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Comment

On the ‘Arab Inequality Puzzle’: A Rejoinder

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ABSTRACT

This rejoinder responds to the Comment by Vladimir Hlasny and Paolo Verme on my article that was recently published in this journal. The acrimonious tone of their Comment is regrettable, and its content reveals a worldview according to which issues such as income inequality and its causal relation to social contention are ‘technical’ in nature and reserved for debate only among ‘apolitical’ econometricians. Beyond that, Hlasny and Verme's Comment adds precious little by way of new argument to the discussion.

INTRODUCTION

I am glad that Vladimir Hlasny and Paolo Verme reacted to my article,[1] even if their Comment is quite acrimonious and verging on ad hominem characterizations. The abstract alone contains a record number of accusations: ‘erroneous’, ‘misleading’, ‘false’, ‘poor understanding’, ‘unfair’, ‘speculations’, ‘conspiracy theories’. I am nevertheless glad, as there can be no better indication that my article touched a nerve with the two researchers who, more than anyone else, attempted to uphold — both ‘technically’ and socio-politically — the highly implausible thesis of a rate of income inequality in Egypt that counts among the world's lowest, and one that is declining at that (see Hlasny and Verme, 2013; Verme et al., 2014).

The gist of Hlasny and Verme's Comment is that the debate on income inequality in the Arab region, particularly in Egypt, and its relation to the regional uprising that was dubbed the ‘Arab Spring’, should not be ‘politicized’ but kept ‘technical’. It speaks volumes about the two authors'

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1. Please see: ‘On the “Arab Inequality Puzzle”: The Case of Egypt’, *Development and Change* 51(3): 746–70 (May 2020).

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worldview that they are unaware just how outmoded and self-defeating this argument is. It is astonishing indeed that anyone could uphold the claim that the discussion about a topic such as the validity of official data under authoritarian regimes and the causality of major popular uprisings against these same regimes is purely 'technical', and one which econometricians alone should engage in, to the exclusion of political economists and all other social scientists — not to mention social and political activists who often know more about their countries than foreign 'experts'.

It is no less astounding that my two critics seem to believe that their own interpretation of reality in Egypt is apolitical whilst any reader of their writings on that country, including my article's citations from their work, can easily detect that their approach is very much politicized. How on earth could anyone believe that condoning a (dictatorial) regime's highly questionable pretence to low income inequality and the World Bank's highly disputable (neoliberal) approach to development is purely a 'technical' matter? Leaving this claim aside, I would like to address the few specific comments by Hlasny and Verme on my article.

WHO IS BEING DISCUSSED?

Debate is always welcome when it helps to clarify issues and contributes to advancing research. It is much less useful when it consists in correcting misrepresentations. Unfortunately, there is much of the latter in Hlasny and Verme's Comment. They begin by trying to gain the support of researchers whom I have not criticized. To this end, they distort my conclusion (pp. 767–68) by attributing to me the assertion that 'the protagonists' of the 'Arab Inequality Puzzle' debate exhibit a 'wilful blindness' and a 'systematic neoliberal bias', whereas I make this comment with reference to the international financial institutions (IFIs), certainly not about all 'the protagonists' of the debate, many of whom I quote approvingly, including researchers who work for the same IFIs.

In fact, my two critics themselves later cite my observation regarding 'the discrepancy that may exist between working papers by individual researchers at intergovernmental institutions and these institutions' official output' (p. 764). To this they respond by explaining that some of the studies that I cite approvingly are by 'World Bank staff who have expressed diverging views publicly and consistently in all their publications', and that 'Hlasny is not a World Bank employee (and in fact has been affiliated with ESCWA since 2015) and four of the authors in Verme et al. (2014) are Egyptians and non-World Bank staff'.[2] This response is puzzling indeed: my two critics seem to want the readers to believe that intergovernmental

2. Do I need to explain that ESCWA is an *intergovernmental* institution?

institutions are like publishing houses that bear no responsibility for the views that they publish under their imprint. They reproach me for what they describe as my 'custom delineation of what constitutes "official" research of IFIs, and research conducted independently'.

Anyone familiar with World Bank reports, as well as those of other intergovernmental institutions, knows that they often involve researchers who are not staff members (including academics, of course). However, these reports are not regarded as the researchers' personal output, but as institutional publications, as is usually confirmed by at least one preface by a senior manager. Accordingly, the report authored by Verme et al. (2014), which is an official World Bank study and certainly not a collective working paper, has three forewords: the first by a World Bank Chief Economist for Africa, a second by the then Finance Minister of Egypt, and a third by the then World Bank Country Director for Egypt. The same goes for the report that was lead-authored by Elena Ianchovichina (2018), which is the other major World Bank report addressing the 'Arab Inequality Puzzle' that I discussed in my article. We are told from the outset (ibid.: xiii) that it was 'conducted under the general guidance' of the then Chief Economist of the World Bank's Middle East and North Africa Region. This is in sharp contrast with personal inputs from researchers working with or for the World Bank. In such contributions, it is standard to find a disclaimer, like the one printed on the cover of World Bank Policy Research Working Papers (e.g. Hlasny and Verme, 2013):

> The papers carry the names of the authors and should be cited accordingly. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

The absence of such a disclaimer on official World Bank studies such as the two reports that I discuss clearly indicates a qualitative difference in status. I believe that I have more than adequately demonstrated the discrepancy that exists between the individual or collective unofficial output of researchers, on the one hand, and World Bank official publications, on the other hand — even when the latter involve researchers who have themselves published studies that are in full contradiction with the official publication's line. This is because the World Bank, like most intergovernmental institutions, and especially the IFIs, is bound by the governmental consensus (be it the Washington Consensus or a refurbished version of it) that they represent. That the IFIs have a 'neoliberal bias' is therefore obvious to everybody but those who are themselves affected by the same bias and thus believe that it is 'scientific' or 'technical'.

As my discussion of their views illustrates, Hlasny and Verme are clearly fervent upholders of the neoliberal paradigm. The argument they advance to dismiss my assertion that the World Bank's adherence to the 'Arab Inequality

Puzzle' thesis is a manifestation of the same paradigm sounds disarmingly naïve. They write:

> It is also unclear what the rationale of the IFIs would be for supporting the view that income inequality in Egypt prior to the Arab Spring was low. This does not help to explain the Arab Spring and, in the case of Egypt, low inequality was not an indicator of a healthy economy (as Achcar suggests). On the contrary, low inequality in Egypt was a symptom of widespread poverty, stagnant salaries and low social mobility. Therefore, it is hard to see how IFIs would gain from maintaining such a stance on income inequality in Egypt. Achcar stops short of offering insight into this point.

The authors falsely claim that I am suggesting that low inequality in itself is 'an indicator of a healthy economy', only to reiterate their belief that low inequality in Egypt is but a symptom of 'widespread poverty, stagnant salaries and low social mobility' — as if inequality were a reflection of the state of the bottom incomes of a society, rather than the gap between the bottom and top incomes. This, of course, is in tune with their claim that Egypt needs more inequality, and not less: a typical neoliberal claim that I discussed in my article (p. 758).[3]

WHAT IS BEING DISCUSSED?

My starting point was to criticize the IFIs' acceptance of the official inequality data of Arab countries as valid, and their adherence to the so-called 'Arab Inequality Puzzle' thesis. According to this thesis, the Arab Spring occurred not *because* of high and rising inequality in the Arab region, as reflected in the prominence given to the issues of inequality and social justice in the social protests, but *despite* low and even declining inequality.

World Bank Data (2020) present the Gini coefficients of Algeria (27.6), Iraq (29.5), Egypt (31.5), Lebanon (31.8), the United Arab Emirates (32.5), Mauritania (32.6), Tunisia (32.8) and Jordan (33.7) and label them all as lower than that of Canada (33.8). This means that, socio-economically, these Arab countries are less unequal than the North American country whose social policies are often cited as among the most advanced in the world. Although these highly implausible figures have been questioned by countless researchers in various publications, including working papers published by the World Bank itself, the latter's official reports on the Arab region uphold them as realistic, and build upon them its interpretation of regional political and social dynamics, as well as its policy recommendations.

In my earlier article, I point out 'a major flaw in international comparisons of inequality published by the IFIs: they end up comparing apples and oranges by including in one and the same list figures based on different calculation modes, ranging from household consumption surveys to income taxation data' (p. 747). This much should be indisputable since some

3. I refer again to ECLAC's excellent report on the inefficiency of inequality (ECLAC, 2018).

countries' Gini coefficients on the same IFIs' lists are based on consumption figures, others on income figures obtained exclusively through household surveys, and others (most industrialized countries) on income surveys that make use of 'administrative data', such as tax records.[4]

Surprisingly, Hlasny and Verme reply to my rather factual statement by asserting peremptorily that it is 'false'! Why false? Well, because my two critics 'compare income and consumption Ginis in Egypt to those in surveys worldwide, carefully noting country groupings and welfare aggregates used'. How my general statement about international comparisons published by the IFIs could be disproved by whatever Hlasny and Verme do is beyond my understanding.

It is hardly disputable that when it comes to income inequality there is no better way to assess the plausibility of household surveys than to compare them to taxation data wherever it is possible. However, since it can safely be assumed that no one would normally reply to an official household survey by declaring an income that is *higher* than what they declare to official revenue services, it can also safely be inferred that in countries like Egypt — where the revenue service is a sham and corruption is just one of many ways of avoiding tax — verification of household survey results by means of official taxation data would not help much, even if such data were available.[5] Add to this the well-known facts that many higher-income households manage to evade the surveys altogether and that survey teams tend to miss or avoid many of the poorest areas deemed too unsafe in a country where an estimated 70 per cent of households live without secure tenure (BEDI, 2016).[6]

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4. For example, the French 'Survey of Fiscal and Social Revenues', according to its methodological annex (INSEE, 2016: 2), 'is not properly speaking a survey since part of the collected data, those regarding revenues and social benefits, are not collected by conducting a survey, but by using administrative data for a representative sample of the population'. In the UK, respondents to 'Understanding Society: The UK Household Longitudinal Study' are 'asked to consent to link their survey responses to administrative records on the receipt of state benefits held by the Department for Work and Pensions and tax records held by HM Revenue and Customs (HMRC). Income is known to be under-reported in surveys … and so administrative data linkage offers an exciting way forward to potentially reduce measurement error that may occur in survey reports of income' (Fischer et al., 2019: 23–24). In the US, '[a]ccess to federal individual income tax return data is vital to the U.S. Census Bureau's income and poverty estimates', according to the bureau on its website (US Census Bureau, 2017).
5. In countries like Egypt, only wage-earners in the (minority) formal sector, for example, in the governmental sector, end up duly paying their income tax, which is generally withheld at source. Taxation is heavily defective for most other incomes. And yet, even with Lebanon's flawed taxation data, Lydia Assouad (2017: 7) was able to show that 'inequality statistics based on surveys are seriously downward biased'.
6. In a remarkable study on the underestimation and misrepresentation of poverty in greater Cairo, Sarah Sabry probes what she calls 'under-sampling residents of informal settlements' in CAPMAS surveys (Sabry, 2009: 15–20).

The problem has been summarized by Christophe Lakner et al. (2016) in a World Bank blog posting which discusses 'the underlying welfare measure (income or consumption) used to quantify the extent of inequality within a country, and the fact that estimates of inequality based on data from household surveys are likely to under-report incomes of the richest households'. Lakner et al. (ibid.) correctly conclude:

> In developing countries, the availability of tax record data remains limited, and, where they are available, data quality may be problematic given the absence of broad income taxes in many developing countries and the incomplete taxation of capital incomes. Some studies have explored the use of alternative data on the top tail, such as real estate prices, as well as combining top data with household surveys to get a better picture of the overall distribution. While better measurement and data certainly take time and money, they remain crucial for making public policies more efficient and equitable.

THE CASE OF EGYPT

Focusing on Egypt, my earlier article cites several studies exploring such 'alternative data' based on national accounts, executive income, or house prices, all of which reach emphatically the same conclusion, that Egyptian income inequality is grossly underestimated. Most of the studies cited in my article were conducted after the debate that was sparked by Hlasny and Verme's (2013) paper in which they assert that in Egypt official figures are plausible, income inequality is truly remarkably low and that it has even been declining over the years preceding the 2011 uprising. The two authors tried to prove the plausibility of the official figures by way of a statistical-econometric exercise which is summarized in my article (p. 755). Not being an expert in statistics or econometrics myself, I cite the 'technical' rebuttal of their demonstration by two well-known experts, Facundo Alvaredo and Thomas Piketty (2014), who strongly disagree with the claim that income inequality is remarkably moderate in Arab countries.

And yet, instead of engaging with the 'technical' rebuttal of Alvaredo and Piketty, Hlasny and Verme attempt in their Comment to dismiss the paper article by their two critics in one stroke by pointing to an *additional* argument that Alvaredo and Piketty make about cross-country regional inequality. Hlasny and Verme pretend that it is 'a message that drowned out [Alvaredo and Piketty's] confirmation that national income inequalities appear moderate'. Readers will appreciate this style of arguing and misrepresentation.

Another example of the difficulty Hlasny and Verme appear to have with reading and citing properly occurs when they claim that the study by Sami Bibi and Mustafa Nabli (2010), which is summarized in my article, 'concluded that Arab states fall within the range of countries with moderate inequality', and then accuse me of failing 'to mention [Bibi and Nabli's] central conclusion that income inequality was modest'. That's very surprising indeed, since Bibi and Nabli (ibid.: 31) assert that 'inequality in earnings appears to be relatively higher in Arab countries compared to other regions, which contrasts with the findings for inequality based on household expenditures'. To this, they add that 'the little evidence available on inequality in the distribution of other variables beyond income (education, health and land) suggests that the Arab countries are among the most unequal' (ibid.).

There is no need for me to carry on and repeat here what is already in my article. Whoever is interested can easily compare Hlasny and Verme's Comment to what I write, and judge for themselves. I have only reinstated some of the statements that my two critics blatantly misrepresent in their Comment. Beyond this, they hardly bring any fresh argument to the discussion.

CONCLUSION

In their conclusion, Hlasny and Verme write the following:

> Achcar's view is not unique among regional commentators, and speaks to the degree of non-transparency, traditional data paucity, and contamination of available evidence with hearsay in the region. As scholars working on income inequality, we find it essential to keep the debate on a technical level and leave speculations and conspiracy theories aside.

In sum, for my two critics, I am one of those 'regional commentators' who are influenced by 'hearsay' and adhere to 'speculations and conspiracy theories'. This is a common pretence of so-called experts working for international institutions who claim to know more about a region than those who are natives of it (while some of these so-called experts don't even know the region's language), and believe that they hold a monopoly on scholarship over all those who disagree with them, who can therefore only be uninformed speculators. Pity such expertise.

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