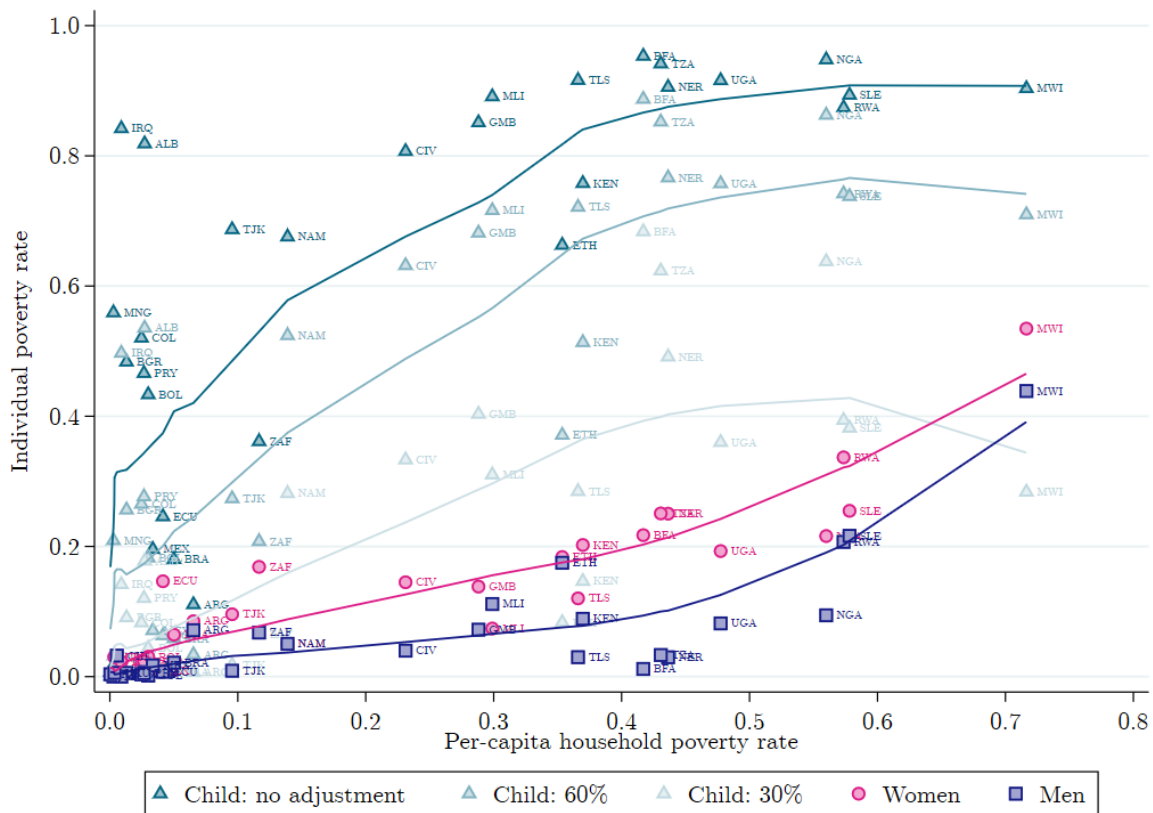
Having estimated individual resource shares, we can calculate individual consumption *levels* of children, women and men to estimate their respective poverty rates. We focus on the international extreme poverty line of \$1.9 per person/day (2011 PPP), but most of our conclusions are similar when using higher poverty lines commonly used for middle-income countries (Ravallion, 2020). In Figure 3.3, we plot individual poverty rates for men, women and children against the household per-capita poverty. In line with the general pattern of resource sharing in Figure 3.1, children’s poverty rates are higher than adults’, and women’s poverty is higher than men’s. At this stage, we do not account for difference in needs (we focus on child poverty under the ‘no adjustment’ scenario). Clearly, there is a monotonic relationship between individual and per-capita poverty, but it is not linear and reflects to some extent the slowly declining inequality pattern described above. That is, as countries get richer and household poverty declines, i.e. moving from right to left on the horizontal axis, child poverty first diminishes slowly because intra-household inequality remains high, as seen in Figure 3.1. For richer settings in our sample, this inequality disappears and children catch up on adults in terms of experiencing a more marked decline in poverty rates.

As discussed before, a key unknown is the difference in needs between adults and children, which explains part of the child-adult inequality we previously found. Official scales exist in terms of nutritional requirements (see FAO/WHO/UNU, 1985), but it concerns part of total consumption. In standard poverty analyses, equivalence scales usually incorporate these differences in needs among household members (in addition to scale economies in the household). For instance, OECD equivalence scales give a weight of 1 for the first adult, 0.5 for the rest adults, and 0.3 for the children, meaning that a child represents 60% of an adult fully benefiting from scale economies (or 40% of the average for the two adults). With this scale and assuming a fair distribution, i.e. an egalitarian allocation proportional to needs, each child in a couple with 2 children should receive $0.3/2.1=14.3\%$ of total household resources, which is probably a lower bound of the children’s resource shares observed for rich countries in previous studies (see the discussion in Bargain, Donni and Hentati, 2022). In a couple with 3 children, a child should receive $0.3/2.4=12.5\%$ which is still higher than the mean child share obtained here, i.e. 11%. This may reflect the fact that most households in our samples contain more than a nuclear family so that each child gets less than what she would obtain in richer countries. To further investigate this question, we compute child poverty assuming child needs equivalent to 60% or 30% those of an adult. The 60% adjustment is relatively standard,²² while the 30% adjustment seems a lower bound of what child needs could be. Strikingly, Figure 3.3 shows that our conclusion about the child-adult poverty gap, especially in countries with intermediate living standards, hold even with the 60% adjustment. It is only by assuming extremely low needs for children that this gap can be reduced (in intermediate countries) or eliminated (at the extremes). Figure 3.A2 in the appendix shows very similar results with the poverty line of

²²See for instance Dunbar et al. (2013) and note that FAO/WHO/UNU adjustment factors for nutrition give a weight of around 70% for children under 10.

\$3.2 per day.

Figure 3.3: Individual and household poverty at \$1.9/day (2011 PPP)



Source: authors' estimations using the data from household expenditure surveys of 33 countries. *Notes:* Figure illustrates country-level individual poverty rates of men, women, and children by per-capita household poverty estimates. Poverty rates are calculated for the sample of households with men, women, and children. Individual poverty rates are based on predicted resource shares for women, men, and children. Results for child poverty are presented for three equivalence scales: (i) the same as an adult (no adjustment), (ii) 60 percent of an adult, and (iii) 30 percent of an adult. Smooth lines are based on locally weighted regressions of individual poverty on per-capita household poverty.

3.3.4 Discussion and Cross-validation

Correlation with Socio-economic Development and Culture. We suggest a first set of informal checks to see if high intra-household inequality in some countries, and low child shares in particular, could be pure statistical noise caused by the method at use or are related to intuitive factors in terms of development, empowerment and cultural dimensions. Note that we do not aim at a causal relationship but simply at a more complete picture of what is associated with within-country inequality. We present results of country-level regressions of child shares in Table 3.A7. The first two estimates confirm that richer countries, but also those with a higher female adult literacy, redistribute more to children. The next two estimates, proportion of muslim population and proportion of patrilineal ancestry, report a significant correlation with cultural traits possibly associated with higher fertility and, as discussed, lower redistribution toward children. Precisely, higher fertility in muslim countries was documented in Figure 3.2 while there is also recent evidence on the links between intra-household inequality and ancestral patrilineality, i.e. the practice of living with or near the parents of the groom (Jayachandran,

2015; Alesina et al., 2021; Aminjonov et al., 2023). Finally, the last coefficient reflects the link between child resource shares and child undernutrition measured by the proportion of wasted children under five in each country (as reported by the World Health Organization, WHO). Wasting refers to low weight-for-height, also known as acute malnutrition, and is a frequently used proxy for short-term food deprivations or illness. Although consumption reflects more than nutrition, we expect a significantly positive correlation, confirmed in Table 3.A7, meaning that children deprived in nutritional terms are also deprived in terms of general consumption.

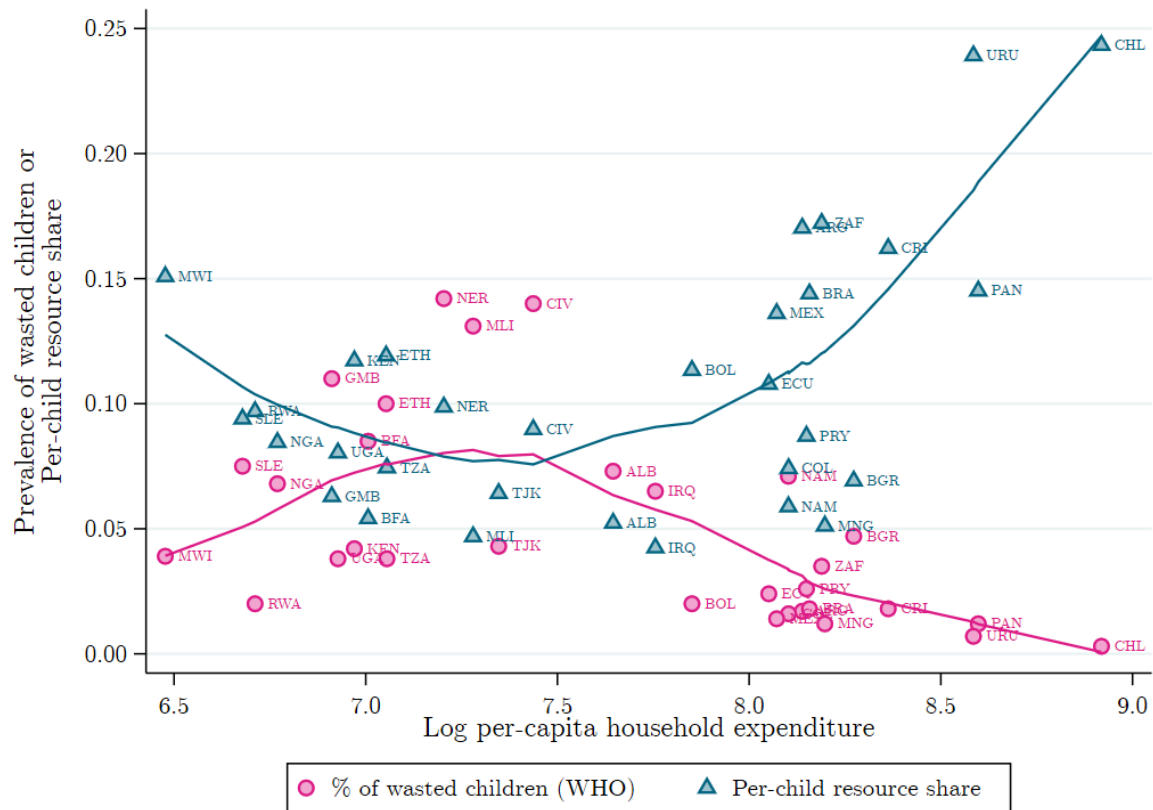
Child Resources and Undernutrition: Macro Comparisons. Given this strong correlation between child resource shares and child undernourishment, we carry on a cross-validation exercise by extending our international comparisons using proxies for nutrition. We still focus on child wasting as a short-term indicator of child welfare and, as such, more directly comparable to current child access to resources. We will nonetheless provide sensitivity checks using alternative measures of child undernutrition such as child stunting. Stunting is measured as low height-for-age, which is an indicator of long-term, chronic malnutrition, which carries long-term developmental risks. This indicator relates to a history of undernutrition and may be less directly related to the proportion of resources children are receiving at a certain moment.

In Figure 3.4, we reproduce past results showing country mean per-child resource shares at different development levels and add the prevalence of wasting among children under five. Child shares and child undernourishment display remarkably symmetrical patterns, which refine the negative correlation reported in Table 3.A7. We highlight the fact that both measures, consumption share and nutrition proxy, derive from completely different sources of information, i.e. resource shares identified on assignable clothing from microdata in one case and wasting prevalence statistics in the other, which makes this result all the more interesting for cross-validation purposes. On the one hand, it provides some reassurance that the way collective models are identified is valid to a certain extent and allows capturing (some) intra-household consumption inequality. At the same time, it corroborates the idea that child undernourishment is not a problem of overall household poverty but also a question of inequality within households (in line with Brown et al., 2019 or Haddad and Kanbur, 1990a,b for instance), possibly related to the quantity-quality tradeoff previously discussed.

To further illustrate this, we plot countries' prevalence of wasted children against two measures of poverty: household poverty (based on per-capita consumption, i.e. the standard welfare measure used in absence of individual welfare proxies) and individual poverty for children (based on our resource share estimates). As shown in Figure 3.A3, the latter measure yields a much larger correlation with child undernutrition because it accounts not only for the overall poverty faced by households but more specifically for resources accruing to children and ultimately the level of child poverty. For instance, with child needs at 60% of an adult and the \$1.9 per person a day (Panel a), the correlation of wasting prevalence with child poverty is 0.69 while the correlation with per-capita poverty is 0.36. If we alternatively take stunting, these figures are 0.82 and 0.76 respectively (these correlations are summarized in the first row of Table 3.A8).

As seen in Figure 3.A3, results are also robust to the choice of poverty line and to assumptions on child needs.

Figure 3.4: Macro cross-validation: per-child resource shares vs. child wasting prevalence by living standard



Source: authors' estimations using WHO statistics on the prevalence of child wasting and the data from household expenditure surveys for 33 countries. *Notes:* Figure illustrates the average per-child resource share and child wasting by log household expenditure. Country-level averages of per-child resource shares and log household expenditure are based on the sample of households with men, women and children. Correlation between the prevalence of child wasting (WHO) and child resource share: -0.49 . Smooth lines are based on locally weighted regressions of child resource shares and the prevalence of child wasting on log household expenditure.

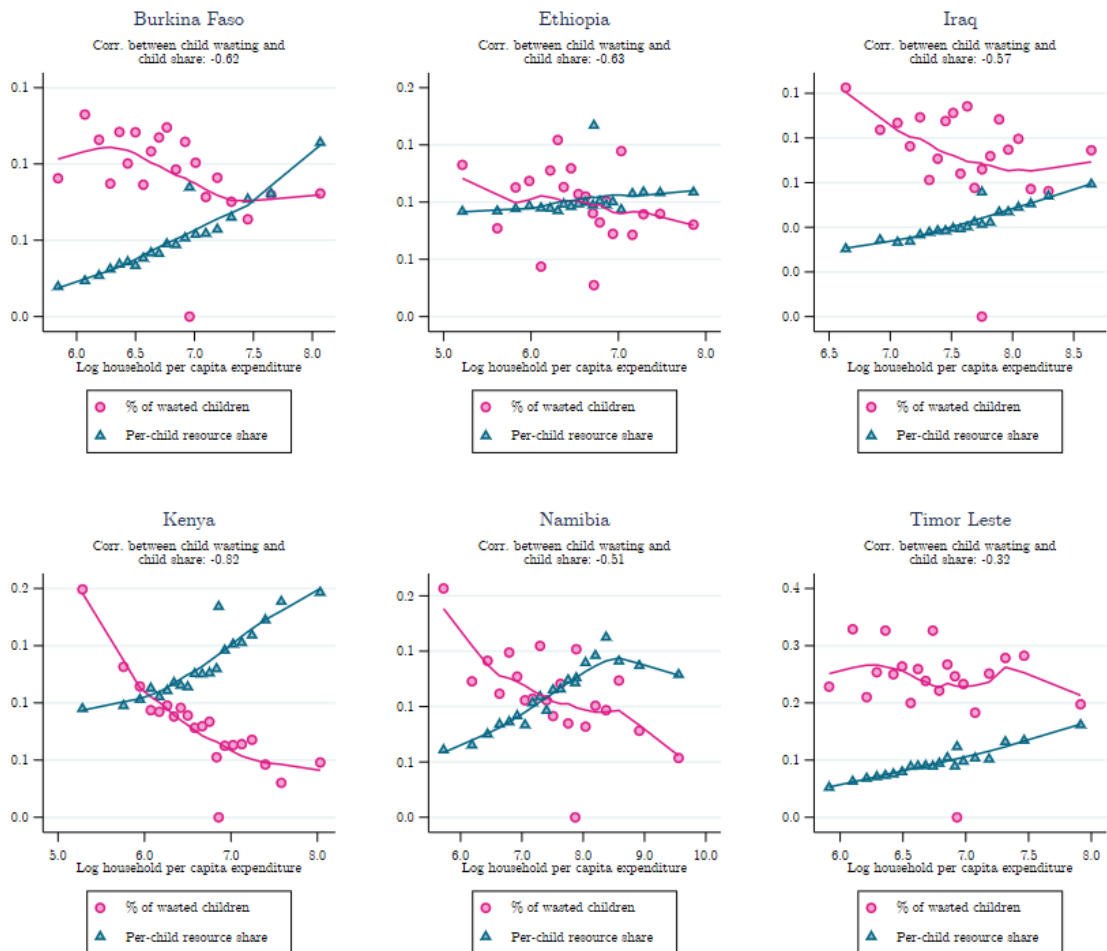
Child Resources and Undernutrition: Micro Comparisons. We can refine the cross-validation of estimated resource shares to nutritional outcomes by using anthropometric information at the micro level for a subset of countries where available. In the spirit of [Brown et al. \(2021\)](#), we compare the prevalence of wasting (or stunting) among children with their estimated resource shares and poverty rates. Note that in the previous section, we used the WHO data on country-level nutrition statistics, which are based on very different sources (sometimes even small surveys conducted in outpatient health services). For micro validation, we focus on the 6 surveys in our global sample that include direct measures of children's weight and height as well as their age in months (for standardization). For each child in these samples, we construct weight-for-height and height-for-age z -scores.²³ A child is considered wasted (stunted) if her weight-for-height (height-for-age) is two standard deviations below the average of her reference

²³We use WHO's `anthro` package in Stata for the standardization of weight-for-height and height-for-age respectively.

group (i.e. $z\text{-score}_j - 2$).

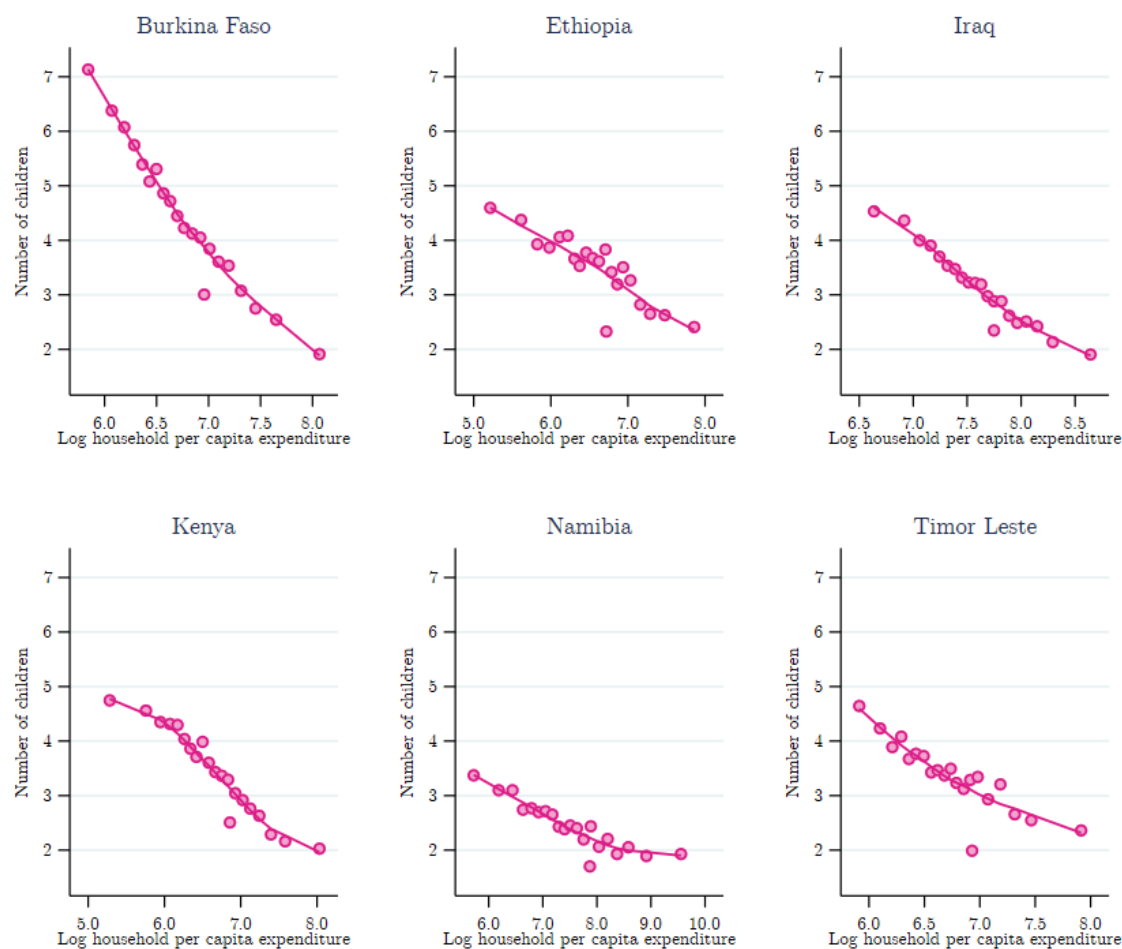
In Figure 3.5, we first examine how child resource shares vary with living standards within each country. We show both overall trends and plots for 20 bins of equal size defined according to the distribution of per-capita household expenditure. Similarly to country-level evidence, systematically across the 6 countries, we find that richer households allocate more resources per child. As for macro results, we can check whether these findings are related to differential quantity-quality trade-offs across living standards. In Figure 3.6, we indeed see that the number of children per household increases with expenditure levels in each country, which tends to support this interpretation. Moreover, Figure 3.5 suggests again a cross-validation by reporting the prevalence of child wasting by bin. The trends for undernourishment are relatively symmetrical to the ones for per-child shares. Subtitles of each graph report the country correlation between child shares and wasting, which ranges from -0.32 in Timor Leste to -0.82 in Kenya, with an average of -0.58. Again, this is an interesting result given the very different origins of the two measures, the shares being inferred from clothing expenditure and child wasting from anthropometric data. Finally, Figure 3.A4 shows similar patterns for child stunting.

Figure 3.5: Micro cross-validation: per-child resource shares vs. child wasting prevalence by living standards



Source: authors' estimations using the data from household expenditure surveys for six countries. Selection of countries are based on the availability of micro-level information on child anthropometrics in household survey data used for the estimation of resource shares. *Notes:* Graphs illustrate the prevalence of child wasting and per-child resource shares, averaged over 20 bins of per-capita household expenditure, by log household expenditure. Smooth lines are based on locally weighted regressions of child resource shares and the prevalence of child wasting on log household expenditure.

Figure 3.6: Average number of children by living standards within country (selected countries)



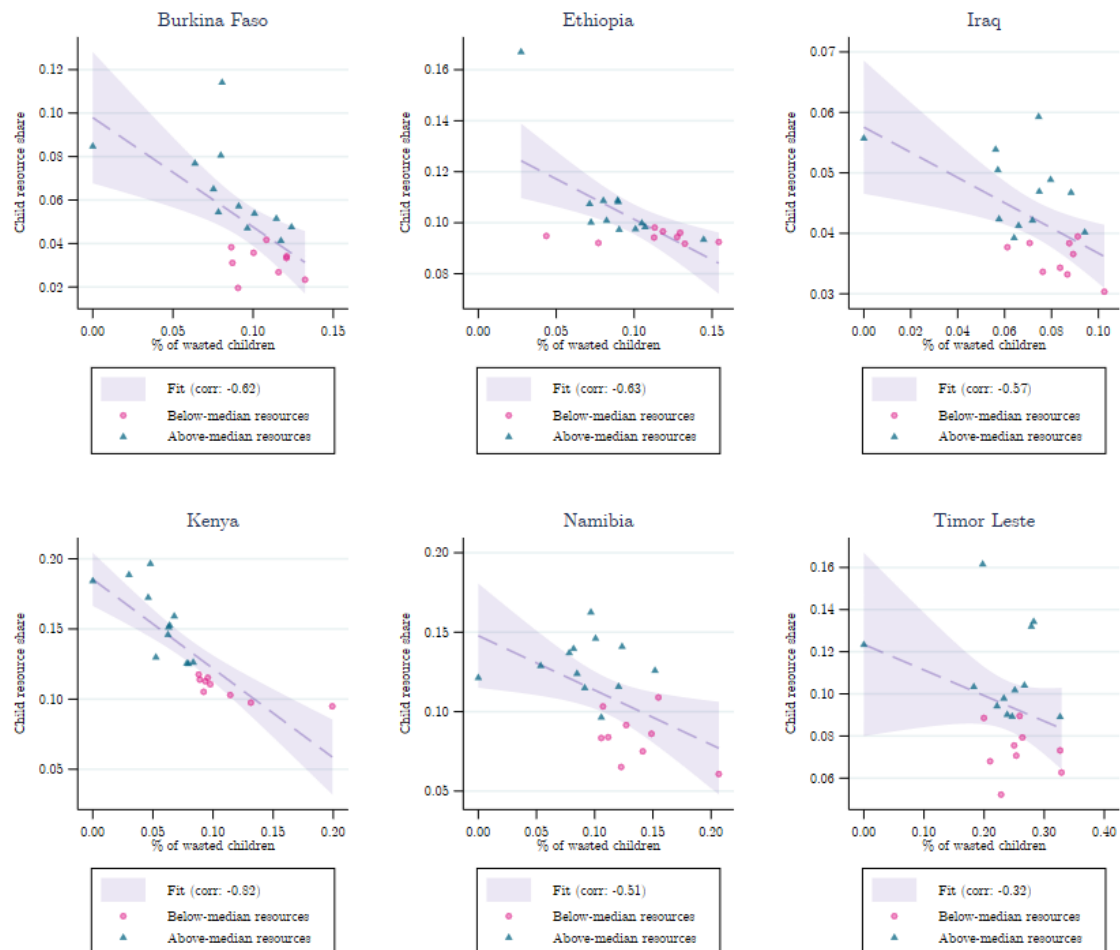
Source: authors' estimations using the data from household expenditure surveys for selected six countries. *Notes:* Graphs illustrate the number of children, averaged over 20 bins of per-capita household expenditure, by log household expenditure. Smooth lines are based on locally weighted regressions of the average number of children on log household expenditure.

In Figure 3.7, we simply plot the correlation between per-child resource shares and the proportion of wasted children across 20 bins defined again according to the distribution of per-capita household expenditure, which allows us to emphasize richer (green triangles) and poorer (pink circles) groups of households. First, we see that nutritional deprivation is present in both richer and poorer households, as also observed in previous studies (e.g. [Brown et al. 2019, 2021](#)). However, there seems to be some complementarity between inequality and poverty: wasted children tend to live in both poorer and more unequal households. Arguably, in some countries such as Ethiopia and Kenya, living in a poor household tends to be enough to identify higher propensity of being wasted. But even in those settings, inequality still matters: the prevalence of wasting tends to be higher in those children receiving a lower resource share. The negative correlation between child shares and undernourishment is actually visible in both consumption groups, i.e. below- or above-average household resources.²⁴ Table 3.A8 (columns 1-3) reports country-level

²⁴[Brown et al. \(2019\)](#) find that child undernutrition is spread across the wealth distribution in 30 African

correlations showing that the prevalence of child wasting is generally more correlated with per-child poverty than with per-capita poverty, as in the macro validation. These results are similar when using child stunting, as illustrated in Figure 3.A5 and Table 3.A8 (columns 4-6).

Figure 3.7: Micro cross-validation: per-child resource shares and poverty vs. child wasting prevalence (bins)



Source: authors' estimations using WHO statistics on the prevalence of child wasting and the data from household expenditure surveys for six countries. Selection of countries is based on the availability of micro-level information on child anthropometrics in household survey data used for the estimation of resource shares. *Notes:* Graphs illustrate per-child shares by the prevalence of child wasting averaged over 20 bins of per-capita household expenditure, differentiating between households with above- or below-median per-capita resources. Correlations between child resource shares and child wasting are reported in legends.

3.4 Concluding Remarks

This paper estimates child, women, and men's resource sharing in 33 countries, mostly low- and middle-income countries, providing the largest exercise of this kind so far. It originally yields a global mapping of gender and age gaps as well as of individual poverty in consumption based on intra-household resource shares inferred from microdata. The huge data assessment accounts countries and that part of this prevalence can be explained by intra-household inequality in nutritional status.

for how often the resource shares can be estimated based on assignable goods in collective model applications and characterizes its limits regarding data availability.

We reinforce and systematize previous results on gender inequality within households, providing evidence of a global gender gap in consumption. We also characterize an adult-child poverty gap in the poorest regions, consistent with an unfavorable child quality-quantity tradeoffs in poor settings where fertility is high. This result is consistently found when comparing countries or with micro evidence within countries. We also contribute to a broad effort of validation of resource-sharing estimation methods. Previous attempts are based on rare datasets containing individualized expenditure (Bargain, Lacroix and Tiberti, 2022; Brown et al., 2021). In this paper, we provide cross-country correlations between child resources and child undernutrition, showing that undernutrition measures align more closely with individual poverty than with per-capita poverty. This result is confirmed for six countries at the micro level, showing that wasted children tend to live in poorer but also more unequal households – the intra-household distribution of resources is relevant to inform on the resources that are actually being allocated to children.

The limits of our work are first those highlighted in previous methodological contributions (Bargain, Lacroix and Tiberti, 2022). Admittedly, the collective model framework and the sharing rule identification used here rest on transparent assumptions and are easily operationalized with standard expenditure data containing information on exclusive or assignable goods (such as male, female and child clothing). Yet, further work is needed to relax the preference restriction used for identification or to further confirm its validity. Moreover, international comparisons and welfare analyses in terms of monetary poverty for individuals rather than households must be enriched with important dimensions of welfare, such as economies of scale due to joint consumption and to the public nature of some goods consumed in the household. A more comprehensive setting of that kind could draw from recent contributions that model both resource sharing and scale economies (Browning et al., 2013; Bargain and Donni, 2012; Bargain, Donni and Hentati, 2022; Calvi et al., 2023). The latter require information on single individuals, which may be difficult to obtain in the context of poor countries, so new identification results are certainly necessary. Finally, some of the intra-household disparity may not be as inequitable as it seems. In very poor settings especially, inequality in nutrient in-take may be due to labor market specialization of certain family members in energy-intensive tasks (Pitt et al., 1990). This is a difficult question but further work should try to address it both empirically and normatively.

Appendix

Estimation Procedure and Endogeneity

Since the error terms of the empirical model are likely to be correlated across equations, the system of household Engel curves for the different household compositions is estimated using Non-Linear Seemingly Unrelated Regressions (NL-SUR). The SUR estimator is iterated until the estimated parameters and error covariance matrices settle (the iterated SUR is equivalent to maximum likelihood with multivariate normal errors). The likely correlation between the error terms in each budget-share function and the log total expenditure is a frequent source of endogeneity (especially if total expenditure suffers from measurement errors). Each budget share equation is then augmented with the Wu-Hausman residuals obtained from reduced-form estimations of x on all exogenous variables used in the model plus some instruments, namely a quadratic form of the log household disposable income (see [Banks et al. 1997](#); [Blundell and Robin 1999](#)). These instruments are very strong in predicting the log of expenditure (the F statistic on the excluded instruments is well above the usual threshold in all cases).

Additional Results

Table 3.A1: Description of household survey data

Country	Country code	Year	Survey name	World Bank LSMS	Recall period for clothing expenditure (months)	Sample size for estimation
Albania	ALB	2005	Living Standard Measurement Survey 2005	Yes	6	2,603
Argentina	ARG	2018	Encuesta Nacional de Gastos de los Hogares 2017-2018		1	20,946
Bolivia	BOL	2019	Encuesta de Hogares - 2019		3	11,044
Brazil	BRA	2017	Pesquisa de Orçamentos Familiares 2017-2018		3	53,681
Bulgaria	BGR	2007	Multitopic Household Survey 2007	Yes	12	2,690
Burkina Faso	BFA	2014	Enquête Multisectorielle Continue 2014 - passage 2	Yes	3	7,090
Chile	CHL	2017	Encuesta de Presupuestos Familiares VIII		3	14,497
Colombia	COL	2017	Encuesta Nacional de Presupuestos de los Hogares 2016–2017		3	81,936
Costa Rica	CRI	2018	Encuesta Nacional de Ingresos y Gastos de los Hogares 2018		3	4,863
Cote d'Ivoire	CIV	2002	Enquete Niveau de Vie des Menages 2002	Yes	12	7,997
Ecuador	ECU	2011	Encuesta Nacional de Ingresos y Gastos de los Hogares Urbanos y Rurales 2011-2012		6	37,059
Ethiopia	ETH	2015	Ethiopia Socioeconomic Survey 2015-2016, Wave 3	Yes	12	4,052
Gambia	GMB	2015	Integrated Household Survey 2015		3	11,130
Ghana	GHA	2017	Ghana Living Standards Survey 2017	Yes	12	6,204
Iraq	IRQ	2012	Iraq Household Socio-Economic Survey 2012, Second Round	Yes	3	11,346
Kenya	KEN	2015	Integrated Household Budget Survey 2015-2016		3	16,817
Malawi	MWI	2016	Integrated Household Panel Survey (IHPS) 2013	Yes	3	9,678
Mali	MLI	2014	Enquête Agricole de Conjoncture Intégrée 2014	Yes	6	1,353
Mexico	MEX	2018	Encuesta Nacional de Ingresos y Gastos de los Hogares 2018		3	63,195
Mongolia	MNG	2016	Household Socio-Economic Survey 2016		12	9,046
Namibia	NAM	2015	Household Income and Expenditure Survey, 2015/16		3	4,639
Niger	NER	2014	Enquête National sur les Conditions de Vie des Ménages et Agriculture 2014, Wave 2	Yes	6	1,733
Nigeria	NGA	2019	General Household Survey, Panel 2015-2016, Wave 3	Yes	6	3,262
Panama	PAN	2008	Encuesta de Niveles de Vida 2008	Yes	3	8,480
Paraguay	PRY	2011	Encuesta de Ingresos y Gastos y de Condiciones de Vida 2011-2012		3	5,274
Rwanda	RWA	2016	Integrated Household Living Conditions Survey, 2016-2017, VUP		12	12,575
Sierra Leone	SLE	2011	Integrated Household Survey 2011		12	6,109
South Africa	ZAF	2014	Living Conditions Survey 2014/2015		12	8,838
Tajikistan	TJK	2009	Living Standards Survey 2009	Yes	6	974
Tanzania	TZA	2014	National Panel Survey 2014-2015, Wave 4	Yes	12	2,433
Timor Leste	TLS	2007	Timor-Leste - Survey of Living Standards 2007	Yes	12	2,492
Uganda	UGA	2015	National Panel Survey 2015-2016	Yes	12	2,432
Uruguay	URU	2016	Encuesta Nacional de Ingresos y Gastos de los Hogares 2016-2017		3	4,262

Table 3.A2: Descriptive statistics, demographics

Country	Year	Number of men	Number of women	Number of children	Average age of men	Average age of women	Average age of children	Proportion of boys	Urban
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Albania	2005	1.50 (0.73)	1.61 (0.71)	1.99 (0.83)	41.20 (8.83)	39.13 (9.47)	6.96 (3.79)	0.55 (0.38)	0.52 (0.50)
Argentina	2018	1.58 (0.90)	1.68 (0.93)	1.59 (0.86)	34.09 (10.97)	33.32 (8.82)	5.17 (2.73)	0.51 (0.43)	1.00 (0.00)
Bolivia	2019	1.38 (0.68)	1.45 (0.73)	1.82 (0.97)	34.01 (10.93)	32.84 (9.70)	6.08 (3.09)	0.50 (0.42)	0.78 (0.41)
Brazil	2017	1.34 (0.66)	1.39 (0.68)	1.60 (0.87)	37.42 (11.35)	35.85 (10.16)	7.29 (3.89)	0.52 (0.44)	0.75 (0.43)
Bulgaria	2007	1.40 (0.64)	1.44 (0.60)	1.42 (0.63)	39.39 (9.29)	37.40 (9.18)	7.49 (4.33)	0.51 (0.45)	0.74 (0.44)
Burkina Faso	2014	1.54 (0.92)	1.78 (1.07)	4.09 (2.57)	38.60 (10.84)	33.96 (9.33)	6.75 (3.19)	0.51 (0.31)	0.36 (0.48)
Chile	2017	1.36 (0.65)	1.48 (0.72)	1.68 (0.85)	40.62 (11.66)	39.59 (9.83)	8.49 (4.65)	0.51 (0.43)	1.00 (0.00)
Colombia	2017	1.48 (0.79)	1.63 (0.87)	1.57 (0.83)	35.86 (12.24)	34.71 (10.18)	6.15 (3.30)	0.52 (0.44)	0.93 (0.26)
Costa Rica	2018	1.44 (0.72)	1.55 (0.78)	1.60 (0.83)	35.70 (12.06)	34.14 (9.01)	6.39 (3.28)	0.53 (0.44)	0.63 (0.48)
Cote d'Ivoire	2002	1.56 (0.96)	1.65 (1.00)	3.53 (2.12)	38.01 (11.02)	32.87 (9.51)	7.52 (3.72)	0.50 (0.33)	0.43 (0.50)
Ecuador	2011	1.55 (0.84)	1.58 (0.84)	1.87 (1.04)	34.46 (11.39)	33.07 (9.67)	6.17 (3.21)	0.51 (0.41)	0.73 (0.44)
Ethiopia	2015	1.37 (0.68)	1.31 (0.61)	3.03 (1.60)	38.87 (11.84)	35.49 (10.14)	8.54 (3.75)	0.51 (0.34)	0.27 (0.44)
Gambia	2015	1.70 (1.03)	2.10 (1.22)	4.61 (2.72)	40.55 (11.17)	35.15 (8.69)	7.38 (3.17)	0.49 (0.29)	0.21 (0.41)
Ghana	2017	1.68 (1.05)	1.79 (1.07)	2.64 (1.65)	34.91 (12.21)	35.18 (10.89)	6.59 (3.08)	0.51 (0.37)	0.33 (0.47)
Iraq	2012	1.43 (0.93)	1.57 (1.02)	3.01 (1.51)	34.98 (7.86)	32.99 (7.84)	4.11 (2.31)	0.51 (0.33)	0.62 (0.49)
Kenya	2015	1.35 (0.72)	1.31 (0.63)	3.08 (1.76)	38.03 (12.49)	35.43 (11.43)	7.96 (4.01)	0.51 (0.34)	0.35 (0.48)
Malawi	2016	1.21 (0.51)	1.18 (0.48)	2.65 (1.42)	36.67 (12.50)	34.04 (11.88)	7.24 (3.86)	0.50 (0.36)	0.18 (0.38)
Mali	2014	1.60 (0.88)	1.77 (0.92)	3.67 (1.90)	39.51 (10.10)	33.85 (9.71)	5.89 (2.69)	0.49 (0.31)	0.43 (0.49)
Mexico	2018	1.34 (0.62)	1.41 (0.67)	2.02 (1.04)	38.82 (11.35)	37.07 (9.80)	8.72 (4.48)	0.51 (0.40)	0.59 (0.49)
Mongolia	2016	1.19 (0.49)	1.25 (0.55)	1.90 (0.88)	34.48 (8.10)	33.39 (7.41)	5.33 (3.16)	0.51 (0.40)	0.57 (0.50)
Namibia	2015	1.34 (0.66)	1.42 (0.69)	2.21 (1.31)	38.30 (12.54)	37.13 (11.92)	5.27 (3.09)	0.50 (0.40)	0.42 (0.49)
Niger	2014	1.22 (0.57)	1.38 (0.69)	3.57 (2.04)	40.84 (11.24)	33.83 (9.69)	6.21 (2.84)	0.50 (0.33)	0.35 (0.48)
Nigeria	2019	1.36 (0.76)	1.47 (0.78)	3.30 (2.08)	40.07 (11.34)	34.86 (9.92)	7.16 (3.49)	0.51 (0.34)	0.30 (0.46)
Panama	2008	1.60 (0.92)	1.73 (0.94)	1.76 (1.07)	34.66 (11.75)	33.61 (9.75)	5.63 (3.05)	0.52 (0.42)	1.00 (0.00)
Paraguay	2011	1.62 (0.91)	1.64 (0.90)	1.94 (1.16)	36.10 (11.69)	34.90 (11.18)	6.62 (3.48)	0.51 (0.41)	0.59 (0.49)
Rwanda	2016	1.25 (0.56)	1.30 (0.60)	2.73 (1.44)	37.67 (11.67)	35.91 (10.43)	7.68 (4.21)	0.50 (0.35)	0.14 (0.35)
Sierra Leona	2011	1.47 (0.76)	1.62 (0.83)	3.00 (1.57)	39.72 (11.88)	35.34 (10.27)	8.44 (3.48)	0.52 (0.33)	0.32 (0.47)
South Africa	2014	1.42 (0.71)	1.61 (0.81)	1.96 (1.10)	38.28 (12.72)	39.32 (11.99)	6.07 (3.26)	0.50 (0.41)	0.63 (0.48)
Tajikistan	2009	2.10 (1.16)	2.28 (1.19)	3.03 (1.57)	38.94 (8.85)	37.54 (7.69)	7.18 (3.86)	0.51 (0.34)	0.30 (0.46)
Tanzania	2014	1.33 (0.71)	1.32 (0.67)	3.00 (1.91)	36.76 (10.52)	33.18 (9.76)	6.45 (3.75)	0.50 (0.35)	0.35 (0.48)
Timor Leste	2007	1.35 (0.72)	1.33 (0.63)	3.04 (1.55)	37.02 (9.26)	34.12 (8.94)	5.94 (3.19)	0.50 (0.33)	0.44 (0.50)
Uganda	2015	1.40 (0.70)	1.37 (0.66)	3.30 (1.81)	37.12 (11.91)	35.54 (11.00)	8.48 (3.67)	0.51 (0.33)	0.23 (0.42)
Uruguay	2016	1.36 (0.67)	1.44 (0.70)	1.52 (0.75)	35.49 (10.96)	33.82 (8.78)	5.59 (3.09)	0.52 (0.44)	0.74 (0.44)

Source: authors' calculations using the data from household expenditure surveys of each country. Notes: Sample of households with men, women, and children. Standard deviations in parentheses.

Table 3.A3: Descriptive statistics, expenditure

Country	Year	Annual HH expenditure (2011 PPP\$)	Household budget share for clothing			% of zeros in clothing budget shares		
			Men	Women	Children	Men	Women	Children
			(1)	(2)	(3)	(4)	(5)	(6)
Albania	2005	10,250 (5,995)	0.015 (0.016)	0.018 (0.020)	0.022 (0.019)	0.288 (0.453)	0.214 (0.410)	0.100 (0.301)
Argentina	2018	14,745 (12,000)	0.028 (0.047)	0.024 (0.041)	0.033 (0.049)	0.591 (0.492)	0.589 (0.492)	0.479 (0.500)
Bolivia	2019	11,229 (5,774)	0.006 (0.012)	0.007 (0.013)	0.014 (0.020)	0.744 (0.436)	0.717 (0.451)	0.565 (0.496)
Brazil	2017	13,945 (11,846)	0.020 (0.030)	0.018 (0.029)	0.016 (0.026)	0.475 (0.499)	0.515 (0.500)	0.489 (0.500)
Bulgaria	2007	15,598 (7,237)	0.019 (0.015)	0.022 (0.018)	0.025 (0.022)	0.140 (0.347)	0.111 (0.314)	0.189 (0.391)
Burkina Faso	2014	7,110 (3,910)	0.013 (0.018)	0.013 (0.016)	0.016 (0.016)	0.368 (0.482)	0.262 (0.440)	0.200 (0.400)
Chile	2017	31,454 (24,112)	0.009 (0.014)	0.009 (0.013)	0.022 (0.021)	0.535 (0.499)	0.428 (0.495)	0.093 (0.291)
Colombia	2017	14,293 (8,302)	0.013 (0.025)	0.014 (0.025)	0.016 (0.026)	0.675 (0.468)	0.616 (0.486)	0.528 (0.499)
Costa Rica	2018	18,290 (12,081)	0.010 (0.016)	0.012 (0.019)	0.024 (0.028)	0.536 (0.499)	0.451 (0.498)	0.206 (0.405)
Cote d'Ivoire	2002	10,719 (15,259)	0.018 (0.019)	0.034 (0.029)	0.029 (0.024)	0.260 (0.439)	0.147 (0.354)	0.079 (0.270)
Ecuador	2011	14,441 (10,676)	0.032 (0.027)	0.035 (0.030)	0.035 (0.028)	0.077 (0.266)	0.091 (0.288)	0.052 (0.222)
Ethiopia	2015	3,032 (2,164)	0.039 (0.050)	0.032 (0.042)	0.046 (0.051)	0.220 (0.414)	0.201 (0.400)	0.105 (0.306)
Gambia	2015	7,340 (4,890)	0.010 (0.013)	0.011 (0.014)	0.025 (0.030)	0.334 (0.472)	0.184 (0.388)	0.202 (0.402)
Ghana	2017	7,003 (5,197)	0.028 (0.029)	0.023 (0.021)	0.028 (0.024)	0.115 (0.318)	0.068 (0.252)	0.064 (0.245)
Iraq	2012	12,959 (7,242)	0.039 (0.029)	0.029 (0.024)	0.032 (0.024)	0.040 (0.195)	0.051 (0.221)	0.044 (0.205)
Kenya	2015	5,620 (7,901)	0.015 (0.025)	0.018 (0.026)	0.028 (0.037)	0.533 (0.499)	0.427 (0.495)	0.316 (0.465)
Malawi	2016	3,123 (2,810)	0.005 (0.011)	0.009 (0.014)	0.013 (0.019)	0.758 (0.428)	0.548 (0.498)	0.463 (0.499)
Mali	2014	9,002 (6,121)	0.015 (0.018)	0.016 (0.028)	0.007 (0.011)	0.176 (0.381)	0.131 (0.338)	0.429 (0.495)
Mexico	2018	14,341 (12,205)	0.012 (0.023)	0.012 (0.021)	0.034 (0.037)	0.596 (0.491)	0.546 (0.498)	0.222 (0.416)
Mongolia	2016	15,089 (8,836)	0.031 (0.026)	0.037 (0.027)	0.033 (0.023)	0.022 (0.147)	0.009 (0.094)	0.034 (0.181)
Namibia	2015	15,048 (19,378)	0.022 (0.051)	0.020 (0.040)	0.028 (0.048)	0.668 (0.471)	0.594 (0.491)	0.472 (0.499)
Niger	2014	7,538 (4,614)	0.015 (0.018)	0.013 (0.016)	0.019 (0.020)	0.191 (0.393)	0.114 (0.318)	0.100 (0.301)
Nigeria	2019	4,848 (4,583)	0.009 (0.013)	0.011 (0.013)	0.023 (0.023)	0.412 (0.492)	0.332 (0.471)	0.141 (0.348)
Panama	2008	24,883 (16,104)	0.029 (0.030)	0.024 (0.023)	0.026 (0.026)	0.194 (0.395)	0.172 (0.377)	0.148 (0.355)
Paraguay	2011	16,728 (12,248)	0.012 (0.019)	0.024 (0.023)	0.014 (0.020)	0.460 (0.498)	0.115 (0.319)	0.405 (0.491)
Rwanda	2016	4,224 (3,573)	0.011 (0.013)	0.014 (0.013)	0.010 (0.010)	0.125 (0.330)	0.054 (0.226)	0.074 (0.262)
Sierra Leone	2011	4,578 (3,003)	0.014 (0.022)	0.008 (0.014)	0.013 (0.020)	0.158 (0.365)	0.159 (0.366)	0.187 (0.390)
South Africa	2014	15,869 (18,972)	0.036 (0.045)	0.034 (0.038)	0.055 (0.052)	0.333 (0.471)	0.242 (0.429)	0.122 (0.327)
Tajikistan	2009	10,730 (5,552)	0.020 (0.023)	0.025 (0.029)	0.020 (0.027)	0.251 (0.434)	0.141 (0.348)	0.174 (0.379)
Tanzania	2014	5,939 (3,737)	0.016 (0.016)	0.019 (0.018)	0.014 (0.017)	0.143 (0.350)	0.091 (0.287)	0.222 (0.416)
Timor Leste	2007	5,540 (4,868)	0.018 (0.031)	0.018 (0.029)	0.024 (0.036)	0.293 (0.455)	0.284 (0.451)	0.243 (0.429)
Uganda	2015	5,824 (6,403)	0.009 (0.010)	0.009 (0.010)	0.011 (0.010)	0.311 (0.463)	0.271 (0.445)	0.180 (0.384)
Uruguay	2016	21,397 (14,179)	0.016 (0.022)	0.020 (0.024)	0.035 (0.029)	0.396 (0.489)	0.313 (0.464)	0.062 (0.241)

Source: authors' calculations using the data from household expenditure surveys of each country. Notes: Sample of households with men, women, and children. Standard deviations in parentheses.

Table 3.A4: Marginal effects on per-child resource shares.

Country	Year	Per-child resource shares				
		Average age of children	Average adult age	Age difference between women and men	Proportion of boys	Urban
		(1)	(2)	(3)	(4)	(5)
Albania	2005	0.018*** (0.005)	-0.001 (0.002)	0.007*** (0.002)	0.004* (0.003)	0.011** (0.005)
Argentina	2018	-0.036*** (0.010)	-0.004 (0.004)	0.004 (0.004)	0.009 (0.006)	- -
Bolivia	2019	-0.024*** (0.005)	-0.004** (0.002)	0.000 (0.002)	-0.002 (0.003)	-0.019*** (0.006)
Brazil	2017	-0.038*** (0.003)	0.003* (0.002)	-0.011*** (0.002)	-0.006** (0.003)	-0.004 (0.007)
Bulgaria	2007	-0.035*** (0.008)	0.001 (0.002)	0.003 (0.002)	-0.005* (0.003)	0.009 (0.007)
Burkina Faso	2014	0.000 (0.001)	0.002*** (0.001)	-0.001 (0.001)	0.003** (0.001)	-0.002 (0.002)
Chile	2017	-0.004 (0.007)	-0.003 (0.005)	0.010** (0.005)	0.020*** (0.007)	- -
Colombia	2017	-0.020*** (0.002)	-0.001** (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.004 (0.004)
Costa Rica	2018	-0.052*** (0.015)	0.016*** (0.006)	0.009 (0.006)	-0.002 (0.008)	-0.034 (0.021)
Cote d'Ivoire	2002	0.002* (0.001)	0.003*** (0.000)	-0.003*** (0.000)	-0.001 (0.001)	0.009*** (0.002)
Ecuador	2011	0.008*** (0.001)	0.004*** (0.001)	-0.008*** (0.001)	-0.002** (0.001)	0.000 (0.003)
Ethiopia	2015	0.038*** (0.005)	0.011*** (0.002)	0.001 (0.002)	0.001 (0.005)	-0.006 (0.011)
Gambia	2015	0.004*** (0.001)	0.003*** (0.001)	0.001** (0.001)	0.006*** (0.001)	0.001 (0.002)
Ghana	2017	-0.006 (0.005)	0.000 (0.002)	0.004** (0.002)	-0.001 (0.003)	0.018** (0.008)
Iraq	2012	0.009*** (0.001)	0.000 (0.000)	-0.002*** (0.000)	0.001 (0.001)	0.003** (0.002)
Kenya	2015	0.006*** (0.002)	0.001* (0.001)	0.002** (0.001)	0.004** (0.002)	0.001 (0.003)
Malawi	2016	-0.006* (0.003)	0.000 (0.001)	0.001 (0.001)	0.000 (0.003)	0.013* (0.008)
Mali	2014	0.009** (0.005)	-0.003* (0.002)	0.000 (0.002)	-0.002 (0.003)	0.004 (0.007)
Mexico	2018	0.006*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.000 (0.001)	0.003 (0.004)
Mongolia	2016	0.003** (0.001)	0.003*** (0.001)	-0.003*** (0.001)	-0.001 (0.001)	-0.022*** (0.003)
Namibia	2015	-0.020*** (0.007)	-0.003** (0.001)	0.001 (0.001)	0.000 (0.003)	0.008** (0.004)
Niger	2014	0.008 (0.005)	0.001 (0.002)	-0.003* (0.002)	0.002 (0.003)	-0.014* (0.008)
Nigeria	2019	-0.004* (0.002)	-0.001 (0.001)	0.000 (0.001)	0.002 (0.003)	0.008** (0.003)
Panama	2008	-0.086*** (0.009)	0.003 (0.002)	-0.004* (0.002)	0.006* (0.003)	- -
Paraguay	2011	-0.060*** (0.007)	0.000 (0.001)	-0.002 (0.001)	0.010*** (0.003)	0.005 (0.005)
Rwanda	2016	-0.015*** (0.002)	0.000 (0.001)	0.000 (0.001)	0.001 (0.002)	0.023*** (0.006)
Sierra Leone	2011	0.007*** (0.002)	0.000 (0.001)	0.002** (0.001)	-0.002 (0.003)	0.037*** (0.005)
South Africa	2014	0.004 (0.006)	-0.003 (0.002)	-0.002 (0.002)	0.002 (0.004)	-0.001 (0.008)
Tajikistan	2009	-0.004 (0.004)	0.005 (0.003)	-0.003 (0.003)	0.013 (0.004)	0.010 (0.008)
Tanzania	2014	-0.017*** (0.004)	0.002* (0.001)	-0.004*** (0.001)	0.003 (0.003)	-0.007 (0.006)
Timor Leste	2007	-0.040*** (0.008)	0.003 (0.003)	0.002 (0.003)	0.003 (0.004)	0.009 (0.008)
Uganda	2015	0.011*** (0.003)	-0.003** (0.001)	-0.003** (0.001)	-0.004 (0.003)	0.007 (0.008)
Uruguay	2016	0.024* (0.013)	-0.024*** (0.006)	0.000 (0.006)	0.011 (0.008)	0.006 (0.021)

Source: authors' estimations using the data from household expenditure surveys for 33 countries. Notes: Table reports the marginal effects of selected variables on per-child resource shares. Sample of households with men, women, and children. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 3.A5: Marginal effects on per-woman resource shares.

Country	Year	Per-woman resource shares				
		Average age of children	Average adult age	Age difference between women and men	Proportion of boys	Urban
		(1)	(2)	(3)	(4)	(5)
Albania	2005	0.008 (0.007)	-0.001 (0.003)	-0.023*** (0.003)	0.001 (0.006)	-0.014 (0.019)
Argentina	2018	0.012 (0.010)	0.008*** (0.003)	-0.025*** (0.003)	-0.019** (0.008)	-
Bolivia	2007	0.010* (0.006)	-0.001 (0.002)	-0.011*** (0.002)	0.003 (0.005)	0.020 (0.012)
Brazil	2014	0.018*** (0.004)	-0.002 (0.002)	0.032*** (0.002)	0.014*** (0.003)	-0.002 (0.007)
Bulgaria	2017	0.028*** (0.006)	-0.002 (0.003)	-0.035*** (0.003)	-0.017*** (0.005)	-0.001 (0.016)
Burkina Faso	2019	0.006 (0.006)	0.001 (0.003)	-0.015*** (0.003)	0.001 (0.006)	-0.019** (0.008)
Chile	2017	-0.017* (0.009)	0.000 (0.004)	-0.019*** (0.004)	-0.025** (0.010)	-
Colombia	2018	0.013*** (0.003)	-0.002** (0.001)	-0.019*** (0.001)	-0.004* (0.002)	-0.006 (0.010)
Costa Rica	2002	0.038*** (0.013)	-0.005 (0.005)	-0.031*** (0.005)	-0.016* (0.010)	0.005 (0.025)
Cote d'Ivoire	2017	-0.004 (0.003)	0.003* (0.002)	-0.002 (0.002)	0.000 (0.003)	-0.006 (0.005)
Ecuador	2011	-0.009*** (0.002)	-0.001* (0.001)	0.029*** (0.001)	0.001 (0.001)	-0.004 (0.003)
Ethiopia	2015	-0.039*** (0.011)	-0.009 (0.007)	-0.043*** (0.007)	-0.009 (0.011)	-0.043** (0.019)
Gambia	2015	0.008** (0.003)	0.003* (0.002)	-0.007*** (0.002)	-0.011*** (0.004)	0.011* (0.006)
Ghana	2017	-0.012 (0.009)	-0.001 (0.003)	-0.004 (0.003)	-0.013* (0.007)	-0.001 (0.011)
Iraq	2017	-0.001 (0.003)	0.001 (0.001)	-0.020*** (0.001)	-0.001 (0.002)	0.003 (0.005)
Kenya	2012	-0.007** (0.004)	-0.001 (0.002)	-0.006*** (0.002)	-0.005 (0.004)	-0.003 (0.007)
Malawi	2018	0.007 (0.006)	-0.002 (0.003)	-0.005* (0.003)	0.000 (0.006)	-0.038*** (0.014)
Mali	2016	0.008 (0.012)	0.001 (0.005)	-0.010** (0.005)	0.007 (0.010)	-0.049 (0.034)
Mexico	2015	-0.004* (0.002)	-0.007*** (0.001)	-0.043*** (0.001)	-0.002 (0.003)	0.001 (0.006)
Mongolia	2014	0.003 (0.004)	-0.005** (0.002)	-0.031*** (0.002)	-0.007** (0.003)	-0.031*** (0.008)
Namibia	2016	0.012 (0.015)	0.012** (0.005)	0.003 (0.005)	0.005 (0.012)	-0.034* (0.018)
Niger	2015	0.008 (0.016)	0.003 (0.007)	-0.027*** (0.007)	-0.023** (0.012)	0.097*** (0.037)
Nigeria	2014	0.007 (0.005)	0.003 (0.003)	0.001 (0.003)	-0.001 (0.006)	-0.020* (0.011)
Panama	2019	0.064*** (0.007)	0.007*** (0.002)	-0.023*** (0.002)	-0.011** (0.005)	-
Paraguay	2008	0.061*** (0.006)	-0.004* (0.003)	0.020*** (0.003)	-0.005 (0.005)	-0.007 (0.008)
Rwanda	2011	0.021*** (0.004)	0.001 (0.002)	-0.018*** (0.002)	-0.013*** (0.004)	-0.039*** (0.012)
Sierra Leone	2016	0.012* (0.007)	0.005** (0.002)	0.004** (0.002)	-0.011 (0.011)	-0.043*** (0.014)
South Africa	2015	0.015** (0.006)	0.003 (0.002)	-0.025*** (0.002)	0.007 (0.005)	0.029* (0.015)
Tajikistan	2011	0.010 (0.006)	-0.001 (0.004)	-0.010*** (0.004)	-0.011 (0.007)	0.015 (0.017)
Tanzania	2009	0.053*** (0.008)	0.003 (0.004)	-0.008** (0.004)	-0.047*** (0.007)	0.005 (0.014)
Timor Leste	2014	0.050*** (0.012)	-0.006 (0.005)	-0.022*** (0.005)	-0.027*** (0.010)	0.006 (0.021)
Uganda	2014	0.002 (0.009)	0.004 (0.004)	0.004 (0.004)	-0.003 (0.008)	0.002 (0.015)
Uruguay	2007	-0.018 (0.011)	0.013*** (0.004)	-0.024*** (0.004)	0.000 (0.009)	-0.009 (0.022)

Source: authors' estimations using the data from household expenditure surveys for 33 countries. Notes: Table reports the marginal effects of selected variables on per-woman resource shares. Sample of households with men, women, and children. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 3.A6: A review of country-specific resource share estimations

Country	Sample	Per-woman resource shares		Per-child resource shares		Authors
		households with children, women and men	households with women and men	Mean across household compositions	Range across household compositions	
Albania	Complex households	0.26	0.29	0.13	-	Lechene et al. (2022)
Argentina	Couples with children and single parents	-	-	-	0.22-0.61	Echeverría et al. (2019)
Bangladesh	Complex households	0.25	-	-	0.15-0.16	Brown et al. (2021)
Bangladesh	Complex households	0.27	-	0.13	-	Calvi et al. (2023)
Bangladesh	Complex households	0.28	-	-	0.15-0.24	Bargain, Lacroix and Tiberti (2022)
Bangladesh	Complex households	0.29	-	-	0.14	Lechene et al. (2022)
Brazil	Couples with/without children	0.34-0.41	0.47	-	0.10-0.23	Gómez and Coelho (2017)
Brazil	Singles and couples with/without children	0.32-0.39	0.47	-	0.11-0.20	Iglesias and Coelho (2020)
Bulgaria	Complex households	0.39	0.45	0.17	-	Lechene et al. (2022)
Côte d'Ivoire	Singles and couples with/without children	0.38-0.42	0.52	-	0.09-0.19	Bargain et al. (2014)
Ethiopia	Couples with children and single mothers	-	-	0.19	0.15-0.32	Belete et al. (2019)
Ghana	Complex households	0.24-0.28	0.31	0.07	0.06-0.08	Aminjonov et al. (2023)
Iraq	Complex households	0.21	0.26	0.05	-	Lechene et al. (2022)
Malawi	Couples with children	0.30	-	0.10	0.07-0.14	Dunbar et al. (2013)
Malawi	Complex households	0.29	-	0.12	0.11-0.14	Penglase (2021)
Malawi	Complex households	0.27	0.28	0.13	0.07-0.15	Lechene et al. (2022)
Malawi	Complex households	0.28	0.37	0.15	0.14-0.16	Aminjonov et al. (2023)
Mexico	Poor nuclear households (PROGRESA)	0.31-0.38	-	-	0.09-0.32	Tommasi et al. (2016)
Mexico	Poor nuclear households (PROGRESA)	0.34-0.37	-	-	0.07-0.12	Sokullu and Valente (2022)
Mexico	Poor nuclear households (PROGRESA)	0.29-0.31	-	-	0.11-0.28	Tommasi (2019)
Mexico	Complex households	0.34	-	0.16	-	Calvi et al. (2023)
South Africa	Couples with children	0.28-0.36	0.45	-	0.12-0.20	Bargain et al. (2018)

Table 3.A7: Per-child resource shares, culture and development indicators: Correlations

	Dependent variable: Average per-child resource share				
	(1)	(2)	(3)	(4)	(5)
GDP per capita (logs)	0.024*** (0.008)				
Female adult literacy rate		0.067* (0.034)			
% muslim population			-0.075*** (0.022)		
% patrilocal ancestry				-0.061** (0.029)	
Prevalence of child wasting					-0.639*** (0.209)
Observations	31	31	31	31	31
R-squared	0.244	0.121	0.293	0.134	0.218

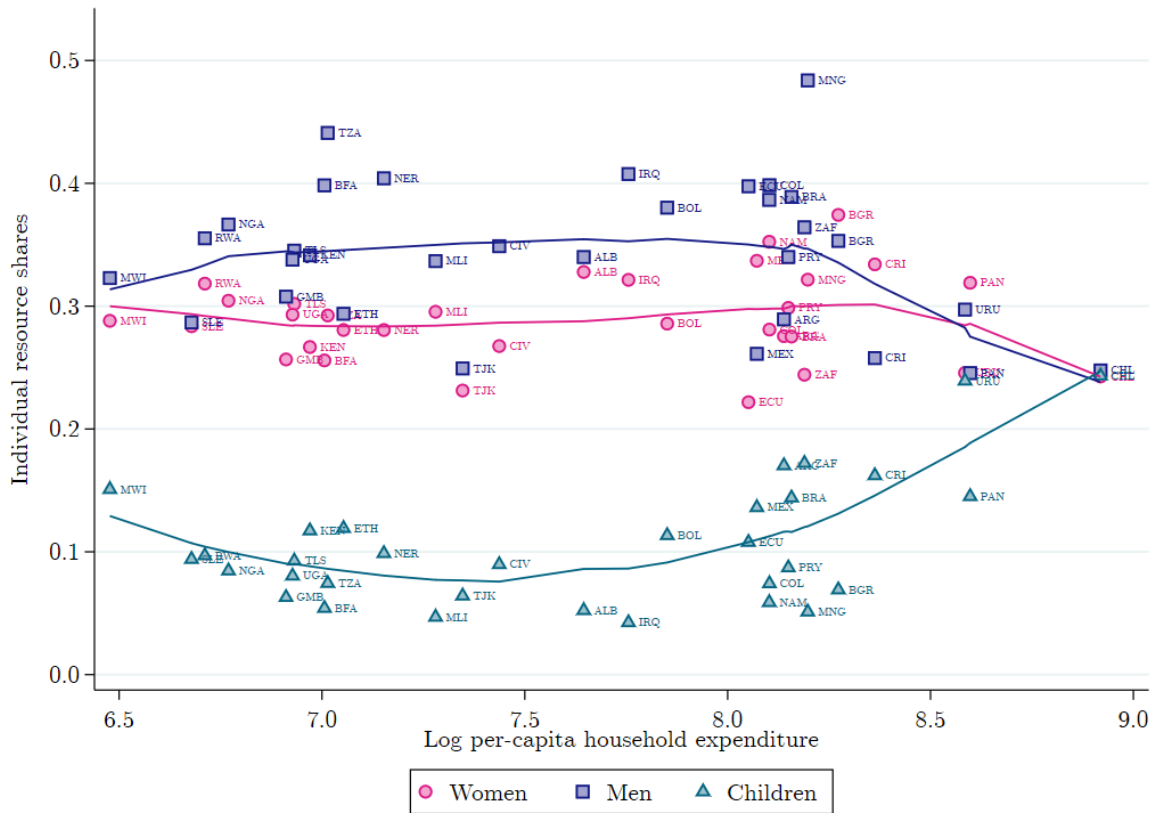
Source: authors' estimations of country mean estimates of child resources on various indicators (closest to the year of expenditure surveys): proportion of children under five with weight-for-height under 2 standard deviations of the population reference average (child wasting) reported by WHO; female literacy rate (women over the age of 15 that can read and write) and per capita GDP (PPP constant 2017 international dollars) reported by the World Bank; percentage of the Muslim population from world population review; ancestral patrilocality as reported by [Alesina et al. \(2013\)](#). Patrilocality is the practice of living with or near to the parents of the groom. We consider ancestral patrilocality which refers to the practice of those who lived in the country before European contact. To build the national-level data, [Alesina et al. \(2013\)](#) provide population-weighted national average for each country based on the Murdock atlas ([Murdock, 1967](#)). *Notes:* Table reports the coefficients from regressions of country average per-child resource shares on each development indicator. The regression for each indicator is run separately. Standard errors in parentheses. *, **, *** indicate 10%, 5% and 1% significance level. Timor Leste is excluded because of extreme child-wasting prevalence and Ghana because of missing data in relevant indicators.

Table 3.A8: Child nutrition, per-child shares, and poverty correlations

	Correlation with child wasting			Correlation with child stunting		
	Child resource share	Per-capita poverty	Child poverty	Child resource share	Per-capita poverty	Child poverty
	(1)	(2)	(3)	(4)	(5)	(6)
Country-level correlation:	-0.49	0.36	0.69	-0.27	0.76	0.82
Micro-level correlation:						
Burkina Faso	-0.62	0.21	0.52	-0.78	0.56	0.61
Ethiopia	-0.63	0.31	0.34	-0.61	0.17	0.35
Iraq	-0.57	0.28	0.34	-0.78	0.25	0.84
Kenya	-0.82	0.68	0.73	-0.87	0.62	0.71
Namibia	-0.51	0.46	0.52	-0.82	0.31	0.86
Timor Leste	-0.32	0.09	0.18	-0.21	0.05	-0.04

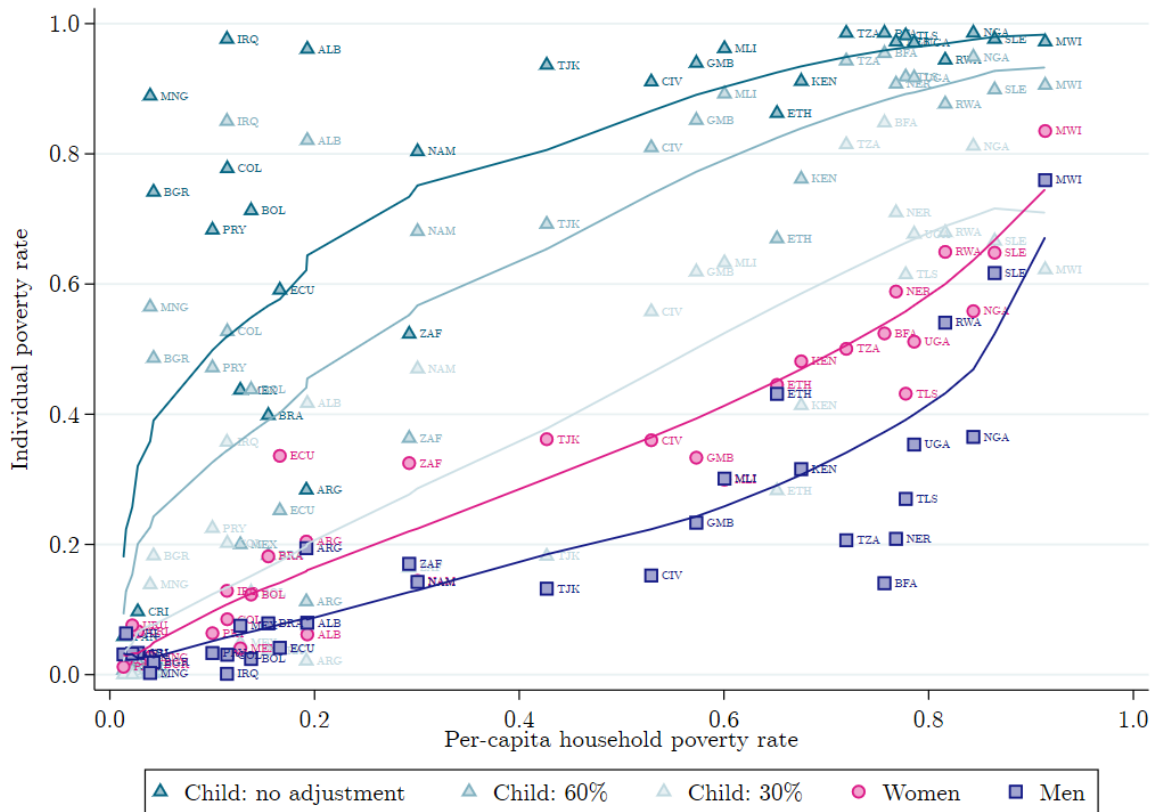
Source: authors' estimations using the data from household expenditure surveys for six countries that record micro-level information on child anthropometrics. *Notes:* Table reports the correlation between child undernutrition indicators, per-child shares, and poverty (household and per-child). Poverty line at \$1.9/day (2011 PPP) and child weight at 60% of an adult. Sample of households with men, women, and children.

Figure 3.A1: Individual resource shares by living standards (within-country mean resource shares)



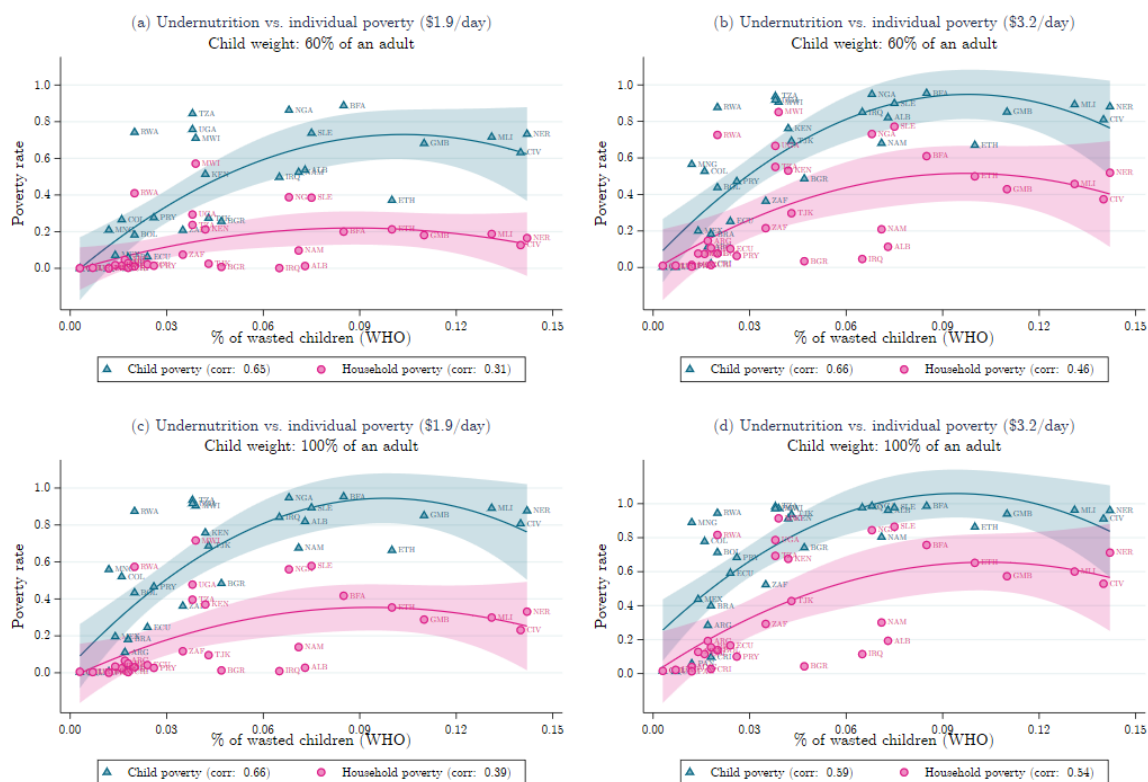
Source: authors' estimations using the data from household expenditure surveys of 33 countries. Notes: Figure illustrates country-level average predicted per-man, per-woman, and per-child resource shares by log total household expenditure. Country-level averages of resource shares and log household expenditure are based on the sample of households with men, women and children. Per-person shares do not add up to one due to the different number of members of each demographic group within households. Smooth lines are based on locally weighted regressions of resource shares on log household expenditure.

Figure 3.A2: Individual and per capita poverty at \$3.2/day (2011 PPP)



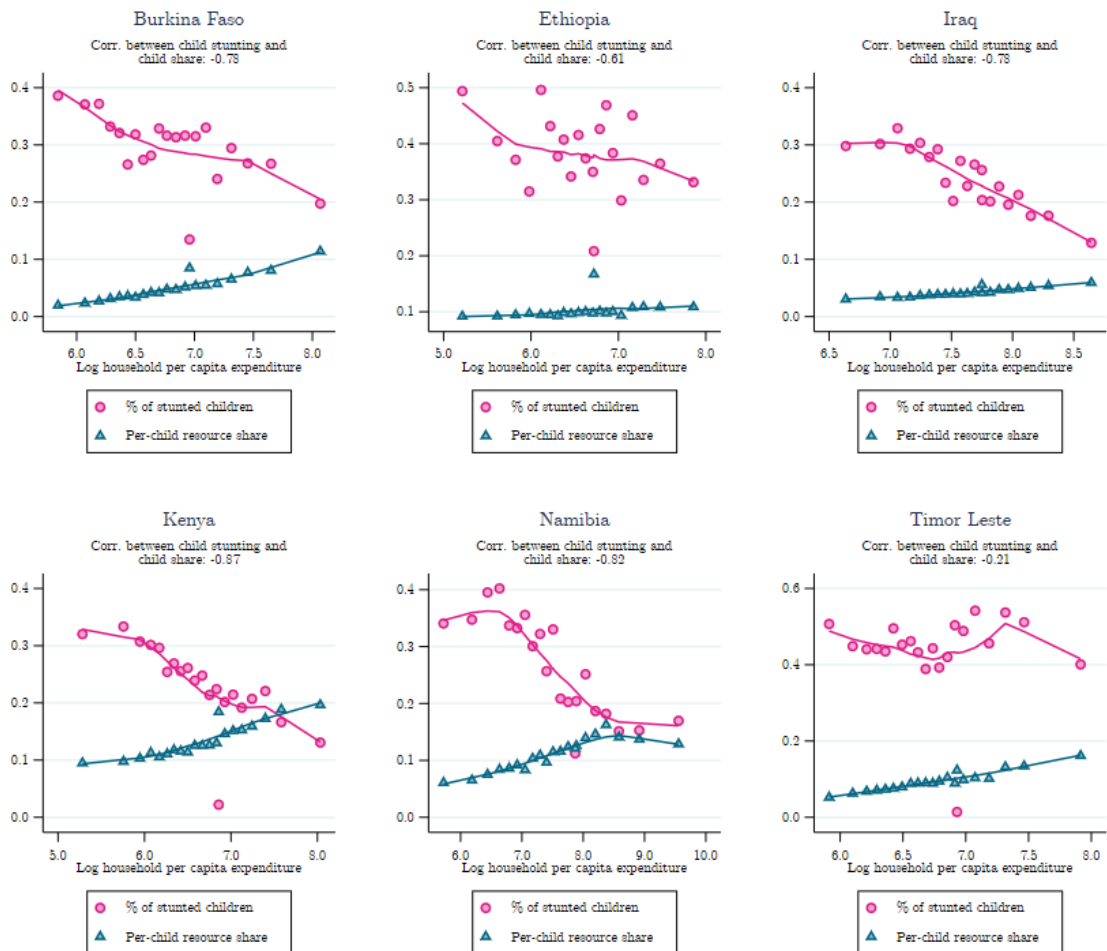
Source: authors' estimations using the data from household expenditure surveys of 33 countries. Notes: Figure illustrates country-level individual poverty rates of men, women, and children by per-capita household poverty estimates. Poverty rates are calculated for the sample of households with men, women, and children. Individual poverty rates are based on predicted resource shares for women, men, and children. Results for child poverty are presented for three equivalence scales: (i) the same as an adult (no adjustment), (ii) 60 percent of an adult, and (iii) 30 percent of an adult. Smooth lines are based on locally weighted regressions of individual poverty on per-capita household poverty.

Figure 3.A3: Sensitivity of macro cross-validation: individual poverty vs. nutrition for different poverty lines and child needs



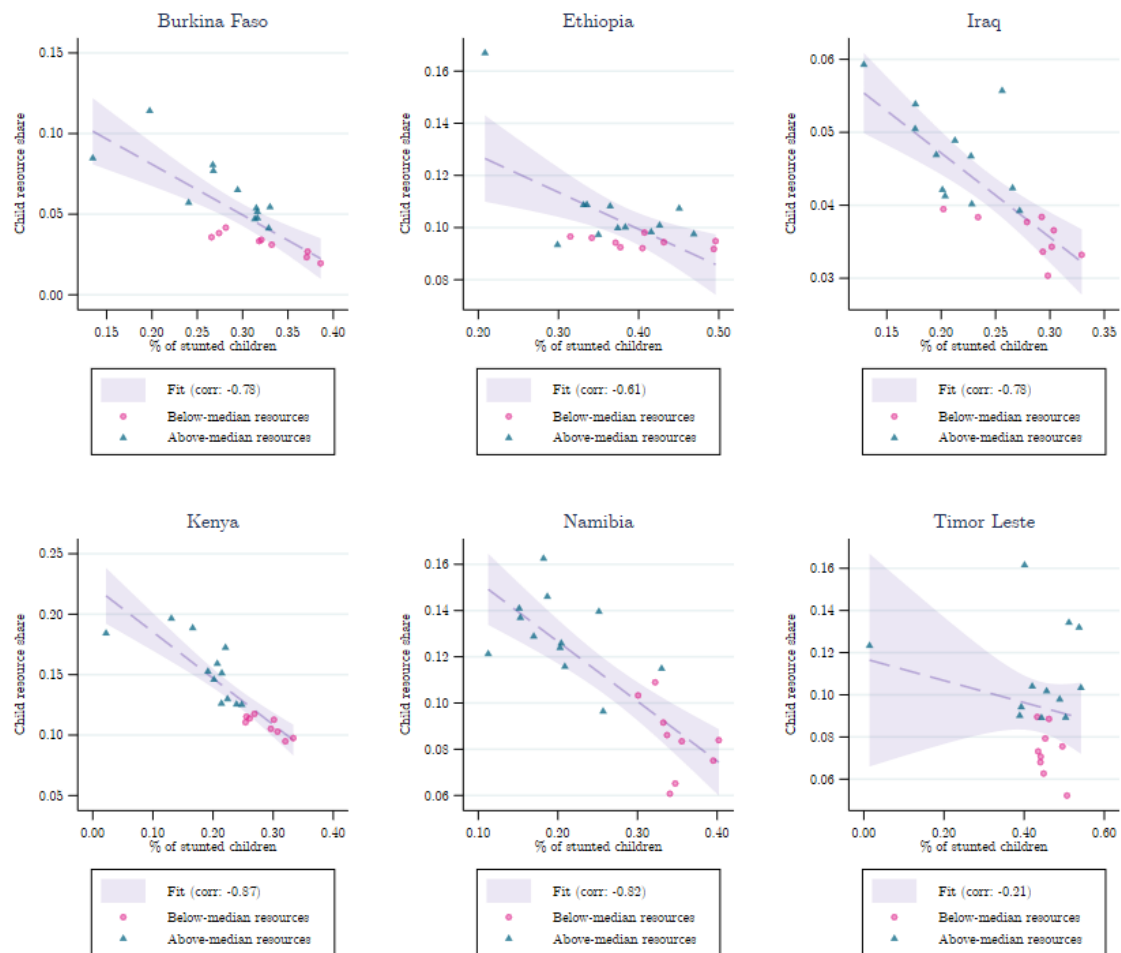
Source: authors' estimations using WHO statistics on the prevalence of child wasting and the data from household expenditure surveys for 33 countries. Notes: Graphs illustrate the correlation between the prevalence of child wasting (WHO) and individual (based on predicted resource shares for children) and household (based on per-capita resources) poverty. Panels (a) and (b) are estimated based on an equivalence scale of 60% and panels (c) and (d) based on an equivalence scale of 100%. Panels (a) and (c) refer to a poverty line of \$1.9/day (2011 PPP) and Panels (b) and (d) to \$3.2/day (2011 PPP). Smooth lines with 95% CI are based on locally weighted regression of individual and household poverty on the prevalence of child wasting.

Figure 3.A4: Sensitivity of micro cross-validation: per-child resource shares vs. child stunting by living standards



Source: authors' estimations using the data from household expenditure surveys for six countries. Selection of countries are based on the availability of micro-level information on child anthropometrics in household survey data used for the estimation of resource shares. *Notes:* Graphs illustrate the prevalence of child stunting and per-child resource shares, averaged over 20 bins of per-capita household expenditure, by log household expenditure. Smooth lines are based on locally weighted regressions of child resource shares and the prevalence of child stunting on log household expenditure.

Figure 3.A5: Sensitivity of micro cross-validation: per-child resource shares and poverty vs. child stunting



Source: authors' estimations using WHO statistics on the prevalence of child stunting and the data from household expenditure surveys for six countries. Selection of countries are based on the availability of micro-level information on child anthropometrics in household survey data used for the estimation of resource shares. *Notes:* Graphs illustrate average per-child shares by the prevalence of child stunting averaged over 20 bins of per-capita household expenditure, differentiating between households with above- or below-median per-capita resources. Correlations between child resource shares and child stunting are reported in legends.

Chapter 4

Shouldering the Weight of Climate Change: Intra-household Resource Allocation After Rainfall Shocks*

4.1 Introduction

Accelerating pace of climate change has made extreme weather events more frequent all over the globe, with developing countries bearing the brunt of the burden. In low-income settings, where social protection is limited and disaster-resilience systems are weak, households' coping and consumption-smoothing strategies often fail during climate shocks, resulting in substantial contractions of income and consumption (Morduch, 1995; Townsend, 1995; Dercon, 2004; Fafchamps and Lund, 2003; Kazianga and Udry, 2006; Maccini and Yang, 2009; Janzen and Carter, 2019; Asfaw and Maggio, 2018). While previous research has mostly focused on household-level welfare impacts of such income shocks, little is known about their intra-household effects.¹ It is often assumed that everyone in a household perceives the shock similarly, but imperfect risk-sharing within household, which is commonly observed in poor families (Doss, 1996, 2001; Dercon and Krishnan, 2000; Duflo and Udry, 2004; Dubois and Ligon, 2011), may induce unequal welfare impacts among its members. Thus, understanding within-household response to

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¹Most of existing studies explore the effects of climate shocks on individual health or educational outcomes. For instance, when affected by drought, women tend to lose more body weight compared to men (Hoddinott and Kinsey, 2000; Hoddinott, 2006) possibly due to a relatively larger reduction in meal intake among women, (Serna, 2011), or become more vulnerable to health issues due to increased risky sexual activities (Swidler and Watkins, 2007; Dinkelman et al., 2008; Robinson and Yeh, 2011; Burke et al., 2015; Treibich et al., 2022). Among children, those who are born at the time of a natural disaster become less advantaged, in terms of health and educational outcomes, compared to their siblings who are born in normal times (Hoddinott and Kinsey, 2001; Alderman et al., 2006; Maccini and Yang, 2009; Currie and Vogl, 2012; Groppo and Kraehnert, 2016, 2017; Dinkelman, 2017; Lo Bue, 2019).

natural disasters is crucial to identify the most vulnerable individuals and develop effective interventions.

This paper attempts to fill this gap by exploring the impact of weather shocks on the allocation of resources within household, and consequently, on individual consumption and poverty. A direct assessment of this question is usually constrained by the scarcity of consumption data collected at the individual level. Related studies exploit individual nutrition or anthropometric data to investigate intra-household response to a climate shock (Dercon and Krishnan, 2000; Hoddinott and Kinsey, 2000; Hoddinott, 2006). Nevertheless, the recent extensions of collective household models offer the tractable way of recovering individual resource shares and their determinants using household-level consumption data (Bargain and Donni, 2012; Browning et al., 2013; Dunbar et al., 2013; Bargain, Donni and Hentati, 2022). These models have been applied to assess, for example, the role of cultural norms (Calvi and Keskar, 2021; Aminjonov et al., 2023) in intra-household resource sharing, the impact of positive income shocks (Tommasi, 2019; Sokullu and Valente, 2022) or negative labor market shocks (Bargain and Martinoty, 2019) on individual resource shares. In this paper, I suggest a further application to test whether exposure to a rainfall shock affects the way household resources are distributed among household members and how it may reflect on individual consumption and poverty.

I focus on rural households in Malawi to investigate intra-household effects of climate shocks. Malawi is one of the poorest countries in the world, with around around 70% of population living below the poverty line of \$2.5 per day (2017 PPP) (World Bank, 2023b). Most of the population live in rural areas and depend on subsistence farming, in particular, rain-fed crops such as maize, for their livelihoods. This certainly raises vulnerability to frequent natural disasters such as floods and droughts, which result in crop failures, increased food insecurity and poverty.² Implications of these extreme climate events in Malawi have been investigated in various contexts, often in relation with household welfare, agriculture, migration, gender or child outcomes (Fisher et al., 2010; Stevens and Madani, 2016; McCarthy et al., 2016; Asfaw and Maggio, 2018; McCarthy et al., 2018; Becerra-Valbuena and Millock, 2021; McCarthy et al., 2021; Caruso et al., 2022; Dessy et al., 2023), but not in terms of intra-household effects. Hence, the present study seeks to extend this previous research by examining intra-household response to rainfall shocks in Malawi.

I start with the estimation of a collective household model to retrieve the share of household resources (consumption) allocated to men, women and children, following the methodological framework suggested in Bargain and Donni (2012) and Dunbar et al. (2013). I pool four waves of household consumption data for Malawi and combine it with geocoded rainfall information from Vicente-Serrano et al. (2010) to determine which households are exposed to a rainfall anomaly (flood or drought) during growing seasons of 2009/10, 2012/13, 2015/16 and 2018/19. In the estimation model, this indicator of rainfall shock with spatial-temporal variation is originally

²For instance, droughts tend to increase poverty by 1.3 percentage points, rising to 17 percentage points if drought is extreme (i.e. 1-in-25 year drought) (World Bank, 2023a).

introduced as a determinant in resource share functions, along with fixed effect terms to control for spatial and time differences among households.

I find that an exposure to rainfall shock during agricultural growing season reduces the share of resources allocated to women. The magnitude is, on average, around 5 percent decrease in per woman resource shares. I find no change in children’s resources, hence, implying that post-disaster redistribution of resources within household takes place between men and women. This pattern of redistribution makes men’s individual consumption and poverty relatively more incompressible when households are hit by a rainfall shock, while exacerbating the negative income effect of the climate hazard for women. Heterogeneity analyses with respect to gender gap in employment suggest that the redistribution of resources from women to men after a negative income shock is likely driven by ‘life-boat’ ethics, i.e. a larger share of resources directed to more productive household members (Pitt et al., 1990). In line with this hypothesis, I find a suggestive evidence of pro-boy bias in post-disaster resource allocation within household. Finally, I provide a weak evidence that cultural practices favoring women’s roles such as matrilineality or matrilocality may help to attenuate, to a certain extent, the adverse intra-household impacts of climate shocks on women.

To my knowledge, this is the first study to quantify the impact of extreme climate events on resource allocation within household and *individual* consumption, complementing previous studies that focus on intra-household distribution of nutrition after climate shock (Dercon and Krishnan, 2000; Hoddinott and Kinsey, 2000; Hoddinott, 2006). From policy perspective, this contributes to identifying and targeting the most vulnerable or ‘newly’ poor individuals, rather than households, by taking into account both inter-household and intra-household effects of natural disasters (Skoufias, 2003). Further, the results of this paper also illustrate that a part of gendered impacts of climate change, which have been well-documented using other measures of individual well-being, is likely driven by within-household response to natural hazards. Finally, this paper adds to a series of empirical studies that apply the recent extensions of collective models in various contexts.

The rest of the paper is structured as follows. Section 2 presents empirical approach, the identification strategy and describes the data. Section 3 presents estimation results and Section 4 provides concluding remarks.

4.2 Empirical Approach

4.2.1 Identification of Resource Allocation Process

Collective Models and Sharing Rule. The methodology applied in this paper builds on the broad literature of collective household models. The core idea in these models is that households make decisions through the bargaining process (Chiappori, 1988; Bourguignon and Browning, 1991). Originally, collective models assume that households decisions are efficient - the as-

sumption that allows decentralization of household decision-making process. The outcome of this process is a sharing rule, that is, the way household resources are distributed among its members. After household resources are allocated, each individual makes his/her own decisions based on available resources and preferences (Chiappori, 1992). Recent methodological extensions have suggested ways to identify sharing rule using household-level consumption data both for childless couples (Lewbel and Pendakur, 2008; Browning et al., 2013) and for couples with children (Bargain and Donni, 2012; Bargain, Donni and Hentati, 2022; Dunbar et al., 2013). Identification in these methods requires additional assumptions and extra information related to preferences (e.g. preference stability, using data on singles). In this paper, I employ similar approach with the assumption that total household expenditure is shared among its members based on some rule, but without imposing the efficiency assumption that is often regarded in the collective model literature as the common way to justify decentralization of decision-making, and yet, not the only one that can explain intra-household allocation process.³

Sharing Rule. I start by assuming that the allocation of household resources is determined by a *sharing rule*. Denote x the log of total private expenditure and $\eta_{i,s}(z^r)$ the share of total private expenditure $\exp(x)$ accruing to each individual of type $i = f, m, c$, i.e. women, men and children, in a household of composition s . Household composition is characterized by the numbers of individuals in each of the three groups denoted by s_f, s_m and s_c respectively and stacked in vector $s = (s_f, s_m, s_c)$. Under the sharing rule, each household member of type i in a family of composition s receives, in log terms, $x_{i,s} = x + \ln \eta_{i,s}$ as her own private resources. Note that in a complex household, for example with several adult women and men, this approach allows identifying only the total resource share of each group $s_i \times \eta_{i,s}(z^r)$, and not the resource shares among the persons of each type. Hence, $\eta_{i,s}$ represents a per-person resource share of individual type i .⁴ Resource shares depend on a set of determinants in vector z^r , which include (i) household demographic characteristics (e.g. the number and average age of men, women and children, the proportion of boys among children); (ii) an indicator of exposure to rainfall shock (in binary or continuous form); and (iii) grid fixed effects (one grid corresponds a spatial area of approximately 50 km² at which rainfall is measured) to account grid-level differences among households, and survey wave fixed effects to account for general time-related factors.⁵

Structural Engel Curves at Individual and Household Level. Following Bargain and Donni (2012) and Dunbar et al. (2013), I adopt a semi-parametric identification based on the assumption of ch4eq:q1 indirect utility function (Deaton and Muellbauer, 1980a). This

³Efficiency is often questioned in the context of poor countries (see Baland and Ziparo, 2018). Although empirical rejections relates to production decisions (e.g. Udry, 1996) rather than consumption, efficiency is more defensible in the case of frequently decisions with less of a strategic content, such as daily consumption (see Baland and Ziparo, 2017). Also note that using the data from Bangladesh, Lewbel and Pendakur (2022) show that the departure from efficiency leads to relatively small variation of resource sharing estimations.

⁴To estimate the resource shares of, for example, young women vs. old women among the $i = f$, the model would need expenditure data on exclusive goods related each subgroup (i.e. expenditure on young women's clothing vs. old women's clothing). While this is a data limitation, one can control in the sharing rule for specific variables such as age of women (as done in Calvi (2020)) to investigate gender bias in sharing rule (cf. Bargain et al. (2014) or Dunbar et al. (2013)).

⁵They also depend on prices but time variation in market prices is captured by survey wave fixed effects

approach yields individual Engel curves that are linear in the logarithm of individual resources. Namely, for a good k consumed by any person of type i , the *individual budget share* is written as:

$$w_{i,s}^k = \alpha_{i,s}(z^p) + \beta_{i,s}(z^p) \cdot x_{i,s}(z^r), \quad (4.1)$$

with z^p preference shifters and z^r sharing rule determinants. The identification requires the presence of exclusive goods, that is, goods consumed only by specific type of individuals. Denote these goods k_c, k_f, k_m for children, women, and men respectively. For example, if k_f corresponds to female clothing, a woman living in a household composition of s spends $w_{f,s}^{k_f}$ share of her resources $\exp(x_{i,s})$ on clothing. As a function of (log) individual expenditure, the equation 4.1 defines individual Engel curves. With the structure placed on individual demand, household Engel curves can also be retrieved. Multiplying individual Engel curve $w_{i,s}^{k_i}$ by resource share $\eta_{i,s} = \exp(x_{i,s})/\exp(x_s)$ and the number of persons of type i would show the level of spending on good k_i as a fraction of total household expenditure (i.e. family budget share): $W_s^{k_i} = s_i \cdot \eta_{i,s} \cdot w_{i,s}^{k_i}$. Given that family budget shares are usually observed in standard expenditure surveys, I can write a system of household budget shares for exclusive goods of women, men and children:

$$\begin{aligned} W_s^{k_c} &= s_c \cdot \eta_{c,s}(z^r) \cdot (\alpha_{c,s}(z^p) + \beta_{c,s}(z^p) \cdot (x + \ln \eta_{c,s}(z^r))) \\ W_s^{k_f} &= s_f \cdot \eta_{f,s}(z^r) \cdot (\alpha_{f,s}(z^p) + \beta_{f,s}(z^p) \cdot (x + \ln \eta_{f,s}(z^r))) \\ W_s^{k_m} &= s_m \cdot \eta_{m,s}(z^r) \cdot (\alpha_{m,s}(z^p) + \beta_{m,s}(z^p) \cdot (x + \ln \eta_{m,s}(z^r))). \end{aligned} \quad (4.2)$$

Restrictions and Identification of Resource Shares. The identification strategy here aims at retrieving the key elements in the above system from the reduced-form estimation of family budget shares on log household expenditure. First, as resource shares add up to one, one of the resource shares can be rewritten as a complement to the rest. For instance, if I choose men's resource share as 'residual', I can rewrite it as: $s_m \eta_{m,s} = 1 - s_c \eta_{c,s} - s_f \eta_{f,s}$. Consequently, the derivatives of the system in the equation (4.2) with respect to log household expenditure are:

$$\begin{aligned} \partial W_s^{k_c} / \partial x &= s_c \cdot \eta_{c,s}(z^r) \cdot \beta_{c,s}(z^p) \\ \partial W_s^{k_f} / \partial x &= s_f \cdot \eta_{f,s}(z^r) \cdot \beta_{f,s}(z^p) \\ \partial W_s^{k_m} / \partial x &= s_m \cdot (1 - s_c \cdot \eta_{c,s}(z^r) - s_f \cdot \eta_{f,s}(z^r)) \cdot \beta_{m,s}(z^p) \end{aligned} \quad (4.3)$$

for each s out of total S household compositions. The left-hand derivatives are observed and the system has $3S$ equations and $5S$ unknowns ($\eta_{c,s}, \eta_{f,s}, \beta_{c,s}, \beta_{f,s}$ and $\beta_{m,s}$ for each s). For the identification of resource shares, one needs to put additional restriction on preference term β . I employ the Similarity Across People (SAP) assumption as suggested by [Dunbar et al. \(2013\)](#). This assumption implies that the shape of individual Engel curves for exclusive goods is similar across individual types, which formally yields: $\beta_{c,s} = \beta_{f,s} = \beta_{m,s} = \beta_s$ for each $s > 0$. Thus, it

provides an exact identification with $3S$ unknowns ($(\eta_{c,s}, \eta_{f,s}, \beta_s$ for each s) and $3S$ equations.⁶

4.2.2 Specification and Estimation Method

Specification. As specified in the equations 4.1 and 4.2, the semi-parametric approach based on ch4eq:eq1 preferences provides the log-linear specification of household Engel curves. To estimate the model, I add error terms $\epsilon_{i,s}$ to each household Engel curve in the demand system in 4.2 and impose the SAP restriction as follows:

$$\begin{aligned} W_s^{k_c} &= s_c \cdot \eta_{c,s}(z^r) \cdot (\alpha_{c,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{c,s}(z^r))) + \epsilon_{c,s} \\ W_s^{k_f} &= s_f \cdot \eta_{f,s}(z^r) \cdot (\alpha_{f,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{f,s}(z^r))) + \epsilon_{f,s} \\ W_s^{k_m} &= s_m \cdot \eta_{m,s}(z^r) \cdot (\alpha_{m,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{m,s}(z^r))) + \epsilon_{m,s} \end{aligned} \quad (4.4)$$

with

$$s_m \cdot \eta_{m,s}(z^r) = 1 - s_c \cdot \eta_{c,s}(z^r) - s_f \cdot \eta_{f,s}(z^r)$$

Engel curve parameters $\alpha(z^p)$ and $\beta(z^p)$ vary linearly with a set of preference shifters z^p . These variables include household composition indicators (s_f, s_m, s_c). Similarly, resource shares $\eta_{f,s}(z^r)$ also take a linear form with a set z^r variables that include: (i) variables in z^p ; (ii) other household demographic factors, including average age of children, men, and women, and the proportion of boys; (iii) a variable measuring rainfall shock (binary in the baseline); and (iv) vectors of grid and survey wave fixed effects to account for differences across grids and time.⁷

The estimation of the system in equation 4.4 is computationally demanding, especially, in the presence of fixed effects and multiplicative terms that generate interactions between variables in z^p and z^r . For instance, with four regressors in z^p (including constant term) and 58 variables in z^r (including grid and time fixed effects), the multiplicative term $\eta_{i,s} \cdot \beta_s$ would generate 232 parameters to be estimated in each Engel curve. To ease the burden on estimation process, I introduce a simplification in the estimation, which takes advantage one of the features of the SAP assumption. Let the Engel curve for total clothing in household be given by $W_s = \sum_i W_s^{k_i}$. Then, given the SAP and that the resource shares add up to one, the derivative of this total

⁶In a series of tests using directly observed resource shares, [Bargain, Lacroix and Tiberti \(2022\)](#) tend not to reject SAP.

⁷Alternatively, one could specify resource share functions in logistic form that would ensure that resource shares are bounded in $[0, 1]$ (e.g in [Bargain and Donni, 2012](#); [Bargain et al., 2014](#); [Bargain and Martinoty, 2019](#)). Yet, in the presence of moderately high-dimensional fixed effects, this would add additional non-linearity in the model, making it computationally hard to estimate the parameters of interest. Results with linear function show that the fraction of resource shares outside $[0, 1]$ is negligible.

Engel curve with respect to log expenditure is:

$$\begin{aligned}
\partial W_s / \partial x &= \sum_i \partial W_s^{k_i} / \partial x \\
&= \sum_i s_i \cdot \eta_{i,s}(z^r) \cdot \beta_s(z^p) \\
&= \beta_s(z^p) \sum_i s_i \cdot \eta_{i,s}(z^r) \\
&= \beta_s(z^p)
\end{aligned} \tag{4.5}$$

This implies that preference term $\beta_s(z^p)$ can simply be obtained by estimating the Engel curve for total clothing:

$$W_s = \alpha_s(z^p) + \beta_s(z^p)x + \epsilon_s \tag{4.6}$$

with $\alpha_s(z^p)$ as an approximation for the rest of terms arising from $\sum_i W_i^{k_i}$ based on the system 4.4. Thus, one can simplify the estimation process and identification of resource shares either by pre-estimating $\beta_s(z^p)$ and then ‘plugging’ into the demand system in equation 4.4, or replacing one of three Engel curves in the system 4.4 with the one for total clothing (i.e. W_s). For example, if I replace Engel curve for men’s good by total Engel curve (as men’s resource shares are taken as ‘residual’), the system becomes:

$$\begin{aligned}
W_s^{k_c} &= s_c \cdot \eta_{c,s}(z^r) \cdot (\alpha_{c,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{c,s}(z^r))) + \epsilon_{c,s} \\
W_s^{k_f} &= s_f \cdot \eta_{f,s}(z^r) \cdot (\alpha_{f,s}(z^p) + \beta_s(z^p)(x + \ln \eta_{f,s}(z^r))) + \epsilon_{f,s} \\
W_s &= \alpha_s(z^p) + \beta_s(z^p)x + \epsilon_s
\end{aligned} \tag{4.7}$$

Intuitively, the third equation in this model would ‘feed’ the other two with the estimates for $\beta_s(z^p)$ which accommodates the identification of resource shares $\eta_{i,s}(z^r)$.⁸ Similar approach is also suggested by Lechene et al. (2022) but only as a pre-test of the model identification (i.e. testing if $\beta_s(z^p) = 0$).

Effect of Rainfall Shock on Resource Sharing. To estimate the effect of rainfall shock on resource shares, this analysis exploits spatial and temporal variation in the level of rainfall, combined with repeated cross-sectional survey data. The baseline measure of rainfall shock used here records the degree of rainfall, for a grid equivalent to geographical area of approximately 50 km², during *the growing period of maize* - the most prevalent rainfed crop in Malawi. This generally corresponds to the period from mid-November to mid-April.⁹ Recall that the fieldwork in each wave of the survey started around April and lasted about 12 months. Such timing of survey fieldwork allows exploring whether households re-distribute resources internally *after*

⁸Note that as soon as $\beta_s(z^p)$ is retrieved using total Engel curve 4.6 and $\beta_s(z^p) \neq 0$, resource shares can also be retrieved by estimating Engel curves for each individual type i one by one, without combining them into a system of equations.

⁹There are minor differences in the growing period across agro-climatic zones, but on average, they fall into the period from November to April. For further details about agricultural periods, please check FAO Crop Calendar <https://cropcalendar.apps.fao.org>.

rainfall shock in the growing season. To do so, I combine, for each wave the survey, a rainfall shock indicator that reflects the level of rainfall in the growing period *preceding* the start of the survey fieldwork in that wave. For example, for all households of the wave 2010/11 (i.e. interviewed starting April 2010), the variable of rainfall shock indicates what happened in the period November 2009-April 2010. Denoting $R_{g,t}$ as an indicator of rainfall shock in grid g and survey wave t , I can write the baseline resource share equation as:

$$\eta_{i,s}(z^r) = \sigma z^d + \delta R_{g,t} + \phi_g + \lambda_t \quad (4.8)$$

with $z^r = \{z^d, R_{g,t}, \phi_g, \lambda_t\}$, z^d a vector of demographic variables (including preference shifters z^p), ϕ_g and λ_t grid and survey wave fixed effects respectively. Grid fixed effects ϕ_g control for time-invariant differences across grids, and survey wave fixed effects λ_t capture general time-related disparities across survey waves. The identification of rainfall shock's effect on individual resource shares is based on the assumption that rainfall shocks, as with any natural disaster, occur more or less in a random way in terms of timing and geographical location. After eliminating overall differences across grids and time using fixed effect terms, δ estimates the average effect of rainfall shock on resource shares of individual type i . Notably, as the data collection lasted 12 months, one may obviously expect that the effect δ may vary by timing of interview within each wave (e.g. it might be smaller for those who were interviewed at the end of fieldwork, and vice versa). This will be investigated further in robustness checks.

Estimation Procedure and Endogeneity. As the error terms of the model are likely to be correlated across equations, each system is estimated using Non-Linear Seemingly Unrelated Regressions (as, for instance, in [Calvi and Keskar, 2021](#); [Bargain, Lacroix and Tiberti, 2022](#), or [Aminjonov et al., 2023](#)). To correct for the likely correlation between the error terms in each budget-share function and the log total expenditure, each budget share equation is augmented with the Wu-Hausman residuals (see [Banks et al., 1997](#); [Blundell and Robin, 1999](#)). I obtain these residuals from reduced-form estimations of x on all exogenous variables used in the model plus some instruments such as a quadratic form of the log household income.

4.2.3 Data Sources, Main Variables and Sample Selection

Malawi Integrated Household Survey. In this study, I mobilize a household survey data that pools four waves of Malawi Integrated Household Survey (IHS) conducted in 2010/11, 2013/14, 2016/17 and 2019/20. These survey series are a part of the Living Standards Measurement Study - Integrated Surveys on Agriculture (LSMS-ISA) project that aims at improving the quality of agricultural data in Sub-Saharan Africa and ultimately contributing to research on the link between agriculture and poverty reduction in the region. All waves of Malawi IHS record detailed information on household consumption and socio-demographic characteristics. I use total as well as categorized expenditure variables provided by the World Bank, which aggregate household spending on food and non-food products. Most importantly, the survey collects data on clothing expenditure separately for children, women and men. Another essential feature of

this data is that it has information on (approximate) geographic location of households, which allows combining spatially household survey data with geocoded rainfall information.¹⁰

Expenditure Data on Exclusive Goods. As discussed above, the identification of resource shares requires spending data on private assignable goods. I use clothing as an exclusive household expenditure. Clothing has become a common choice of assignable good in the literature of collective models (e.g. in [Browning et al., 1994](#); [Bourguignon et al., 2009](#), and the applications in [Dunbar et al., 2013](#); [Bargain and Donni, 2012](#); [Bargain et al., 2014](#); [Bargain, Lacroix and Tiberti, 2022](#); [Aminjonov et al., 2023](#)) as standard household surveys usually distinguish children’s, women’s and men’s clothing expenses.

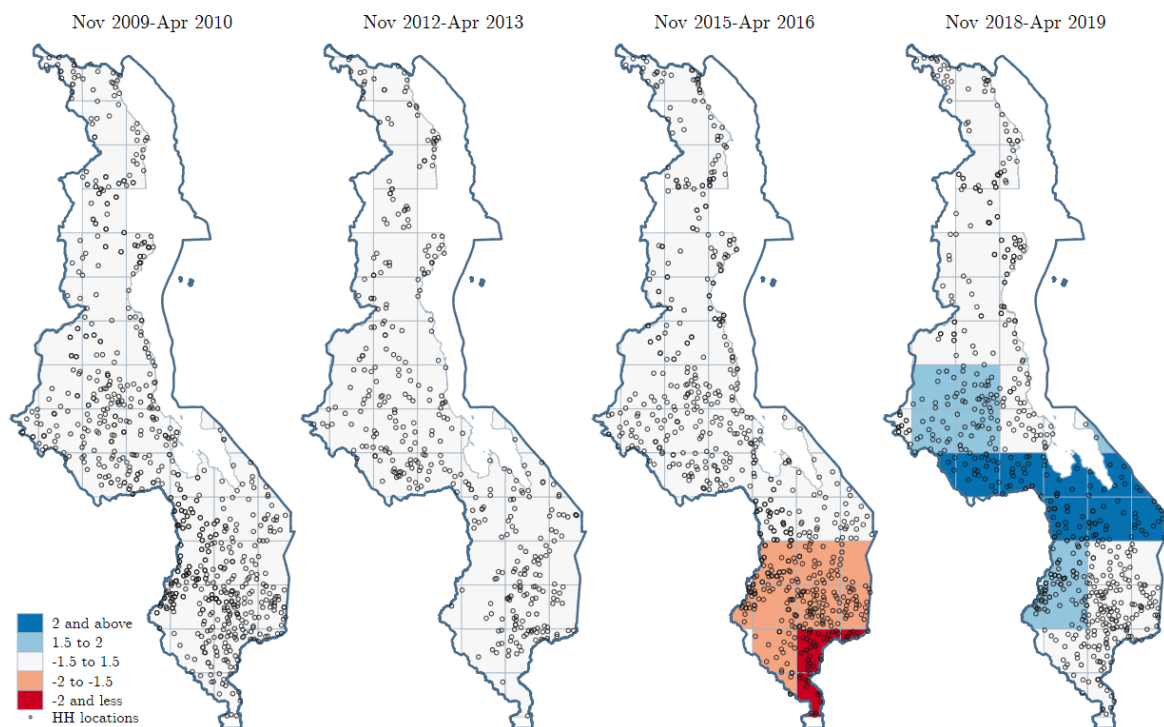
Sample Selection. The main objective of this paper is to investigate whether climate shocks affect intra-household allocation of resources and provide insight on the implications for individual poverty as broadly as possible. Considering this nature of the analysis, I impose a following set of restrictions. First, I restrict my sample to households composed of at least men and women (hence, excluding households with only men, or women, households with women and children, and households with men and children) to capture, if any, gendered feature of resource allocation. Second, I focus on families with up to four men/women and eight children, which represent around 99% of all households. Note that most of earlier studies investigated resource sharing in nuclear households ([Bargain and Donni, 2012](#); [Dunbar et al., 2013](#); [Bargain et al., 2014, 2018](#)). However, the recent applications of this approach ([Calvi and Keskar, 2021](#); [Brown et al., 2021](#); [Bargain, Lacroix and Tiberti, 2022](#); [Aminjonov et al., 2023](#); [Calvi et al., 2023](#)) focus more on complex households, especially in the context of developing countries where families tend to live in large extended households. Third, I discard households where any adult is older than 65 to ensure that only economically active adults are part of the analysis. Moreover, since I use repeated cross-sectional survey data, not panel, I keep only grids for which there is at least one observation per wave so that the number of grids ‘observed’ in each wave is the same. Households for whom basic information on consumption and demographics is missing, and for whom rainfall information cannot be obtained (due to missing GPS data) are also excluded. Also, I trim the top one percent of clothing budget shares and total household consumption to minimize measurement error and ensure a smoother estimation. Finally, I exclude households from urban areas as rainfall shock is more likely to directly affect households in rural areas, whose livelihoods depend on farming, and specifically, on rainfed crops. The final pooled sample comprises 13,863 households.

Measuring Rainfall Shocks. To detect rainfall anomalies, I use the Standardized Precipitation-Evapotranspiration Index (SPEI) suggested by [Vicente-Serrano et al. \(2010\)](#). This is a multiscalar index that shows how the level precipitation moves, in terms of standard deviation, with respect to its historical average for a selected area. For instance, a SPEI value of -1.5 implies that the level of rainfall is 1.5 standard deviation below than historical average for the given area. An important advantage of this index is that it takes into account, unlike other

¹⁰Note that GPS data is provided at the level of community where households live.

standard rainfall or drought measures, potential evapotranspiration (movement of water from earth’s surface into the atmosphere via evaporation and transpiration). I employ the data from SPEI Global Drought Monitor that measures SPEI at the level of a grid with a spatial resolution of 0.5 degrees in latitude and longitude, which corresponds approximately to an area of 50 km².¹¹ As I focus on the rainfall level during the growing period of maize in Malawi, I use the 6-month SPEI measured for the period from November to April and combine it spatially with the household survey data using the information on geographic location of households’ residence. To simplify interpretation in the analysis, I use dichotomized version of the SPEI index as a baseline. Namely, I define the occurrence of rainfall shock, in binary form, if an absolute value of SPEI is equal or larger than 1.5 ($|SPEI| \geq 1.5$), in other words, if rainfall level is 1.5 standard deviations below or above historic mean.¹² As a sensitivity check for the choice of cutoff, I estimate the baseline model at different levels of rainfall deviations (i.e. categorized version of SPEI) or directly using absolute value of SPEI in continuous form.

Figure 4.1: Grid-level SPEI for the period November-April (growing season) in Malawi.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor.

Figure 4.1 illustrates the spatial variation of SPEI across grids during growing seasons of 2009/10, 2012/13, 2015/2016 and 2018/2019. In 2009/10 and 2012/13, there were no rainfall shocks as per the definition above. Severe and extreme rainfall shocks were recorded in

¹¹For further information on SPEI, see Vicente-Serrano et al. (2010) or <https://spei.csic.es/home.html>

¹²Note that there is no general consensus on the choice of threshold. Researchers often use one or two standard deviations from the historic average to define rainfall anomaly (Rose, 2001; Marchetta et al., 2019; Dessy et al., 2023; Caruso et al., 2022).

2015/16 and 2018/19 growing season in the southern and central parts of Malawi. Note that two first waves are included in the analysis, despite no rainfall shocks, to account for grid-level differences in the absence of rainfall anomalies, and also to increase the number of observations, which certainly improves convergence in the estimations and the identification of resource shares.

4.2.4 Descriptive Statistics

Table 4.A1 documents mean and standard deviation of the variables used in the estimation of resource sharing model. Column (1) reports statistics for households with children, women and men, and column (2) for households with women and men. Rainfall level, hence the exposure to rainfall shocks, is similar in both groups. Average budget shares of women’s and men’s clothing are also similar in both family compositions. Note that the infrequency of clothing purchases is not necessarily an issue for the estimation (Dunbar et al., 2013). Table 4.A2 further compares the characteristics of households who are exposed and not exposed to rainfall shock. On average, both groups of households are similar, except noticeable differences in cultural traits of lineage and locality. Majority of ‘affected’ households are matrilineal or matrilineal+matrilocal. Note that this difference would be captured, to certain extent, by grid fixed effects in the estimation. However, it poses a limitation in potential heterogeneity analysis across cultural groups by reducing the necessary statistical power in terms of cultural variation within ‘affected’ households. Further, the gap in household consumption is expected as a general negative income simply reflects the negative income effect of rainfall shocks.

4.3 Results

4.3.1 Baseline Estimations

Predicted Resource Shares. I start with presenting the estimated resource shares in Table 4.1. Column (1) presents results for households with children, women and men, and column (2) for households with women and men. Panel (a) of the table reports average per-person resource share for children, women and men. Men tend to get the largest share of resources in the household, with an average of 36% and 57% of consumption in households with and without children respectively, while women receive around 32%-35% of family resources. Children consume, on average, 13% of household resources, which is also in line with the related literature. This pattern of resource allocation is in line with similar studies for Malawi (Dunbar et al., 2013; Penglase, 2021; Lechene et al., 2022; Aminjonov et al., 2023), as well as for other African countries (e.g. Bargain et al., 2014, Bargain et al., 2018). The rows at the bottom report ‘validation’ statistics for the estimated resource shares and the share of households affected by rainfall shock in each survey wave. As I use simple linear resource sharing function, predicted resource shares may be outside $[0, 1]$. Reassuringly the share of such cases are close to zero in both family settings. Another important test for the model applicability is to check whether households have flat Engel curves for clothing, which may prevent the identification of resource shares. For all households in the sample, the estimated values of the slopes β of individual

Engel curves are statistically significantly different from zero.

Table 4.1: Predicted resource shares and the effect of rainfall shock.

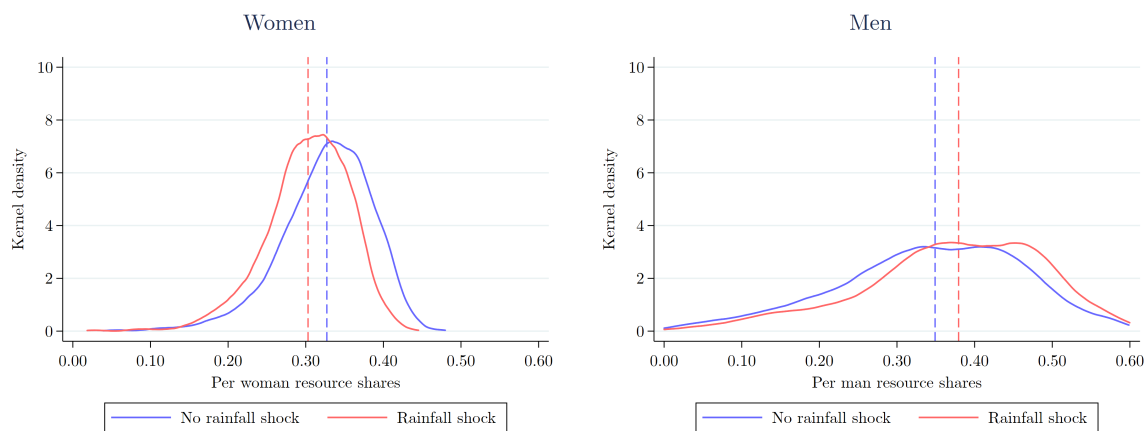
	HHs with children, women and men	HHs with women and men
	(1)	(2)
(a) Estimated resource shares:		
Per child	0.126 (0.031)	- -
Per woman	0.322 (0.058)	0.354 (0.056)
Per man	0.356 (0.126)	0.565 (0.149)
(b) Effect of rainfall shock on per-person resource shares:		
Children's resources	0.001 (0.003)	- -
Women's resources	-0.017 ** (0.008)	0.004 (0.016)
Observations	12,529	1,334
% of HHs with resource shares outside [0,1]	0.005	0.000
% of HHs with non-flat Engel curve	1.000	1.000
% of HH affected by rainfall shock, overall	0.234	0.244
% of HHs affected by rainfall shock, 2009/10 season	0.000	0.000
% of HHs affected by rainfall shock, 2012/13 season	0.000	0.000
% of HHs affected by rainfall shock, 2015/16 season	0.376	0.387
% of HHs affected by rainfall shock, 2018/19 season	0.440	0.437

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Panel (a) shows average predicted resource shares. Standard deviations in parantheses. Panel (b) shows the effect of rainfall shock ($|SPEI| \geq 1.5$) on per child and per woman resource shares. Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects and survey wave fixed effects. Standard errors clustered at the grid level in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

Effect of Rainfall Shock on Resource Shares. Panel (b) of Table 4.1 reports estimates for the effect of rainfall shock δ on per-person resource shares of children and women. The exposure to a rainfall shock in the agricultural growing season of maize is associated with a lower proportion of household resources accruing to women in households with children. Relative to average shares reported in panel (a), the effect corresponds to a 5.3 percent decrease in per-woman resource shares. However, women's shares in households without children seems to remain unaffected. Given that I do not find any effect of rainfall shock also on per-child resource shares, rainfall shock seems to induce the redistribution of resources from women to men in households with children. Figure 4.1 illustrates this shift in resource allocation from women to men by comparing the full distribution of their respective resource shares in areas affected and unaffected by rainfall shock. This pattern of redistribution can be due to various factors. For instance, there may be changes in relative bargaining power within household upon exposure to rainfall shock (e.g. if women's assets are affected more severely than men's) that may have

impacted consumption decisions (Doss, 2001; Duflo and Udry, 2004; Quisumbing et al., 2018). At the same time, this could simply be driven by productivity-related factors. Households may channel a larger share of resources to members with higher marginal productivity (Pitt et al., 1990; Dercon and Krishnan, 2000; Estudillo et al., 2001; Coates et al., 2018), or as an incentive or nutritional investment to induce off-farm employment within household when there is a crop failure (Dubois and Ligon, 2011). This is often referred as ‘life-boat’ ethics, which can especially amplify in times of economic distress, for example, due to a natural disaster. In the following sections, I test this hypothesis using heterogeneity analysis with respect to relative employment of women and men.

Figure 4.2: Distribution of women’s and men’s resource shares by exposure to rainfall shock.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Graphs illustrate Kernel density of predicted per-woman and per-man resource shares. Vertical lines represent median resource shares for households affected and unaffected by rainfall shock.

Implications for Individual Welfare. With predicted individual resource shares presented above, I further calculate the level of individual consumption (by applying resource shares to total household consumption), which is also used later to identify individual poverty status. Table 4.2 reports the level of per-person daily consumption (in 2011 PPP dollars) for each demographic cell by households’ exposure to rainfall shock. Column (1) shows average individual consumption for households not affected by rainfall shock, and column (2) for those affected by rainfall shock. Column (3) presents the simple difference between these two averages. Overall, the pattern of resource shares from Table 4.1 reflects into individual consumption levels: men generally tend to consume more than women, while children’s consumption is much lower than adults’. Yet, when considering differences between households affected and unaffected by rainfall shock, women tend to lose substantially more in terms of consumption than children or men. In households with children and affected by the shock, women consume around 17% less compared to households not exposed to the natural disaster, while men and children consume approximately 7% and 3% less respectively. In households without children, the intra-household effect of the shock seems quite balanced, with around 10-11% loss in consumption both for women and men.

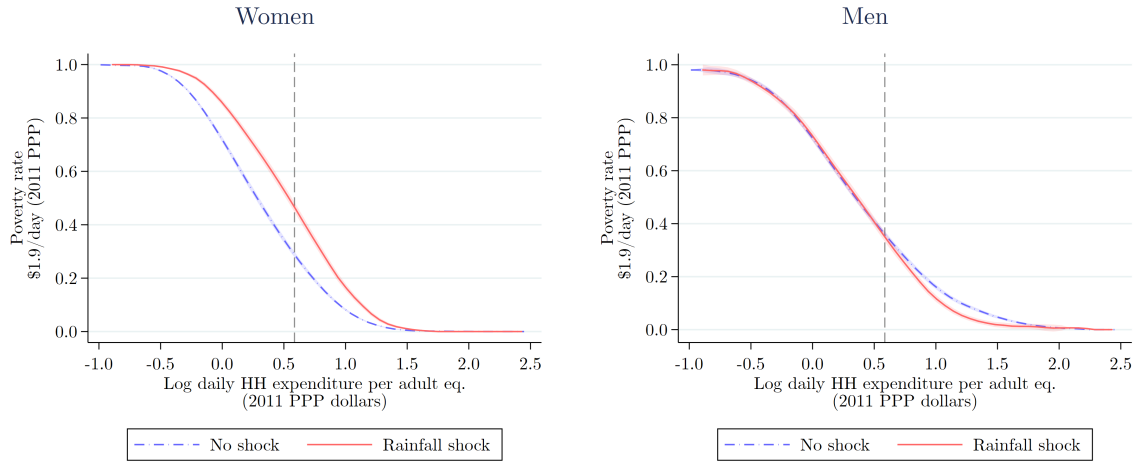
Table 4.2: Individual consumption by exposure to rainfall shock.

	No rainfall shock	Rainfall shock	Difference (2)-(1)
	(1)	(2)	(3)
Daily consumption in 2011 PPP dollars:			
(a) HHs with children, women and men			
Per child	1.026 (0.030)	0.956 (0.038)	-0.070 * (0.044)
Per woman	2.702 (0.064)	2.252 (0.088)	-0.450 *** (0.105)
Per man	2.862 (0.079)	2.787 (0.109)	-0.075 (0.137)
Observations	9,544	2,923	
(b) HHs with women and men			
Per woman	2.591 (0.084)	2.285 (0.123)	-0.307 * (0.166)
Per man	4.010 (0.125)	3.591 (0.130)	-0.419 ** (0.191)
Observations	1,008	326	

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Table reports average daily per person consumption in 2011 PPP dollars by exposure to rainfall shock and respective differences for the sample of households with children, women and men. Column (3) reports the results of t-test for the significance of difference in individual resource between shock-affected and unaffected households. Standard errors in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

Yet, the simple differences in individual consumption capture both income and redistribution effect of rainfall shocks on individual consumption. To separate these two effects, I conduct a counterfactual analysis where I check the patterns of individual consumption if there was no rainfall shock. First I estimate the effect of rainfall shock on (log) total household consumption, which represents the average income effect common to all household members. I adjust household consumption by this estimated effect to calculate the counterfactual “no-shock” level of household consumption. Next, I re-estimate resource shares but now setting rainfall shock indicator to zero. Applying these shares to actual household consumption gives individual consumption with income effect but without redistribution effect of rainfall shock, and applying it to the counterfactual household consumption from the previous step provides the approximate value of individual consumption if there had been no shock. Results are reported in Table 4.A3. The estimated average income effect on consumption is around 12% reduction for all members. But this is further intensified only for women due to the redistribution. The loss in per woman consumption due to the redistribution accounts for almost 1/3 of the total reduction due to shock. Symmetrically, the re-allocation of resources tend to partially offset men’s losses in consumption, hence leading to relative smaller losses due to rainfall shock. Note that this exercise is suggestive and intended to elicit income and redistribution effects of rainfall shock on individual consumption.

Figure 4.3: Women’s and men’s poverty by exposure to rainfall shock.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Graphs illustrate local polynomial fit with 95% CI of poverty headcount ratio at the poverty line of \$1.9 (2011 PPP) a day across values of log total household expenditure per adult eq./day. Individual poverty rates are based on predicted resource shares for women and men. The median log expenditure is indicated by the vertical dashed line.

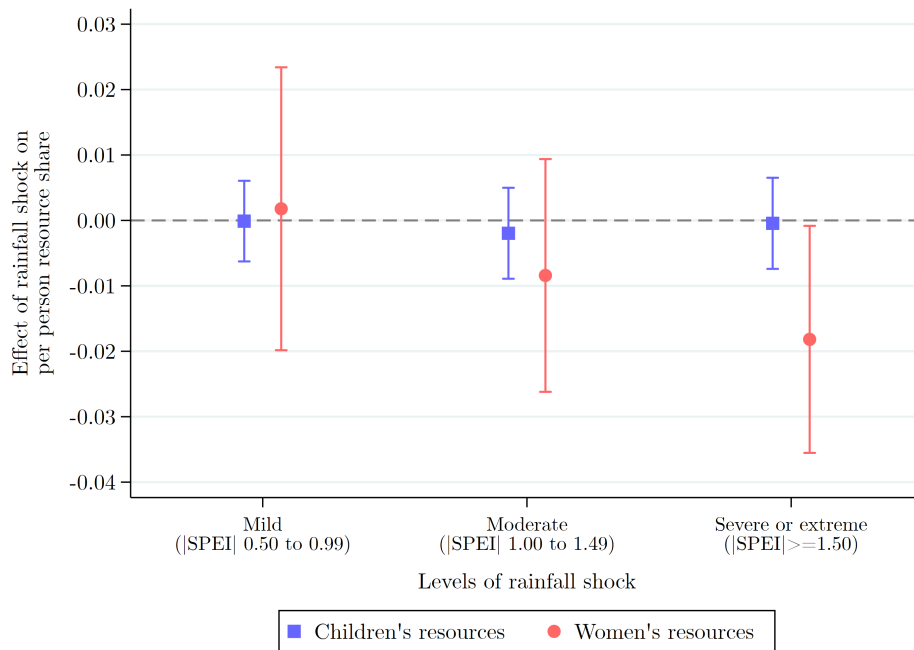
Finally, to complement the analysis above, I further look at poverty implications of the women-to-men resource redistribution in times of a rainfall anomaly. I define individual poverty for each demographic cell by comparing respective individual consumption with the standard poverty line of \$1.9 a day (2011 PPP), which is commonly used poverty threshold for low-income countries. Figure 4.3 plots individual poverty headcount rates for women and men living in households affected versus unaffected by rainfall shock at different values of per adult equivalent household expenditure. In line with the resource share patterns, women’s poverty tend to shift rightward across all levels of household expenditure. On the other hand, men’s poverty remains almost unchanged, but at the cost of women’s well-being. The gap in women’s poverty (around 17 points in poverty rate at the median household consumption) is relatively larger compared to the difference in individual consumption observed above. This might potentially be due to generally low level of living standards in Malawi, and as a result, many families living close to the poverty cutoff (so that even a small decrease in consumption levels may push many below the poverty line).

4.3.2 Robustness Checks

Intensity of Rainfall Shock. In the baseline estimations, I have used the binary indicator of rainfall shock to make interpretation simpler. To explore the variation of individual resource shares at different levels of rainfall deviations, I estimate the same resource sharing model but using a ‘categorical’ indicator of rainfall as a measure of rainfall shock intensity. Based on the interpretation of SPEI values often used in climatological studies (World Meteorological Organization, 2012; Nam et al., 2015), I define four levels of rainfall variation: (i) near normal/no shock ($|\text{SPEI}| < 0.5$), (ii) mild ($|\text{SPEI}| \in [0.5, 1)$), (iii) moderate ($|\text{SPEI}| \in [1, 1.5)$), and (iv)

severe/extreme ($|\text{SPEI}| \geq 1.5$). Figure 4.4 illustrates the estimated effects at the last three levels of rainfall shock, while using the first "near normal" level as a reference group. As expected, the farther rainfall deviates from its normal level, the less resources are allocated to women. With no effects on children's resource shares, men tend to obtain more resources with increasing intensity of rainfall shock. Figure 4.A1 illustrates the full distribution of women's and men's resource shares at different levels of rainfall shock.

Figure 4.4: Effect of rainfall shock by intensity.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. The graph shows the effect of rainfall shock by at different levels of intensity on per child and per woman resource shares, compared to near normal level of rainfall ($|\text{SPEI}| < 0.5$). Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects and survey wave fixed effects. Capped spikes illustrate 95% based on standard errors clustered at the grid level.

Alternative Measures of Rainfall. Further, I check sensitivity of the baseline results to the choice of rainfall measure as well as the cutoff used to define rainfall shock. First, I replicate the estimations but using the absolute SPEI value of 1 (i.e. one standard deviation far from the historic average) as the threshold to define a rainfall anomaly, instead of 1.5. Results for the sample of households with children, women and men are reported in Table 4.A4, column (2). I find a slightly smaller but still significant effect of rainfall shock with this cutoff than the baseline. This is not surprising given that a weaker effect observed between the absolute SPEI values of 1 and 1.5 in the previous part (Figure 4.4) is likely to pull down the effect of rainfall shock when the SPEI value of 1 is used as the cutoff. Next, I exploit full variation in absolute rainfall deviations and re-estimate the model using two continuous measures of rainfall. The first continuous measure is the absolute value of SPEI index that was used to define the baseline binary measure of rainfall. The second measure is based on the absolute values of a

high-resolution standardized precipitation index (SPI). This index is measured at 0.05-degree grids, which approximately corresponds to an area of 5x5km.¹³ For both indices, I define the absolute values below 0.5 as normal level and set them to zero, and the values equal to or above 0.5 are left as they are. Columns (3) and (4) of Table 4.A4 documents the estimation results using these two continuous measures of rainfall. With the continuous SPEI (column 3), I find statistically insignificant but still negative effect of rainfall deviations on women’s resource shares. The effects are stronger for both women and children’s shares with the high-resolution SPI (column 4). Despite weak magnitudes, both results are reassuring because they still tend to convey the same message as the baseline findings.

Timing of Survey Fieldwork and Agricultural Zones. The last pair of sensitivity checks is related to the timing of survey data collection and different agro-ecological zones in Malawi. As noted before, the data collection all waves of Malawi IHS starts in April and continuous for around 12 months. The rainfall information is retrieved for the period November-April preceding the start of the data collection. Thus, the effect of rainfall shocks on resource sharing might also depend on the timing of survey interview (e.g. the effect may start to fade away if the household data is collected at later periods of fieldwork). However, given that the identification of resource shares is based on clothing expenditure, which are relatively less frequent, estimating heterogeneous effects of rainfall shock by timing of survey interviews may not be viable. An alternative is simply to check the robustness of main estimates by controlling for the time of survey interview. I use the information on month of interview and agricultural season (i.e. planting, growing, harvesting) at the time of interview. Results are reported in columns (5) and (6) of Table 4.A4 respectively. In both cases, the effect of rainfall shock on women’s shares only slightly decreases in magnitude. Moreover, there are four agro-ecological zones in Malawi¹⁴, differences across which may also distort the effect of rainfall shock on resource shares. Results in column (7) of Table 4.A4, show that the baseline effects hold when controlling for differences among agro-ecological zone differences.

4.3.3 Discussion and Heterogeneity Analysis

Suggested Mechanism: Life-boat Ethics. As shortly discussed above, a widening gender gap in intra-household resource allocation due to rainfall shock can be driven by many factors. One of the most likely explanations is the distribution of resources based on needs: more productive household members are likely to consume a larger share of household resources, which is generally referred in the literature as ‘life-boat ethics’ (Pitt et al., 1990; Estudillo et al., 2001; Dubois and Ligon, 2011; Coates et al., 2018). In times of a economic distress, this can be even more intensified, leading to an increase in the share of resources accruing to members with relatively higher marginal productivity (Dercon and Krishnan, 2000). I suggest

¹³I construct high-resolution SPI using the precipitation data from Climate Hazards Group Infrared Precipitation with Stations (CHIRPS) of University of California at Santa Barbara and the R command `spi`. For the baseline, SPEI is preferred because it takes into account water evapotranspiration, i.e. the water movement from earth’s surface to atmosphere, along with precipitation, while SPI focuses only on precipitation level.

¹⁴These are (i) tropic-warm/semiarid, (ii) tropic-warm/subhumid, (iii) tropic-cool/semiarid, and (iv) tropic-cool/subhumid.

a simple test of this hypothesis by comparing the effect of rainfall shock on women’s resources in areas (grids) with higher and lower gender gap in employment indicators. To do this, I first calculate the distance between men’s and women’s employment at the grid level. For the sake of simplicity, I split grids into two groups with lower and higher gender gap in employment using the median level. Results are reported in Table 4.A5. Column (1) reiterates the baseline estimates. In the next columns, I consider heterogeneity across three employment indicators: (i) any type employment (columns 2 and 3), (ii) off-farm employment (columns 4 and 5), and (iii) hours spent on income-generating activities (columns 6 and 7). In columns (2), (4) and (6), I first check the sensitivity of the baseline estimates to controlling for gender gap in employment indicators as some of the effect of rainfall shock may also be captured by these variables.¹⁵ In all cases, controlling for the gender gap in employment indicators do not alter the effect of rainfall shock on women’s resource shares estimated at the baseline. Columns (3), (5) and (7) present heterogeneity effects by lower and higher level of gender gap in employment. Coefficient estimates in panel (b) reveal that the negative effect of rainfall shock on women’s resource share is concentrated in areas where men are more actively involved in employment or income generating activities compared to women. This suggests that the redistribution of household resources from women to men in times of an extreme weather event is likely induced, to certain extent, by a higher marginal productivity of men, the importance of which is obviously more accentuated during economic hardship.¹⁶

Pro-boy Bias. In the spirit of ‘life-boat’ ethics discussed above, I further check whether households exhibit any pro-boy bias in resource allocation as a part of intra-household strategy to cope with a climate shock. Here I simply compare the effect of rainfall shock on per child resource shares in households with only boys versus households with only girls or girls+boys. Table 4.A6 reports estimation results. Column (1) replicates the baseline findings, and column (2) shows heterogeneous effects for households with only boys. The estimates in panel (b1) show that exposure to a rainfall shock seems to increase per child resource shares in only-boy households. Compared to average per child resource shares, the effect corresponds to around 6.3% increase. While previous studies find a little evidence of pro-boy bias (Deaton, 1989; Haddad and Hoddinott, 1994; Bargain et al., 2014; Aminjonov et al., 2023), the findings here suggest that it may still emerge when households need additional workforce to recover from negative income shock.

Alternative Mechanisms. Among other possible coping mechanism, labor migration is often discussed in the related literature. Yet, in low-income settings such as Malawi, this may be less likely due to high initial costs of migration that may put households in the position of higher vulnerability to future income shocks, if, for example, households have to sell their

¹⁵Note that variables representing gender gap in employment have grid-time variation, and hence are not captured by grid fixed effects.

¹⁶Increased importance of men’s employment can also improve their relative bargaining power within household, which in turn may provide them a better access to household resources (Hoddinott and Haddad, 1995). Several studies have demonstrated this mechanism by investigating gendered impacts of shocks on assets (Doss, 2001; Duflo and Udry, 2004; Quisumbing et al., 2018).

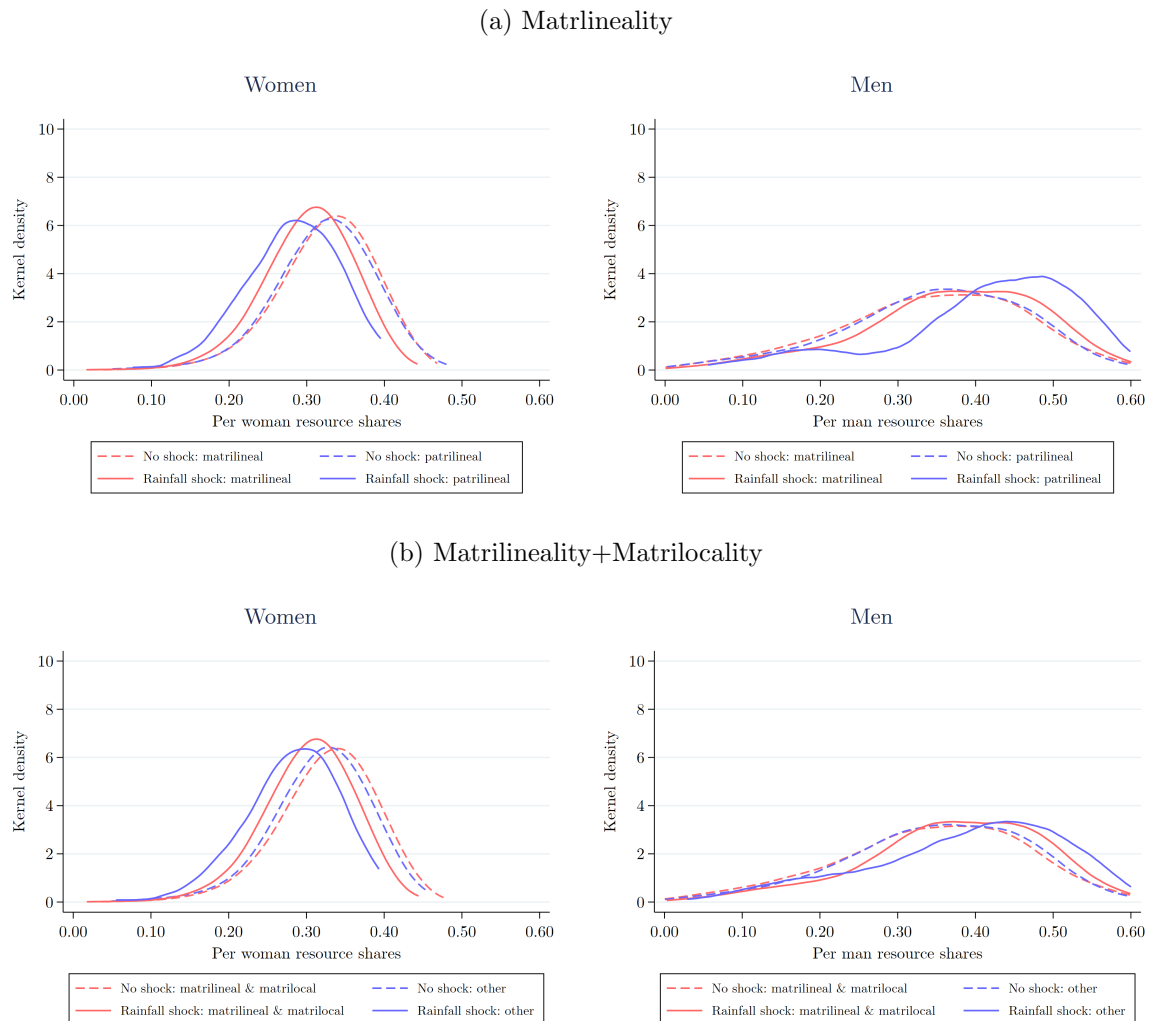
assets to cover these costs (Lewin et al., 2012; Jovanovic et al., 2019). Another strategy that households in poor countries use to reduce the burden on household budget in times of income shock is to marry off daughters (Becerra-Valbuena and Millock, 2021). As both strategies may affect intra-household consumption decisions, I suggest a simple check of whether exposure to a rainfall shock induce any substantial changes in household composition. Results in Table 4.A7 show that household composition does not significantly change in response to a rainfall shock (columns 1-4). Additionally, I use information from the survey section on migration of children, the patterns of which I find unaffected by rainfall shock (columns 5 and 6).

Role of Cultural Norms. Lastly, I discuss whether cultural practices favoring women’s roles mitigate the negative effect of natural disasters on women’s resource shares. In the absence of shocks, traditional norms, especially those that are strongly associated with gender inequality, are suggested to have impact on the share of household resources controlled by women (Giuliano, 2020; Calvi and Keskar, 2021; Aminjonov et al., 2023). At the same time, existing evidence shows that cultural norms are also likely to alter the impact of policy interventions (La Ferrara and Milazzo, 2017; Bargain, Loper and Ziparo, 2022; Ashraf et al., 2020) as well as climate shocks (Asfaw and Maggio, 2018; Corno et al., 2020; Caruso et al., 2022). In Malawi, the prevalence of contrasted traditions, such as matrilineality versus patrilineality or matrilocality versus patrilocality, provides a rare setting to investigate the role of traditions in household decision-making. For instance, Aminjonov et al. (2023) show that in Ghana and Malawi, women in patrilocal households tend to receive lower share of resources compared to those living in matrilocality households. Here I suggest a simple test of how households practicing different traditions respond to a rainfall shock in terms of resource allocation within household. I focus on matrilineality, the practice of tracing descent through mother’s family line, and its combination with matrilocality, the practice of living with or near wife’s family after marriage.¹⁷ However, as mentioned above, there is very little cultural variation within the group of households affected by a rainfall shock: around 94% of affected households is matrilineal, and 87% is matrilineal+matrilocal (cf. Table 4.A2). This is simply due to the fact that rainfall anomalies mostly hit central and southern parts of Malawi, where matrilineal or matrilocality communities are also concentrated (see Figure 4.1 and 4.A2). Thus, heterogeneity analysis that I propose here is only suggestive, as it lacks statistical power and is likely to produce less precise estimates. Table 4.A8 documents the estimation results for households with children, women and men. As before, column (1) reproduces the baseline estimates, and column (2)-(3) present heterogeneity for matrilineal and matrilineal+matrilocal households. As expected, coefficient estimates reported in panel (b1) and (b2) have large standard errors, hence, are not statistically significant. Nevertheless, these results may at least shed a ‘dim’ light on the expected cultural differences in resource sharing after an extreme climate event. In matrilineal households, both children and women seem to lose relatively smaller share of their resources when affected by a rainfall shock, compared to those living in patrilineal households. And similar results are observed when focusing on the combination of matrilineality and matrilocality. Note that smaller magnitude in this case is explained by a

¹⁷I look at the combination of matrilineal and matrilocality households to obtain more cultural variation within affected households, and also to investigate the role of these cultural practices when they are combined.

‘weaker’ degree of gender-related contrast between matrilineal+matrilocal households and the reference group which is composed of patrilineal as well as matrilineal households with other locality practices (e.g. patrilocal or neolocal).

Figure 4.5: Distribution of women’s and men’s resource shares by exposure to rainfall shock and cultural norms.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Graphs illustrate Kernel density of predicted per-woman and per-man resource shares by exposure to rainfall shock and cultural norms.

Figure 4.5 plots the full distribution of women and men’s resource shares by cultural norms. In line with the coefficient estimates, exposure to rainfall shock decreases resource shares of women in all cultural groups. But women living in patrilineal (or matrilineal and non-matrilocal households for graph (b)) seem to suffer the most. Men’s resource shares exhibit symmetric patterns, with those from patrilineal groups gaining the most in resource shares. These findings highlight, although weakly, the mitigating role of cultural norms and more importantly, the relevance of additionally employing cultural information to target the most vulnerable individuals in pro-

grams designed to enhance disaster resilience.¹⁸

4.4 Conclusion

Welfare impacts of natural disasters are often evaluated at the household level, without taking into account intra-household interactions. However, families may respond to a climate shock internally by adjusting, for instance, consumption decisions. This of course has implications on how each individual within household perceive the negative effect of a climate shock. Evidence on intra-household response to extreme weather events is very rare, and focus more on nutrition-based outcomes (e.g. [Dercon and Krishnan, 2000](#)). Using recent methodological frameworks in collective household models, this study investigates the effect of climate shocks on intra-household allocation of resources and its implications for individual welfare.

In this paper, I pool four waves of household survey data for Malawi and combine with geocoded rainfall data to explore geographic-time variation in rainfall and to assess the implications of rainfall shocks on resource sharing within household. Results show that an exposure to a rainfall anomaly during agricultural growing season is likely to decrease the share of resources accruing to women, which in turn exacerbates the negative income effect of the shock for women. Per child resource shares remain almost unchanged, implying the redistribution of household resources from women to men after the shock. I provide a suggestive evidence that this women-to-men shift of resources within household is possibly driven by relative differences in marginal productivity, which households may utilize to cope with effects of a climate hazard. In line with this hypothesis, I find that households also tend to exhibit pro-boy bias in the allocation of resources when affected by a natural disaster. Finally, exploiting cultural heterogeneity in Malawi, I provide a weak evidence for the mitigating role of cultural norms, which favor women's roles, in the intra-household impacts of climate shock on women. But as noted above, this exercise is constrained by limitations in terms of power and cultural variation within affected households.

The findings of this paper entail important policy implications, particularly in terms of targeting. Even in the absence of shocks, targeting solely based on household-level poverty assessment may overlook poor individuals living in non-poor households ([Haddad and Kanbur, 1990a](#); [Alderman et al., 1994](#); [Brown et al., 2019](#)). Similarly, policies that ignore intra-household effects of climate shocks may not effectively reach individuals who become 'newly' poor ([Skoufias, 2003](#)), for example, due to post-disaster re-allocation of resources within household, as demonstrated in the present study. Therefore, policy interventions aimed at improving disaster resilience, should be designed to target vulnerable *individuals* who are at greater risk of welfare losses during extreme climate events. Additionally, incorporating observable information such as cultural practices or

¹⁸Note that similarity of resources share distributions between two cultural groups in the absence of shock, as depicted in Figure 4.5, is due to the fact that grid fixed effects capture time-invariant geographical differences including cultural heterogeneity, and do not necessarily imply 'no impact' of these cultural norms in the absence of climate shocks.

the proportion of boys/girls in these policy designs can contribute to further refining targeting strategy to reach the most vulnerable individuals. Such targeted approach could ultimately facilitate consumption smoothing at the individual level.

Appendix

Table 4.A1: Descriptive statistics by household composition.

	HHs with children, women and men	HHs with women and men
	(1)	(2)
# of children	2.53 (1.31)	- -
# of women	1.10 (0.35)	1.12 (0.38)
# of men	1.12 (0.40)	1.21 (0.52)
Mean children's age	5.46 (3.21)	- -
Mean women's age	29.82 (8.77)	35.52 (14.97)
Mean men's age	33.50 (9.37)	35.29 (14.08)
Boys' ratio (as % of children in HH)	0.49 (0.36)	- -
% of matrilineal HHs	0.83 (0.38)	0.83 (0.37)
% of matrilineal & matrilocal HHs	0.76 (0.43)	0.76 (0.43)
Mean absolute SPEI in growing season	0.91 (0.71)	0.92 (0.70)
% of HHs affected by rainfall shock ($ \text{SPEI} \geq 1.5$)	0.23 (0.42)	0.24 (0.43)
Annual HH expenditure in 2011 PPP dollars	3007 (1804)	2621 (1695)
HH budget shares:		
Children's clothing	0.01 (0.02)	- -
Women's clothing	0.01 (0.02)	0.01 (0.02)
Men's clothing	0.01 (0.01)	0.01 (0.02)
Total clothing	0.03 (0.04)	0.03 (0.04)
% of zero HH budget shares:		
Children's clothing	0.46 (0.50)	- -
Women's clothing	0.54 (0.50)	0.51 (0.50)
Men's clothing	0.72 (0.45)	0.72 (0.45)
Total clothing	0.35 (0.48)	0.43 (0.49)
Observations	12,529	1,334

Source: author's calculations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Standard deviations in parentheses.

Table 4.A2: Descriptive statistics by rainfall shock exposure.

	No rainfall shock	Rainfall shock
	(1)	(2)
# of children	2.33 (1.47)	2.15 (1.38)
# of women	1.10 (0.36)	1.09 (0.34)
# of men	1.13 (0.40)	1.15 (0.44)
Mean children's age	4.87 (3.42)	5.14 (3.56)
Mean women's age	30.19 (9.56)	30.95 (10.10)
Mean men's age	33.72 (9.89)	33.49 (10.08)
Boys' ratio (as % of children in HH)	0.44 (0.37)	0.46 (0.38)
% of matrilineal HHs	0.80 (0.40)	0.94 (0.25)
% of matrilineal & matrilocal HHs	0.73 (0.44)	0.87 (0.34)
Mean absolute SPEI in growing season	0.60 (0.45)	1.92 (0.40)
Annual HH expenditure in 2011 PPP dollars	3044 (1844)	2728 (1615)
HH budget shares:		
Children's clothing	0.01 (0.02)	0.01 (0.02)
Women's clothing	0.01 (0.02)	0.01 (0.02)
Men's clothing	0.01 (0.02)	0.01 (0.01)
Total clothing	0.03 (0.04)	0.03 (0.03)
% of zero HH budget shares:		
Children's clothing	0.51 (0.50)	0.53 (0.50)
Women's clothing	0.53 (0.50)	0.56 (0.50)
Men's clothing	0.72 (0.45)	0.73 (0.45)
Total clothing	0.35 (0.48)	0.37 (0.48)
Observations	10,608	3,255

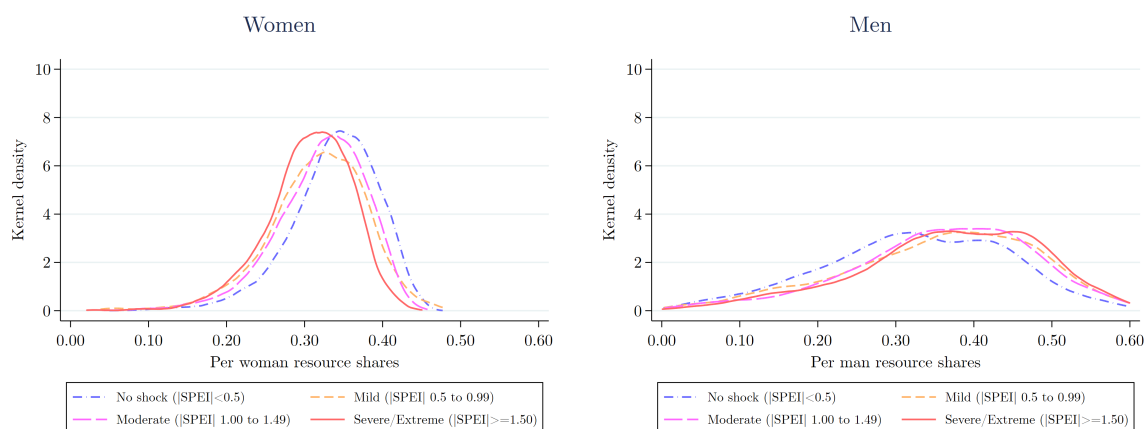
Source: author's calculations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Standard deviations in parentheses.

Table 4.A3: Counterfactual analysis with individual consumption.

	HHs with children, women and men			HHs with women and men	
	Per child	Per woman	Per man	Per woman	Per man
	(1)	(2)	(3)	(4)	(5)
Estimated daily consumption	0.96 (0.57)	2.25 (1.30)	2.79 (1.83)	2.28 (1.48)	3.59 (2.06)
Loss/gain due to rainfall shock (counterfactual analysis)					
(i) Loss/gain due to effect on HH consumption	-0.13 (0.19)	-0.32 (0.19)	-0.36 (0.24)	-0.22 (0.14)	-0.35 (0.20)
(ii) Loss/gain due to redistribution within household	0.00 (0.00)	-0.13 (0.08)	0.12 (0.09)	0.03 (0.02)	-0.03 (0.02)
(iii) Total loss/gain (i)+(ii)	-0.13 (0.07)	-0.45 (0.26)	-0.24 (0.19)	-0.19 (0.13)	-0.38 (0.22)
Counterfactual daily consumption (no rainfall shock)	1.08 (0.64)	2.71 (1.56)	3.03 (2.01)	2.48 (1.60)	3.97 (2.28)
Observations	2,923	2,923	2,923	326	326

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Table reports average per person daily resources in 2011 PPP dollars and losses/gains due to rainfall shock for the sample of households with children, women and men, and affected by rainfall shock. Standard deviations in parantheses. Loss/gains are calculated based on counterfactual resource shares/individual resources without the effect of rainfall shock on HH consumption and on individual resource shares. The effect of rainfall shock on HH consumption is estimated by regression log total HH consumption on rainfall shock indicator, household-level controls, grid, agro-ecological zone and survey round fixed effects. Counterfactual resource shares without rainfall shock are calculated by adding/subtracting the effect of shock from the baseline results.

Figure 4.A1: Distribution of women's and men's resource shares by intensity of rainfall shocks.



Source: Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Graphs illustrate Kernel density of predicted per-woman and per-man resource shares by intensity levels of rainfall shock.

Table 4.A4: Robustness checks.

	Baseline	Alternative measures of rainfall shock			Controlling for time of interview		Agro-ecological zone FE
		Alternative cutoff	Continuous SPEI	Continuous SPI (high-res.)	Month of interview	Agri. season at interview	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Resource shares for HHs with children, women and men:							
Per child	0.126 (0.031)	0.126 (0.031)	0.126 (0.030)	0.127 (0.031)	0.118 (0.025)	0.125 (0.030)	0.129 (0.032)
Per woman	0.322 (0.058)	0.315 (0.058)	0.320 (0.059)	0.316 (0.058)	0.288 (0.051)	0.321 (0.057)	0.330 (0.059)
Per man	0.356 (0.126)	0.363 (0.127)	0.357 (0.127)	0.360 (0.128)	0.403 (0.125)	0.360 (0.127)	0.341 (0.126)
(b) Effect of rainfall shock on per person resource shares:							
Children's resources	0.001 (0.003)	-0.001 (0.003)	0.001 (0.002)	0.005 ** (0.002)	0.001 (0.002)	0.001 (0.003)	0.001 (0.003)
Women's resources	-0.017 ** (0.008)	-0.014 ** (0.006)	-0.005 (0.004)	-0.009 * (0.006)	-0.015 * (0.008)	-0.014 * (0.008)	-0.017 ** (0.008)
Observations	12,529	12,529	12,529	12,529	12,529	12,529	12,529
% of HHs with shares outside [0,1]	0.005	0.005	0.005	0.005	0.005	0.005	0.005
% of HHs with non-flat Engel curve	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Panel (a) shows average predicted resource shares in households with children, women, and men. Standard deviations in parantheses. Panel (b) shows the effect of rainfall shock on per child and per woman resource shares in households with children, women, and men. Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects, survey wave fixed effects, and agro-ecological fixed effects in column 7. Standard errors clustered at the grid level in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 4.A5: Heterogeneity analysis by gender gap in employment.

	Heterogeneity by levels of gender gap in employment (Z)						
	Baseline	Z = Z _{men} - Z _{women}					
		Z1: Any employment		Z2: Off-farm employment		Z3: Hours on income-generating activities	
		As control	Heterogeneity	As control	Heterogeneity	As control	Heterogeneity
(1)	(2)	(3)	(4)	(5)	(6)	(7)	
(a) Resource shares for HHs with children, women and men:							
Per child	0.126 (0.031)	0.123 (0.030)	0.123 (0.030)	0.126 (0.031)	0.124 (0.030)	0.126 (0.031)	0.125 (0.030)
Per woman	0.322 (0.058)	0.322 (0.059)	0.325 (0.059)	0.319 (0.058)	0.320 (0.059)	0.317 (0.058)	0.316 (0.058)
Per man	0.356 (0.126)	0.360 (0.126)	0.358 (0.126)	0.358 (0.127)	0.359 (0.127)	0.361 (0.127)	0.362 (0.127)
(b) Effects on per woman resource shares:							
Rainfall shock	-0.017 ** (0.008)	-0.018 ** (0.008)	-0.001 (0.052)	-0.017 ** (0.009)	-0.004 (0.009)	-0.017 ** (0.008)	-0.003 (0.011)
Rainfall shock X High gender gap			-0.025 (0.053)		-0.029 * (0.016)		-0.035 ** (0.014)
Observations	12,529	12,529	12,529	12,529	12,529	12,529	12,529
% of HHs with shares outside [0,1]	0.005	0.005	0.005	0.005	0.005	0.005	0.005
% of HHs with non-flat Engel curve	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Panel (a) shows predicted resource shares for the sample of households with children, women and men. Standard deviations in parantheses. Panel (b) shows the effect of rainfall shock and its differential effect for grids with abover-average gender-gap in employment indicators (in columns) on per child and per woman resource shares in households with children, women, and men. Columns 2, 4 and 6 test sensitivity of baseline estimates by simply adding variables of gender gap in employment as controls. Columns 3, 5, and 7 checks heterogenous effects of rainfall shock by the level of gender gap in employment. Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects, agro-ecological fixed effects and survey wave fixed effects. Standard errors clustered at the grid level in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 4.A6: Heterogeneity analysis by child gender composition.

	Baseline	Heterogeneity: Pro-boy bias
	(1)	(2)
(a) Resource shares for HHs with children, women and men:		
Per child	0.126 (0.031)	0.127 (0.031)
Per woman	0.322 (0.058)	0.320 (0.058)
Per man	0.356 (0.126)	0.356 (0.126)
(b1) Effects on per child resource shares:		
Rainfall shock	0.001 (0.003)	0.000 (0.003)
Rainfall shock X Only-boy HHs		0.008 ** (0.003)
(b2) Effects on per woman resource shares:		
Rainfall shock	-0.017 ** (0.008)	-0.018 * (0.010)
Rainfall shock X Only-boy HHs		0.004 (0.013)
Observations	12,529	12,529
% of HHs with shares outside [0,1]	0.005	0.005
% of HHs with non-flat Engel curve	1.000	1.000

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Panel (a) shows predicted resource shares for households with children, women and men. Standard deviations in parentheses. Panel (b) shows the effect of rainfall shock and its differential effect for only-boy households on per child and per woman resource shares in households with children, women, and men. Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects and survey wave fixed effects. Standard errors clustered at the grid level in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 4.A7: Effect of rainfall shock on household composition and child migration

	HH size	# of children	# of women	# of men	Any migrated child (=1)	# of migrated children
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall shock	-0.014 (0.040)	-0.023 (0.034)	-0.007 (0.007)	0.016 (0.013)	-0.001 (0.006)	-0.003 (0.007)
R-squared	0.202	0.172	0.042	0.178	0.066	0.060
Observations	12,529	12,529	12,529	12,529	12,529	12,529
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Household-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of outcome variable	4.754	2.533	1.096	1.125	0.0318	0.0374

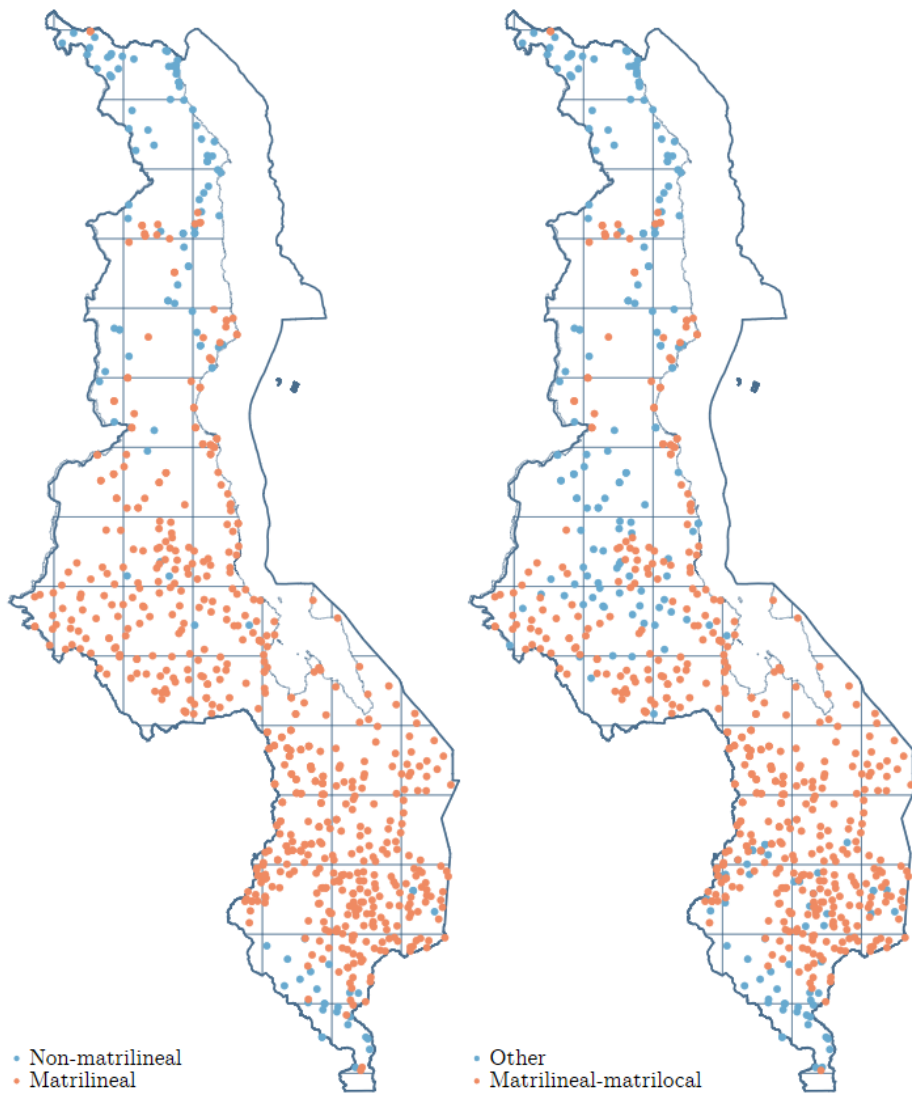
Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Table reports OLS estimation results for the effect of rainfall shock on migration indicators specified in columns for households with children, women and men. Standard errors clustered at grid level in parentheses. *, **, *** indicate 10%, 5% and 1% significance level.

Table 4.A8: Heterogeneity analysis by cultural norms.

	Baseline	Heterogeneity by cultural norms	
		Matrilineal	Matrilineal+ Matrilocal
	(1)	(2)	(3)
(a) Resource shares for HHs with children, women and men:			
Per child	0.126 (0.031)	0.127 (0.031)	0.126 (0.031)
Per woman	0.322 (0.058)	0.322 (0.059)	0.319 (0.058)
Per man	0.356 (0.126)	0.354 (0.126)	0.358 (0.126)
(b1) Effects on per child resource shares:			
Rainfall shock	0.001 (0.003)	-0.014 (0.018)	-0.006 (0.007)
Rainfall shock X Matrilineal		0.015 (0.018)	
Rainfall shock X Matrilineal+Matrilocal			0.008 (0.008)
(b2) Effects on per woman resource shares:			
Rainfall shock	-0.017 ** (0.008)	-0.044 (0.047)	-0.034 (0.022)
Rainfall shock X Matrilineal		0.029 (0.048)	
Rainfall shock X Matrilineal+Matrilocal			0.019 (0.022)
Observations	12,529	12,529	12,529
% of HHs with shares outside [0,1]	0.005	0.005	0.005
% of HHs with non-flat Engel curve	1.000	1.000	1.000

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Panel (a) shows average predicted resource shares for households with children, women and men. Standard deviations in parantheses. Panel (b) shows the effect of rainfall shock and its differential effect across cultural norms related to lineage and post-marital residence. Other variables included in the resource share equations are household-level controls (average age of men, women, and children, the number of men, women, and children, urban dummy, proportion of boys), grid fixed effects, and survey wave fixed effects. Standard errors clustered at the grid level in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

Figure 4.A2: Geographic location of matrilineal and matrilineal+matrilocal communities.



Source: Malawi IHS 2010/2011, 2013, 2016/17 and 2019/20.

Table 4.A9: Effect of rainfall shock on the frequency of clothing expenditure.

	Zero total clothing (=1)	Zero children's clothing (=1)	Zero women's clothing (=1)	Zero men's clothing (=1)
	(1)	(2)	(3)	(4)
Rainfall shock	0.033 (0.021)	0.019 (0.022)	-0.001** (0.001)	-0.000 (0.000)
R-squared	0.043	0.038	0.037	0.032
Observations	12,529	12,529	12,529	12,529
Grid FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Household-level controls	Yes	Yes	Yes	Yes
Mean of outcome variable	0.350	0.464	0.543	0.723

Source: author's estimations using Malawi IHS 2010/11, 2013, 2016/17 and 2019/20 and rainfall data from SPEI Global Drought Monitor. Notes: Table reports OLS estimation results for the effect of rainfall shock on the probability of zero clothing expenditure for households with children, women and men. Standard errors clustered at grid level in parantheses. *, **, *** indicate 10%, 5% and 1% significance level.

General Conclusion

This thesis aims to address two broad topics in the literature of health and development economics. The first two chapters shed light on the patterns of social distancing behavior during the COVID-19 pandemic. This work generally extends our knowledge on mechanisms underlying compliance attitudes towards public health rules in times of a massive health crisis. By considering the settings of advanced and less developed economies separately, but still with a broad geographical coverage, it offers a unique perspective on what factors are relevant for improving compliance in these two contexts. In particular, the second chapter originally unfolds positive externalities of income support policies on compliance behavior among the poor. The next two chapters examine the patterns of intra-household inequalities in consumption and how they might be affected by adverse economic shocks, highlighting its implications for individual welfare. Namely, this research expands the related literature by providing a global ‘picture’ of gender gap in consumption and child poverty. The contribution is also methodological as it demonstrates a large-scale and systematic application of an underutilized methodology that could be used to refine poverty indicators and, accordingly, targeting in redistributive policies aiming to reach the poorest or the most vulnerable individuals.

In Chapter 1, we study whether the degree of trust in government shapes public compliance with strict social-distancing measures in the context of the COVID-19 pandemic. Using subnational mobility data for European countries, we find that the decrease in human mobility around mid-March 2020 is significantly stronger in high-trust regions, which we interpret as a higher level of compliance with national lockdowns in districts that demonstrated higher levels of political trust before the pandemic. Additionally, we show, using continuous measure of policy strictness, that mobility-reducing impact of lockdown stringency is larger in regions with a stronger trust in policymakers. Finally, we quantify the extent to which the effect of trust on human mobility mitigates the growth rate of mortality due to COVID-19.

Chapter 2 addresses similar question but looking at the context of low- and middle-income countries. First, we show that mobility reduction in response to stay-at-home orders is relatively smaller in regions with higher incidence of poverty. This pattern is observed only for work-related mobility, implying that non-compliance in poorer regions is likely driven by higher propensity to continue income-generating activities among the poor. On the other hand, income support programs, either as new or extensions to the existing policies, tend to mitigate the

poverty gap in mobility by raising the ability of the poor to comply with containment rules.

In Chapter 3, we employ recent extensions of collective household models to retrieve the share of resources allocated to children, women and men within household systematically for 33 low- and middle-income countries. Using estimated resource shares, which allow calculating consumption and poverty at the individual level, we provide the first, in its scale, global mapping of gender gap in consumption and child poverty. We find a common pattern of intra-household gender inequality in resource sharing. Remarkably, we observe a Kuznets pattern of intra-household inequality where children's resource shares increase and then decrease with living standards, with men's resources exhibiting the opposite pattern. A cross-validation against child anthropometric indicators reveals that child consumption/poverty, calculated using estimated resource shares, offers a better approximation of child welfare, as opposed to using household-level indicators of welfare when direct measures of child welfare can not be observed. This in particular contributes to the broad efforts aimed at validating resource sharing estimation approaches (e.g. [Bargain, Lacroix and Tiberti, 2022](#))

Chapter 4 suggests a further application of this approach in the context of climate shocks. It originally incorporates a natural experiment, stemming from a random occurrence of rainfall anomalies, in the resource sharing model for Malawian households. I find that exposure to a rainfall shock, which usually comes with a negative income effect for most households in Malawi, leads to a lower share of household resources accruing to women and no change in children's shares. This re-allocation of resources within household puts the heaviest burden of the shock on women. At the same time, heterogeneity analyses show that the negative effect of climate shock on women's resources is concentrated in areas where employment gap between men and women is larger. This suggests that observed women-to-men redistribution of resources is possibly due to households' prioritizing the needs of those members who have greater potential to contribute to household income during difficult times, often referred as 'life-boat' ethics.

At last, this thesis has opened further research avenues that I wish to pursue in the future. Inspired by findings in Chapter 2, more efforts should be made on the assessment of health externalities of policy interventions. In particular, one of the understudied areas in the related literature is the impact of climate-'unfriendly' urbanization strategies on population health. Additionally, as discussed in Chapter 4, cultural norms may alter impacts of climate shocks. This area of research should further be extended, in terms of geographical coverage and also using other outcomes of well-being, as it can significantly help to tailor more targeted and effective policies aimed at improving disaster-resilience of those who are vulnerable to natural hazards.

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