# Comments

**Eduardo Lora:** Kaufmann and Kraay have made extremely valuable contributions to the analysis of the interplay between governance and development. The set of governance indicators they developed has become the most recognized measure of institutional quality, giving rise to numerous studies that have helped to reassert the importance of institutions.[1] "Growth without Governance" is a further step in the process of disentangling the links between development and institutional quality. Until now, economists have had a tendency to assume, either implicitly or explicitly, that those two variables tend to reinforce each other. Kaufmann and Kraay show that this apparently obvious assumption is far from warranted.

The empirical strategy followed by the authors rests on the observation that the estimated coefficient of an institutional variable (more specifically, a measure of the rule of law) in a regression explaining income levels in a cross section of countries is smaller when the method of estimation is ordinary least squares (OLS) than when instrumental variables (IV) are used. Since the endogeneity bias of the coefficient is negative (under certain restrictions that the authors carefully establish), the influence of income on institutions must be inverse. The implications of this result are profound, as it implies that lack of attention to institutional improvement will eventually deter development. Because so little is known on how to improve institutions, any development strategy is bound to face a blind alley, at least until the institutional frontier shifts in the right direction, most likely as a result of some uncontrollable factor.[2]

Enlightening as this result may be, it cannot be accepted at face value without further empirical analysis. A first question is whether it holds at all levels of development. A cursory look at a plot of income levels versus a measure of institutional quality, such as the rule of law, suggests that the

I wish to thank Natalia Pérez for her very valuable assistance with the empirical exercises reported in this comment, and Daniel Kaufmann and Aart Kraay for kindly providing their instrumental variable data.

1. Kaufmann, Kraay, and Zoido-Lobatón (1999a, 2002).
2. See World Bank (2002).

(positive) relationship between these two variables is stronger for upper-income economies than for poorer countries. Some OLS estimations confirm this observation: the coefficient for the richer half of the sample is three times larger than that for the rest of the sample (see table 6).[3] This raises the question of whether institutions are less important for income in poorer countries or whether income is more deleterious to institutions at low levels of development. In principle, one could try to answer this question by running IV regressions and comparing the two coefficients again. While the IV coefficient for the upper half is virtually identical to the OLS result, the IV coefficient for the lower half becomes substantially higher, though insignificant. This suggests that the reverse causality from institutions to income is negligible for the richer countries, a finding that does not support the main hypothesis of the paper for this group of countries. The results for the poorer half are more difficult to interpret, because they may be affected by the measurement errors of the institutional variable, which tend to be larger for the poorer countries, and by the limitations of the instrument used, namely, the settler mortality rate. The authors report that "given the weaker performance of the instrument in the first-stage regression for the poorest half of the sample, we can also not discount the possibility that the differences in slopes in the two samples is simply driven by the problem of weak instruments." The instrument chosen by the authors is defendable on several grounds.[4] It has serious shortcomings, however, such as the fact that it is only available for sixty-eight countries (some of which are imputed values). The authors attempt to solve this shortcoming by expanding the number of observations to 153 countries by further imputing the missing values of settler mortality based on the data for tropical location, colonial origins, and fractions of the population speaking English or another major European language. They thus introduce an additional stage in the process of estimation whereby the true relationship between income and institutions may be obscured. They claim to obtain similar results for the complete sample when they restrict the analysis to the countries for which the original settler mortality data are available, or when they use a set of dummies of linguistic origins as their instrument.

3. In their paper, Kaufmann and Kraay run regressions based on income levels for 1996 and the quality of institutions for 2000–01. In my own regressions I prefer to take both variables for 1997 to maintain a strict cross section that is not contaminated by possible dynamics between the two variables (see below).

4. See Acemoglu, Johnson, and Robinson (2001).

**TABLE 6. Regression Results**[a]

| | *World* | | | *Rich countries* | | | *Poor countries* | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Log per capita GDP* | | *Rule of law* | *Log per capita GDP* | | *Rule of law* | *Log per capita GDP* | | *Rule of law* |
| *Independent variable* | *OLS* | *IV*[b] | *First stage* | *OLS* | *IV*[b] | *First stage* | *OLS* | *IV*[b] | *First stage* |
| Rule of law, 1997 | 1.484 | 1.987 | | 1.085 | 1.168 | | 0.359 | 3.109 | |
| | (17.77)*** | (12.93)*** | | (12.36)*** | (7.73)*** | | (2.31)** | (1.61) | |
| Settler mortality rate | | | −0.522 | | | −0.656 | | | −0.098 |
| | | | (−8.90)*** | | | (−5.98)*** | | | (−1.61) |
| Constant | 7.535 | 7.521 | 2.283 | 8.236 | 8.184 | 3.058 | 6.439 | 8.015 | −0.091 |
| | (100.61)*** | (89.79)*** | (8.73)*** | (92.85)*** | (69.40)*** | (7.18)*** | (53.89)*** | (7.14)*** | (−0.30) |
| *Summary statistic* | | | | | | | | | |
| No. observations | 148 | 148 | 155 | 74 | 74 | 81 | 74 | 74 | 74 |
| $R^2$ | 0.684 | 0.605 | 0.341 | 0.680 | 0.676 | 0.312 | 0.069 | —[c] | 0.035 |

** Significant at 5 percent.
*** Significant at 1 percent.
a. Per capita GDP is based on purchasing power parity; *t* statistics are in parentheses.
b. The Rule of law was instrumented with the settler mortality (log).
c. Not reported by Stata because the instrument is not valid.

However, given the limitations of the data (especially for the poorer countries), it is unclear whether the links between income and institutions are similar between rich and poor countries.

A second question is related to the dynamics of adjustment toward a long-run equilibrium between institutional quality and income. Since Kaufmann and Kraay's empirical estimates are based exclusively on a cross section, they are right to point out that nothing can be said on the subject on that basis. This is at odds, however, with the very title of the paper and with the availability of institutional quality data covering several decades (for instance, International Country Risk Guide, Freedom House, and Transparency International). The authors are also right in stating that their findings do not necessarily show in short periods of, say, five years or so. Even given this caveat, short-run evidence should at least not be inconsistent with the long-run findings. Specifically, for their conclusions to hold, two results should obtain. First, the standard result shows that countries with good institutions relative to their initial income level should not grow more slowly in subsequent periods than countries with the opposite conditions. Second, the novel result that countries with high initial income levels relative to the quality of their institutions should not experience better improvements of their institutional quality than countries with the opposite conditions.

I checked these predictions using the two sets of institutional indicators assembled by Kaufmann, Kraay, and Zoido-Lobatón for 1997 and 2001. I do not find any evidence to reject the standard proposition that institutions are good for growth: most of the correlation coefficients between the (excess of) institutions in 1997 and subsequent growth (1998–2001) are either positive and statistically significant or insignificant (see table 7). For the richer half of the countries, five of the six correlation coefficients are positive and highly significant, suggesting that institutional quality is more important for income in rich countries in the short run (which is consistent with the cross-sectional results).

With regard to the second prediction, the short-run results are inconsistent with the main thesis of the paper: the excess of initial income correlates positively with subsequent changes in institutions for the whole sample of countries, with five of the six correlation coefficients being highly significant. The results are less clear for the two subsamples, especially for the poorer half, presumably owing to the poor quality of the data on institutional quality.

**TABLE 7. Exploring the Dynamics between Income and Institutions**
Cross-section bivariate correlation coefficients

| *Sample* | *No. countries* | *Rule of law* | *Voice and accountability* | *Political stability and lack of violence* | *Government effectiveness* | *Regulatory quality* | *Control of corruption* |
|---|---|---|---|---|---|---|---|
| *Initial excess of institutions (1997) vs. average growth (1998–2001)* | | | | | | | |
| World | 148 | 0.0560 | 0.0220 | 0.3672* | 0.1125 | –0.1905 | 0.0826 |
| Poor | 74 | –0.0265 | –0.1040 | 0.3394* | 0.0010 | –0.2124 | –0.1586 |
| Rich | 74 | 0.3164* | 0.3708* | 0.4591* | 0.3028* | –0.0178 | 0.3522* |
| *Initial excess of income (1997) vs. change in institutions (1997–2001)* | | | | | | | |
| World | 143 | 0.3197* | 0.1140 | 0.3378* | 0.4379* | 0.1902* | 0.2445* |
| Poor | 71 | 0.1025 | –0.0012 | –0.0169 | 0.2845* | –0.0789 | 0.0384 |
| Rich | 72 | 0.2217 | –0.0673 | 0.2592* | 0.2780* | 0.2633* | 0.0021 |

* Significant at 95 percent.

Source: Author's calculations, based on institutional indexes from Kaufmann, Kraay, and Zoido-Lobatón (1999a, 2002) and per capita GDP from IMF (2002) and World Bank (2001).

a. The excess of institutions is the residual of the regression of the institutional index (1997) against per capita GDP (log, adjusted by PPP, 1997). The excess of income is the residual of the regression.

Thus, while the short-term evidence supports the positive influence of institutions on income, it tends to contradict the hypothesis that income is bad for institutions. These exercises, though rudimentary and limited by a period of analysis that is admittedly too short, shed some light on the importance of studying the dynamics of the relationship between income and institutions.

A third question raised by Kaufmann and Kraay's empirical results is the channel through which the negative influence from income to institutions takes place. The authors argue that elite influence and state capture are the main factors leading to that result. Their account of the experience of the transition economies of eastern Europe is illuminating and interesting, but it is far from compelling and it does not fit well with other arguments in the paper. First, the deterioration of public institutions in those countries was initially led by a decline rather than an increase in income levels. Second, those economies are going through a severe process of transformation toward market economies that is unique in many aspects and can hardly be considered representative of the typical development process. Third, the authors are resorting to the same type of short-term analysis that they previously discarded as invalid for studying the relationship between income and institutions. Finally and most importantly, they are selecting only one of many possible channels through which the negative influence of income on institutions can take place. For instance, income increases may lead, over periods of decades, to demographic changes that may render some of the former institutions unsustainable. An increase in the proportion of young people in the population may cause a reduction in the traditional channels of political representation. Income increases may accelerate the process of urbanization beyond the possibilities of providing certain public services, such as security and justice. Urbanization may also disrupt the networks of social support and cooperation and may erode trust, leading to a loss of credibility of other institutions. Income increases may also go hand in hand with a greater diversification of economic activities, regions, and even life-styles, thereby weakening social identification and deepening all types of social and political fragmentation.

Kaufmann and Kraay have opened an immense avenue for future research. Any good paper should provide more questions than answers. Theirs certainly does.

**Daniel Kaufmann and Aart Kraay respond:** We would like to thank Eduardo Lora for kindly contributing a discussion of our paper. Nevertheless, Lora raises three major issues with which we find ourselves in disagreement.

—*The links between institutions and income are different in rich and poor countries*. Lora makes this point based on the observation that for rich countries, the OLS and IV estimates of a regression of per capita income on governance lead to coefficient estimates that are similar in magnitude, while for poor countries the IV estimates are much larger than the OLS estimates. Since our finding of negative feedback from income to governance depends on a larger IV than OLS estimate, he concludes that negative feedback is not relevant for rich countries.

We do not find this point to be particularly compelling. Rich countries are rich today because they have grown rapidly over the past two centuries, while poor countries are poor today because they have not. The empirical framework we use is designed to capture how the deep historical determinants of institutional quality affect growth performance in the very long run, and so it is not surprising that most of the action in our results comes from differences between rich and poor countries, rather than from differences within the two groups.

—*The time-series evidence on changes in incomes and changes in governance is inconsistent with our finding of negative feedback*. Lora argues that we should take seriously the short-run time-series implications of negative feedback from per capita income to governance. He then reports a positive correlation between the change in governance over the period 1997–98 to 2000–01 and what he calls initial excess income, defined as the residual from a cross-sectional regression of per capita income on institutions in 1997. He interprets this as evidence against negative feedback.

Again, we are not persuaded by this point. The cross-country relationship between incomes and governance that we study reflects the complex interactions between institutions and growth in the very long run. Our simple empirical framework can only summarize the long-run outcomes of this intricate process in which there are likely to be long and variable lags in the relationship between fluctuations in income and fluctuations in institutional quality. In short, our empirical framework has no implications whatsoever for the relationship between institutions and growth over shorter periods, let alone the very short three-year interval Lora considers.

Even if we were to attempt to estimate these short-run dynamics, we doubt that correlations such as those Lora reports would be useful. Recall that the correct interpretation of our main cross-sectional result on negative feedback is that purely exogenous shocks to income (that is, shocks that have no direct effects on governance, but affect it indirectly only through their effects on income) would be negatively correlated with governance. Yet observed shocks to income reflect much more than these hypothetical exogenous shocks; in fact, many of the factors affecting income in the long run are likely to also have direct effects on governance that will dominate any negative feedback. This is why uncovering evidence of negative feedback requires either convincing instruments for income (which are exceedingly difficult to find) or an alternative identification strategy such as the one we propose. Surely the first step toward uncovering short-run dynamics would also be to find comparable exogenous shocks to income over shorter periods. The evidence Lora reports, however, makes no attempt to isolate the exogenous shocks to income that would be required to identify our effects.[1]

*—The example of state capture in transition economies is not compelling as a mechanism of negative feedback.* Lora makes this point based on the observation that during the rather unusual decade of transition, declines in income were accompanied by deteriorations in governance, which is the opposite of what he would expect if negative feedback were present.

We believe he misses the point of our discussion on state capture. Our point is certainly not that the observed output declines in transition economies should have been accompanied by improvements in governance, as a very naive interpretation of negative feedback might suggest. As discussed above, only purely exogenous shocks to income that have no direct effects on governance should exhibit this negative correlation with institutional quality through the negative feedback channel. In fact, the experience of the transition economies is a great example of a shock that affects both income and institutional quality directly: transition led to the loss of the old economic system, causing huge dislocations in the pattern of production, and also to the collapse of socialist institutions, which

1. We surmise that in constructing excess income, Lora is attempting to measure the empirical counterpart of the error term in equation 4 in our paper, $e_j$. Since OLS applied to equation 4 is inconsistent, Lora is not retrieving a consistent estimate of $e_j$, let alone the exogenous component of this variable.

created a power vacuum. In many countries in the former Soviet Union in particular, this led to capture of the fledgling new states by powerful elites and a worsening of governance. It would thus be reasonable to expect a positive simple correlation between output and governance changes. However, looking at such simple correlations between growth and institutional quality in transition economies would be exactly the wrong way to try to find direct evidence of negative feedback.

Rather, our point here is more subtle. In order to understand how improvements in income might lead to deteriorations in governance, we speculate that it is important to understand who gains from these increases in income. If these gains accrue to powerful elites with influence over the policies, laws, and regulations of the state, it is plausible that there will be strong incentives for these elites to continue to subvert the institutions of government in order to protect their interests. This could lead to an ongoing process in which, other things equal, increases in income are associated with continued declines in governance. In our view, the emerging evidence on state capture from both transition economies and selected countries in Latin America does suggest such a hypothesis is plausible, and worth exploring in depth in future research.

**Lant Pritchett:** This is a lovely paper, one that I enjoyed reading. It has two loosely connected halves, both of which are thought provoking and innovative. As with many things lovely, however, I just don't believe the first half, despite the attractiveness.

The first half I enjoyed because it seriously considers the differences between instrumental variables and OLS estimates. Many authors simply do OLS and then IV without really examining closely the source of the gap: $\beta_{IV} - \beta_{OLS}$. But if there is a difference, then this difference does, in fact, say something about the structural equations and the error. Since it is possible to sign the two most common sources of bias—measurement error and simultaneity—the difference might even be able to say quite a lot. If measurement error and simultaneity are working in opposite directions, then one cannot really say much without an independent estimate of the magnitude of measurement error. The truly very clever innovation of the first half is to use an independent measure of measurement error of the governance variables (because their variables were themselves constructed from multiple estimates) to then estimate the magnitude of atten-

uation bias and point out that unless measurement error is huge, this is inconsistent with a positive impact of income on governance.

Just as no good deed goes unpunished, so also is cleverness sometimes rewarded with an embarrassment of riches. Had the authors' technique indicated that the impact of income on governance was smaller, even much smaller, than is routinely claimed, I would have been happy to believe them. If it told me that the impact of growth on governance was zero, I would be puzzled, but I would at least have given a sympathetic ear to their argument. But their default estimate is that a doubling of income leads to almost a one-standard-deviation reduction in the rule of law. This base case estimate implies that an exogenous doubling of per capita income would lead the rule of law to deteriorate by 0.90 (1.28*0.7). In other words, this would transform rule of law from the level of the Bahamas (0.85) to that of Ghana (–0.08) or from Costa Rica (0.61) to that of Ethiopia (–0.24). Their estimates are so big as to be incredible, in the sense of cannot be believed.

There are three justifications of my disbelief. First, the finding simply defies history. In the now-developed countries, governance has improved tremendously since 1870, while per capita incomes have increased more than tenfold. The paper's predicted impact of that income growth is that the United States, for instance, should have seen a deterioration in rule of law by 2.83 points (roughly from Costa Rica to Zaire). Instead, the United States is now at 1.58, which represents a considerable increase. This rough statement ignores both the feedback effect from governance to growth and the fact that other things that were good for growth were also good for governance, so I am overstating the case, perhaps a lot. Even so, the nongovernance-related growth should have had these negative effects. If income really leads to worse governance, then either the history of the Organization for Economic Cooperation and Development (OECD) is an amazing counterexample or governance and governance-related causes account for nearly all growth. As it turns out, I can't think of a single example of a country in which sustained increases in income were associated with sustained deterioration in governance, though I can think of lots of examples of better-better and worse-worse.

Second, accepting the authors' result of a negative effect of income on governance involves giving precedence to the math over common sense, but they themselves bravely inform their readers that all that is required to not believe their present work is to disbelieve their previous work. That is,

the larger the measurement error in their indicator of governance, the more attenuation bias can reduce, or even change the sign of, the effect of income on governance. So, if the measurement error in the governance indicator is twice as big, then the income effect is positive, not negative. The authors argue that these are incredibly large measurement errors, in that one could not distinguish Mexico from either Haiti (below) or Chile (above) at the 90 percent level. Keep in mind, however, that disbelieving the negative coefficient is not the same as believing that Mexico and Chile actually have the same governance or even that your prior assumptions are unable to distinguish the difference. Rather, disbelieving the negative result involves believing that *their estimates* cannot distinguish Chile and Mexico at a high level of statistical reliability. The authors understandably "find this degree of measurement error implausibly high," but if I don't, there is nothing to stop me from accepting the implications. If I have to choose between believing that income worsens governance or that their cross-national estimates of governance have low precision, I'll take the latter.

Third, the paper does not offer a plausible specification of the causal mechanisms whereby income worsens governance. As it is, I believe that equation 5, which postulates a linear link between income and governance, is misspecified. That is, "income" does not appear to be a useful summary statistic. Suppose one country's income increased because a single natural resource doubled in value, while another's increased because the quantity and quality of its people's education were raised. These processes would not have the same impact at all. It is not at all clear to me, therefore, what exactly gamma is meant to identify—the pure income effect as a demand-side phenomenon? Governance results stem from complex political and social interactions, and no model can produce an equilibrium relationship between income and governance that is invariant of the source of income, the political forms, institutional history, and so forth.

That, incidentally, is why I love the second half of the paper, which says two very sensible things with which I agree—and which can be maintained and defended without the first half of the paper. First, waiting for income increases to work their magic is a silly strategy for improving governance. That is, relying on general development to improve governance in the absence of specific actions simply because people might believe that income causes governance is unwise. It would be unwise even if there

were a large income coefficient, since these types of reduced-form relationship are not destiny. Even if gamma is large and positive, it does not rule out of the possibility of attaining large improvements in governance based on purposive public policy reforms with a high benefit-cost ratio. Linking the two arguments (the low income impact and the need for purposive actions to improve governance) actually weakens the latter, which is the more important of the two.

Second, the section on state capture is wonderful and informative. The information on the negative spillover of actions by the capturing firms on others is a striking and important result. It is not at all clear, though, how it links to the argument about income and governance, since growth could actually be higher with state capture if the investment impacts of the capturing firms are larger than the disincentive effects on others. Furthermore, there is no reason why income growth should materially alter the incentives for state capture—I would think that, as a firm, if you could you should—and certainly there is no reason to believe that all income-increasing changes would increase capture.

In sum this is an action-packed, interesting, and thoughtful paper. It is right where it matters, namely, on the implications for future research and policy, and possibly wrong only where it doesn't.

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