

Growth and Governance: A Rejoinder

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In this brief rejoinder we would like to point several key areas where we disagree with Kurtz and Shrank's response entitled "Growth and Governance: A Defense".

Definitions

KS begin their defense by accusing us of "abandoning" our measure of Government Effectiveness in favour of other indicators such as Rule of Law. All we noted in our original response, and reiterate here, is that we find KS's exclusive emphasis on this one particular dimension of governance to be idiosyncratic, and not shared by the large economics literature on institutions and growth. We also note that we discussed all six of our governance indicators in our response simply because the critiques that KS raise of government effectiveness, notably potential respondent biases and halo effects, could equally well be applied to our other indicators. We think it important to demonstrate the scarcity of empirical support for these critiques for all six of our governance indicators.

Ideological Biases

KS remain unpersuaded by our arguments that ideological and business-oriented biases of respondents do not play a significant role in our indicators. Regrettably, however, KS continue to fail to provide any concrete evidence of such biases. KS quote the opinions of some scholars that some of our data sources are "ideologically loaded". As we noted in our response, in the case of ideological biases, we have empirically investigated whether supposedly "right-wing" think tanks give better scores of governments who share their political orientation, and found virtually no evidence of this. Any empirical evidence to the contrary that KS could bring would be a welcome addition to the debate, but simply citing the assertions of others strikes us as unhelpful.

The evidence KS report from the Afrobarometer surveys on businesspeople's preferences is irrelevant to their critique of our indicators. We acknowledge that KS have a useful suggestion that one can use the identity of respondents to this survey (who are asked to self-identify as businesspeople or other types of respondents) to investigate whether businesspeople's views differ markedly from others' on what constitutes good governance. All KS have shown is that on average businesspeople respond more

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favourably to questions about government services. While this is interesting, it is irrelevant to the issue at hand. What matters for our indicators is how countries are ranked relative to each other. Therefore, KS would need to show that a ranking of countries based on businesspeoples' responses differs significantly from a ranking of countries based on other responses. It is entirely possible that businesspeople on average in all surveys provide more positive responses than other respondents (perhaps they are on average just more optimistic?). But this need not affect the relative ranking of countries, which is what matters for our indicators.

KS also emphasize the relatively low correlation of the Latinobarometro survey question we use in the Government Effectiveness Indicator with corresponding variables from commercial risk rating agencies. It is useful however to recognize that the particular Latinobarometro question we use for Government Effectiveness is an unfortunately vague question about respondents' overall trust in government. In contrast, the Afrobarometer survey asks several questions much more specifically focused on access to public services that are much closer to those asked of firms in the Global Competitiveness Report survey, and these two sources accordingly are much more highly correlated. Indeed, if we compare a more specific question about trust in police from Latinobarometro, that we use in our Rule of Law indicator, with a similar question about the police from the Global Competitiveness Survey, we find a quite high correlation of 0.77. We are therefore neither surprised nor particularly concerned at the lower correlation of the one particular -- and unfortunately vague -- Latinobarometro question that KS emphasize in their Defense.

We do not dispute that data sources capturing the views of businesspeople and commercial risk rating agencies play a prominent role in our governance indicators (although certainly not an exclusive role, given our reliance on citizen surveys, NGOs, and multilateral organizations). We note also that this feature of our indicators is dictated by data availability -- there simply are not many regularly-updated cross-country household surveys that we can incorporate in our indicators. However, the key issue is whether country rankings based on such alternative and yet-to-be-created data sources would be substantially different from those we present. On this key point KS provide no new evidence.

Halo Effects

We can only read KS's Defense on this issue as a conceding our point that their original work failed to provide robust empirical evidence of halo effects. KS nevertheless wonder why we "depart from past practice" by using secondary school enrollment rates to expand the sample of countries (selectively citing one or two of our earlier papers on completely different topics), and suggest that the lack of robustness of their results in fact constitutes a critique of our indicators. In response, we briefly note that:

- Although of course conceptually distinct, secondary schooling rates are extremely highly correlated with stocks of human capital, with a correlation in our sample of

- 0.85. While we are unaware of any well-specified theoretical reason to prefer the stock variable over the flow variable as a determinant of government effectiveness, it is perfectly reasonable to use the flow data as a proxy for the stock in the interests of expanding country coverage. And as we showed, in this larger sample there is no systematic evidence of halo effects.
- As KS themselves note, enrollment rates could well be interpreted as an indicator of government effectiveness itself. This is a useful observation that in fact provides a further justification for using this variable, since it serves as an "objective" control for unobserved true government effectiveness. But under this interpretation, KS wonder why enrollment rates are not more strongly correlated with government effectiveness. The answer is straightforward, as the regressions KS propose also include per capita income together with schooling variables as explanators of government effectiveness. Unsurprisingly these two right-hand-side variables are very highly correlated at around 0.8, and this multicollinearity problem makes it difficult to identify a significant *partial* correlation between schooling and government effectiveness. In fact, the simple correlation between the two is a very respectable 0.72.

Growth and Governance

KS assert that our measure of Government Effectiveness should by construction be positively correlated with short-run subsequent growth, claiming that our measure contains several questions about anticipated short-run growth. This claim is based on an overly-literal reading of the criteria used by just one of our 17 data sources for Government Effectiveness. DRI, a commercial risk rating agency, uses a somewhat peculiar phrasing when it provides numbers which show their assessment of the likelihood of certain "risk events" like "a decrease in government personnel quality that lowers growth in a 12-month period." We have discussed this question at length with staff of DRI and have come to the understanding that this is simply peculiar phrasing serving their own marketing purposes, and that they are actually providing an assessment of levels of civil service quality. This, combined with the sensible observation that we do not think commercial risk rating agencies have perfect foresight about growth even one year hence, means that we are completely unconcerned that this particular measure, or the aggregate indicator, are uncorrelated with subsequent very short-run growth in the handful of empirical specifications that KS offered in their article.

We also think it important to correct the mischaracterization of the empirical literature on institutions and growth that KS offer in their Defense. In particular:

- KS claim that the idea that differences in per capita income today primarily reflect differences in long-run growth performance requires "heroic assumptions about the distribution of income across countries in 1800". This stylized fact is not an assumption, but rather is based on the careful studies by economic historian Angus Maddison, who documents as carefully as possible that the ratio of per capita incomes between the richest and poorest countries in the world 200 years ago was on the order of three or four to one, while today it is an order of

magnitude greater. Based on this factual observation it logically follows that the much larger cross-country income differences we observe today primarily reflect very long-run differences in growth performance.

- KS claim that using perceptions of expropriation risk is using a measure of policy (the decision to expropriate) instead of an institution. This is a rather superficial interpretation, and would only be true if studies used actual expropriations, rather than assessments of the likelihood of expropriations averaged over a decade. Investor perceptions of expropriation risk are widely interpreted as capturing something about the respect of the state for private property, which is a fundamental notion of institutional quality.
- KS dispute the use of settler mortality rates as an instrument for institutional quality because settlers were "the most violent expropriators in world history". It appears to us that KS are choosing to miss the point of one of the most influential empirical papers on institutions and growth. That Europeans propagated all sorts of atrocities in the countries where they settled is neither in dispute, nor is it in any way relevant to the empirical strategy at hand. What matters is that the disease environment faced by settlers provides an exogenous source of variation in the incentives that settlers faced to set up the institutions of property rights protection, which persist to this day. This exogenous variation can be used to statistically identify the causal effect of institutions on very long run growth.

As we noted in our original response, we do not wish to argue that the empirical literature on institutions and growth is conclusive -- and so we cannot agree with KS's characterization of us as taking an "intolerably conservative approach to social scientific argumentation". We think it is entirely appropriate for KS, or any other scholars, to critique, and so develop, the literature on institutions and growth. But we do not think that offering a few regressions, so far removed from the best-practice frontier in this literature, constitutes a serious critique that helps to advance our understanding of these important questions.