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Rate of Economic Growth, Level of Development, and Income Inequality: Rejoinder to the Reply by Edwards and McGuirk

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CONTINUATION OF EXCHANGE BETWEEN JEFF EDWARDS AND ANYA MCGUIRK, AND JIH Y. CHANG AND RATI RAM FROM THE AUGUST 2004 ISSUE OF *EJW*.

WE APPRECIATE THE OPENING PARAGRAPH OF THE REPLY BY Edwards and McGuirk (2004b). However, the Reply contains largely irrelevant or inconsequential statements. To place this exchange in perspective, we recall that our original paper (Chang and Ram 2000) suggested, on the basis of estimates of a Kuznets-quadratic from cross-country data, that high-growth economies are likely to experience lower income inequality at any given income level. In addition to some secondary matters, Edwards and McGuirk (2004a) claimed that our conclusion does not hold if two regional intercept dummies are included in the regression model. We pointed out (Chang and Ram 2004) that Edwards-McGuirk's

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claim was simply inaccurate because, contrary to their own basic point, the predicted inequality numbers were derived by ignoring parameters for the regional dummies. We also showed that even when the two regional dummies are included, one observes essentially the same pattern as indicated by Chang and Ram (2000) if accurate predicted values of inequality are used. The long Reply (Edwards and McGuirk 2004b) provides no meaningful basis for altering the aforesaid position. The contents of the Reply appear largely irrelevant to the main point or are inconsequential or inaccurate. It might have been reasonable for them to "step back and give some perspective on" their comment if they focused on the core of the exchange and dealt with secondary matters briefly in a transparent manner. However, most of the Reply deals with secondary aspects in a diffuse style, which makes it difficult for us to formulate a structured response. The following paragraphs offer brief observations on a few aspects that we are able to comprehend in the Reply.

1. Their observation about being "skeptical of, and disappointed with, the current state of published empirical work in economics" (244) has little relevance to the substance of the exchange. Irrespective of the basis of their comment, they should deal with the main point of our reply (Chang and Ram 2004). If they carry any general dissatisfaction or disappointment about the state of empirical research in economics, its expression is irrelevant to this exchange. To the extent their reservations can be cogently articulated, these belong in a more general paper about the "state of published empirical work in economics." Their Reply should deal primarily with the point whether Chang and Ram (2004) are right in saying that the predicted inequality numbers used by Edwards and McGuirk (2004a) were wrong for many countries and that accurate predicted values from even their own model yield largely the same pattern as noted by Chang and Ram (2000). It is inappropriate to introduce more general issues about the "state of empirical research" or to bring in views of scholars like Leontief, Pagan and Spanos on those general aspects.

2. Their observations on page 246 about what they were doing in the comment seem like an afterthought. At any rate, most of this part is redundant in response to the main point of our reply. While one wishes that these assumptions were "easily testable," listing of the standard assumptions of the linear regression model seems particularly pointless since these can be found in almost any elementary econometrics text. We also believe that the word "forged" from Pagan's quote, stated (246) as a possibility relative to our results, might be more applicable to a procedure

like that followed by Edwards and McGuirk in which the specification (and the set of estimates) is "tortured until it confesses."

3. The further discussion (Edwards and McGuirk 2004b, 247-248) about the choice of regional dummies is also irrelevant. Although the dangers of dropping "insignificant" variables are well known, there is no need to make an effort to justify their choices when we have shown (Chang and Ram 2004) that even with the inclusion of their two regional dummies, Chang-Ram (2000) conclusion is not affected if accurate values of predicted inequality are used.

4. Their statement (249) about what they did in the comment (Edwards and McGuirk 2004a) is wrong. They generated predictions inaccurately by ignoring parameter estimates for the regional dummies and (amazingly) assuming "the countries are all in the control group" (Edwards and McGuirk 2004a, 230). While it is obvious that their procedure would generate accurate predictions for the base group, they can derive no comfort from that since the predicted values for all "nonbase" countries were wrong. It is surprising that Edwards and McGuirk are hesitant to acknowledge even this obvious and major error, and are trying to obscure it. As a minor aspect, what we said on this point (Chang and Ram 2004, p. 239) was that the comment ignored parameters for regional dummies, and not that they did not model regional differences.

5. The "another attempt to illustrate the differences in predictions" (Edwards and McGuirk 2004b, 250) is misleading. It is not correct to compare high- and low-growth inequalities for the "base" countries separately from those for "CA" and "E" regions. Since the estimates are derived from the combined sample, predicted inequalities for high- and low-growth cases *must* be compared for the entire group of 48. The blue and red "smooth" curves in their Figure 1 (250) are a distortion. The related statements about there being "little difference in the inequality-GDP relationship between . . . high- and . . . low-growth countries," the differences being "economically insignificant," and their model not predicting "that higher growth countries have lower inequality at all income levels," are also misleading. These refer only to the base group and are inaccurate. The only valid and meaningful comparison is between predicted values of inequality for *all* high-growth and *all* low-growth cases in the sample. These predicted values are shown in Table 2 of our reply (Chang and Ram, 2004, 240) and plotted in our Figure 1 (241) which supports the conclusion stated in our original paper (Chang and Ram 2000). If accurate, every point in their Figure 1 (Edwards and McGuirk, 2004b, 250) must be

identical with that of our aforesaid Figure 1; the separation into regions is misleading and seems intended to obscure the failure of their comment.

6. The statements below their Figure 2 (251) are inconsequential or inaccurate. It is obvious from their own exposition that the two regional dummies capture to a substantial extent the high- and low-growth dichotomy in the sample. At any rate, we have shown that the correct predictions even from models that include the two regional dummies used by Edwards and McGuirk yield the conclusion that high-growth countries have lower inequality.

7. The long discussion about Kuznets hypothesis (Edwards and McGuirk 2004b, 252-255) is a distraction and misleads the reader from the main point. Edwards and McGuirk (2004b, 252) acknowledge that this is a "minor focus" for us, but still devote more space to this than to the core points. The futility of this part of their Reply is indicated by several other considerations also. First, if they look at their own Figure 1 (Edwards and McGuirk 2004b, 250), they will find an unmistakable Kuznets-U in each of the four smooth curves, which, of course, are not relevant to a comparison of the inequalities in high- and low-growth cases. Second, the scatter in their Figure 3 (252) is incomplete since it includes only 48 countries, and cannot be used to judge the presence of a Kuznets pattern in the sample. Even when the hypothesis holds in the entire sample, one can always find a subset where it does not hold. We have shown (Chang and Ram, 2004, 237) that even when two regional dummies are included, there is strong evidence of a Kuznets-U in the full sample. Although we postulate parametric variations in high- and low-growth cases, the specification used by Edwards and McGuirk (2004b, 253) is less appropriate than ours since Kuznets-hypothesis should be explored on the basis of what might be called "averaged" parameters for the entire sample. It is pointless to try to explore the hypothesis in subsamples. It seems Edwards and McGuirk (2004b) do not realize that our consideration of low-growth and high-growth samples was meant *only* to show that the evidence on the hypothesis depends on the data and not the quadratic model. It is difficult to see what could possibly be gained by redoing our exercise after including regional dummies. Incidentally, contrary to what Edwards and McGuirk (2004b, 255) state, their different findings on Kuznets hypothesis for low- and high-growth cases are obviously detrimental to the claim that there are no significant parametric differences between the two groups.

8. The part on "final aspect of (our) complaints" is also inaccurate or inconsequential. We have addressed the "first aspect" in the foregoing paragraphs. To repeat, even using the model that Edwards and McGuirk

find "statistically valid," accurate numbers for predicted inequality refute the claim made by them in the comment and support the Chang-Ram (2000) conclusion. We have also noted that the "fatal flaw" in their comment was to use inaccurate values of predicted inequality for many countries. The "second aspect" is based on a false premise. There is *not* an "overall lack of significance of most of the variables in the model." On the contrary, our EDCC estimates (Chang and Ram, 2000, 792), on the basis of which Edwards and McGuirk proceed further, show that, despite the limited sample size and considerable collinearity, each income term is significant at least at the 10% level, and the three high-growth dummies are jointly significant at the 6% level. Even in the specification that includes regional dummies, the three high-growth dummies are jointly significant at better than the 10% level. These are reasonable significance levels, and the premise on which Edwards and McGuirk proceed is false.

In addition to being based on a false premise, introduction of confidence intervals by Edwards and McGuirk is not meaningful for several other reasons also. First, if confidence intervals were really an important consideration relative to our paper, their comment (2004a) should have focused on these instead of complaining about the "Kuznets curveball missing the regional strike zone." Like the bulk of the Reply, confidence intervals are an afterthought, and amount to improperly writing another comment. Second, standard practice in the profession is to base inferences about the significance of the estimates and the parametric differences on the point estimates and the related standard errors. We have already noted that the three high-growth dummies in our (2000) estimates, which Edwards and McGuirk use for confidence intervals, are jointly significant at the fairly stringent 6% level and each income term is significant at least at the 10% level. Structure of confidence intervals would just reflect the point estimates and the standard errors and can shed *no* light on the "significance" of the differences in predicted inequalities for the two groups. Third, by choosing to work with 95% confidence intervals, Edwards and McGuirk are apparently looking for significance at the 5% level. It is not evident where the sanctity of the 5% level comes from. It is just one of the conventional levels, and the 10% level may be more appropriate in this case. Fourth, if Edwards and McGuirk consider 5% as the only acceptable level for judging "significance" or "confidence," it is evident that our estimates (Chang and Ram 2000, 792) do not meet that criterion. Instead of writing 29 pages of the comment and the Reply, Edwards and McGuirk could just have written a page suggesting rejection of our conclusions due to lack of significance at the 5% level. Fifth, aside from everything else, and

even assuming that the confidence intervals are accurate, these support our (2000, 2004) position that high-growth economies are likely to experience lower inequality. It can be seen from their Figure 4 (257) that, like the predictions based on the point estimates, each bound of the 95% confidence interval for high-growth cases lies below the corresponding bound for the low-growth cases at almost every income level. Thus the pattern indicated by the predicted values based on the point estimates is reinforced by confidence intervals. Judging, without any criterion, "width" of the confidence intervals, or considering the location of predicted values relative to the intervals, can generate no inference about the "degree of confidence" or significance of the difference between the inequality profiles in the two groups. By introducing confidence intervals and mentioning the aforesaid characteristics of the intervals, Edwards and McGuirk seem to be only trying to obscure the failure of their comment. As an aside, it is interesting to note that Edwards and McGuirk work with confidence intervals based on our estimates and not on their "statistically valid" model. Perhaps the confidence intervals based on their model did not look as "good" despite being "almost as large as those of Chang and Ram" (257).

We also make a few remarks about their "thinking" articulated near the bottom of page 255. As we tried to explain in our reply (Chang and Ram 2004), it is *not* true that if the variables are unrelated, estimating a quadratic specification would indicate "some sort of quadratic relationship." The reasoning is spurious. Following that argument, one can also say that if the variables are unrelated, estimating a linear model would indicate "some sort of a linear relationship" and estimating a cubic (or a logarithmic function) would indicate "some sort of a cubic (or logarithmic) relationship." It should also be noted that, contrary to what Edwards and McGuirk seem to suggest, patterns of predicted inequality are not dependent on a quadratic specification. These should show in any reasonable functional form. We used the Kuznets-quadratic because it is a widely-adopted model for studying the relation between income and inequality.

10. While Edwards and McGuirk claim to worry much about "statistical adequacy" of the models and "reliability of inference," they seem to overlook the point that consideration of an appropriate level of significance is an important part of statistical inference. Given the extensive discussion in the literature about the nexus between income and inequality, it is probably not true that the hypothesis of income (level of development) having no association with inequality is so strong as to require overwhelming evidence for its rejection. On the contrary, a reasonable

approach would probably be to reject the null on the basis of "considerable," and not overwhelming, evidence, and a significance level of 25% (or even less stringent) might be more appropriate than the conventional levels. Such a thought renders even less meaningful their "second aspect," the remarks about "overall lack of significance of most variables," and introduction of 95% confidence intervals.

We conclude by saying that, while we appreciate the tone of the opening paragraph of Edwards and McGuirk (2004b), nothing in their Reply mitigates the failure of the main argument of their comment (Edwards and McGuirk 2004a) that inclusion of two regional dummies alters the Chang-Ram (2000) conclusion. We showed in our Reply (Chang and Ram 2004) that (a) the predicted inequality numbers used by Edwards and McGuirk (2004a) in the comment were wrong in a major way, and (b) accurate predicted values even from their "statistically adequate" model yield the same pattern as stated in our original paper (Chang and Ram 2000). On the first point, Edwards and McGuirk seem to admit the error, although only indirectly and hesitantly. On the second point, the only relevant parts in the long Reply are expressed through Figure 1 (250) and Figure 4 (257). We have shown that the "smooth curves" in Figure 1 are a distortion and are irrelevant to the issue. Figure 4 is based on a false premise, is clearly an afterthought, chooses an arbitrary confidence level, is not consistent with standard econometric practice for judging "significance" of differences in the parameters (and the related fitted values), and can yield no information about the significance of the differences in the inequality profiles for the two groups. Moreover, even if one were to make some use of 95% confidence intervals for shedding light on the main point, an appropriate interpretation of the confidence intervals reinforces our conclusion that high-growth economies are likely to experience lower income inequality.

Despite the failure of the main argument of Edwards and McGuirk, we do not claim that our conclusion is infallible. We are conscious of the hazards of drawing strong conclusions from studies of this type, and made it clear (Chang and Ram, 2000, 795) that the stated inference is subject "to the caveats appropriate for studies such as this one, which work with simple models and cross-sectional data from samples of a modest size." The difficulties in such contexts, particularly the one about drawing policy-relevant inferences from cross-country data, are well known. However, the points made in the comment and the Reply by Edwards and McGuirk (2004a, 2004b) have almost no bearing on those difficulties.

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