Reply to the referee report No. 2

I thank an anonymous referee for these comments.

If I understand her/him right, the referee has two types of (interrelated) criticisms. One with regard to the methodological question whether the estimated coefficients can be interpreted as causal and a second criticism on the issue of generalized policy conclusions from the estimation results.

I start with the first – the methodological – issues. The referee writes: "*... causality from financial liberalization is not proven at all. Other factors could explain the empirical results. For example, the nature and the origin of the shock to the financial system may have played roles. I don't think one can draw conclusions without having a view of the channels of influence and without checking whether data are consistent with the specification of the channels.*"

I do not agree. I think it is possible and usual to identify causal effects of "treatments" (here financial regulation before the start of the financial crisis) without exactly knowing or identifying the "causal mechanisms" or "channels". In an influential article Holland (1986; P. 959) explains this as follows:

> *"<u>The analysis of causation should begin with studying the effects of causes rather than the traditional approach of trying to define what the cause of a given effect is</u>. [...] Traditional analyses of causation start by looking for the cause of an effect. I think that looking for causes of effects is worthwhile scientific endeavor, but it is not the proper perspective in a theoretical analysis of causation. Moreover, I would hold that the "cause" of a given effect is always subject to revision as our knowledge about the phenomenon increases. For example, do bacteria cause disease? Well, yes ... until we plug deep and find that it is the toxins the bacteria produce that really cause the disease; and this is really not it either. Certain chemical reactions are the real causes .... and so on, ad infinitium."*
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> *Source*: Paul W. Holland (1986), Statistics and Causal Inference, *Journal of the American Statistical Association* Vol. 81, No. 396 (Dec., 1986), pp. 945-960

Nevertheless, I do agree that it would be desirable to learn as much as possible about the channels of influence in order to give detailed policy conclusions. And in my reply to the first referee report, I mentioned already:

> *"I am in complete agreement with the reviewer with regard to this issue. This has already been reflected in the Introduction (Page 3) and the Conclusion (Page 24) of the paper. And there is no doubt that this is a broad field for future empirical and theoretical research. Nevertheless, the paper includes some empirical indications, which have not been sufficiently discussed in the paper yet: As mentioned at the bottom of Page 11 of the paper, for all three models it is tested whether controlling for the size of the financial sector ("financial deepening") changes the estimated effect of financial regulation (Columns (7) and (8) in Tables 2, 3 and 4). Since this is not the case, one may conclude that the effect does not work through the (relative) size of financial markets, but through some qualitative / structural features of financial markets. I will lay emphasis on this result in the revision of the paper."*
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> *Source*: Tobias Hagen, reply to the first referee report www.economics-ejournal.org/economics/discussionpapers/2013-26/comment.2013-05-16.0382032960/at_download/file

Let me reply to the referee's statement *"Other factors could explain the empirical results."*. I discussed many potential "confounding factors" in the paper on page 11. First of all, as explained and discussed in my reply to the first referee report, endogeneity in the sense that financial regulation (in 2005) is a function of the events after 2007 is almost impossible. Hence, assuming exogeneity of the financial regulation variable is definitely valid.[1] Secondly, the issues of functional form assumption, outliers and low variance are extensively treated in the paper. Thirdly, there may be an omitted variable bias, that is, a biased estimate of the effect due to the omission of variables which are correlated with the outcome variable as well as the financial liberalization index. This issue is discussed in the paper on Page 11. From my point of view, the central result is: I am not able to "eliminate" the statistically significant coefficients of the financial liberalization index in all three models (output, employment, deficit) despite controlling for all possible factors discussed in previous studies. It would have been beyond the scope of the paper to show all the regression output –the current version of the paper already includes 28 different specifications. Further concrete suggestion for control variables or different specifications are welcome.

The referee goes on: "*The proxy for financial liberalization adds the liberalization scores for seven dimensions in the liberalization database. One of these dimensions is strength of capital regulation and supervision. A higher score in this dimension actually represents the opposite of liberalization. If the authors run a regression with strength of capital and regulation and supervision as independent variable the results would most likely show that both stronger regulation and liberalization were associated with poorer academic performance*".

Yes, that is absolutely right. This dimension of the proxy has been studied by Machiandro et al. (2011). On page 2 of my paper you can find a summary of their paper:

> "*Besides other concepts, Masciandaro et al. (2011) make use of the same financial reform index as this paper. They reveal that the countries with the most liberalized financial system were hit the hardest by the crisis. They focus on the effects of various features of supervisory architecture and governance on economic resilience of a set of about 100 countries. Their findings show that they were negatively correlated with economic resilience.*"

That means, we get the surprising result that a higher capital regulation and superivison did not "help" at all. And I agree, that I could (and should) emphasize this aspect more strongly in the paper.

So, why didn't I include the seven dimensions of the financial regulation index separately? First of all, the creators of the index Abiad et al. (2008) define financial liberalization and the index as "*…reduction in the role of government, and an increase in the role of the market, in allocating credit. We measure this using a new financial-liberalization index, which takes into account credit controls, interest rate controls, entry barriers for banks, regulations, privatization, and restrictions on international financial transactions.*".[2] I take them by their words and treat this variable as a "black box" which serves as proxy for the financial liberalization of a country. Doing so, I follow some influential

[1] As stressed by Giannone, Lenza, and Reichlin (2011), the shock of 2007 provides "… *a unique opportunity to identify the link between the structural characteristics of economic and institutional systems before the crisis and their resilience with respect to the global recessionary shock*." In other words: the shock of 2007 can be interpreted as exogenous – and, analyzing its effect implies a cross-sectional approach by definition. Source: www.economics-ejournal.org/economics/discussionpapers/2013-26/comment.2013-05-16.0382032960/at_download/file

[2] Abiad, Abdul, Nienke Oomes, and Kenichi Ueda (2008), The Quality Effect: Does Financial Liberalization Improve the Allocation of Capital? *Journal of Development Economic* 87 (2), pp. 270 -82.

studies which more or less conclude that financial liberalization (as measured by this proxy) lead to higher long-term growth[3]. So I think it is quite interesting to use exactly this aggregate proxy to demonstrate that having a financially liberalized system (in terms of the proxy) was harmful during the crisis. The second reason is, the lack of degrees of freedom (only 88 observations) and low variation between the countries which renders it practically impossible to identify anything using cross-sectional data.

Sorry, I do not understand "*academic* performance" in the referee report.

The second criticism of the referee is on the conclusions on page 24 of the paper. The referee writes: "*One criticism I have is that the author claims or suggests this causality from liberalization to economic performance with a claim to generality. The evidence is clear in this particular crisis but can one draw the conclusion with respect to financial shocks more generally? Evidence from other crises would be needed for such a conclusion.*"

I do agree, that some statements in the conclusion could be rephrased in a more reluctant way. I suggest to modify the Conclusion on page 24 as follows

> *"~~While~~ Even the approach chosen in this paper cannot identify the channels through which financial liberalization amplifies macroeconomic instability. ~~the result of a causal negative effect of financial liberalization on macroeconomic stability is quite robust. In concrete term~~ , it has been found that the higher the financial regulation index by Abiad et al. (2008), and, hence, the more liberalized the national financial markets were before the shock in 2007, the more severe were the subsequent output and employment losses as well as the fiscal crises. One essential conclusion can be clearly drawn: more restrictions on financial activities could have reduced the likelihood of suffering large output and employment losses and government debt increases after the 2007 shock. Hence, this paper continues the series of empirical research indicating the adverse effects of financial deregulation on macroeconomic stability ~~and economic development~~ after the shock. Even if the mechanisms of financial regulation are unclear, the empirical results stress that the euphoric affirmation of financial deregulation as an effective policy for economic development cannot be maintained.*"

I do not want to claim that I can conclude a lot with respect to financial shocks in general. That should already be clear from the title of the paper, and – after some minor revisions indicated above – from the Conclusion as well. However, I think that my results have indeed major implications simply due to the severity of the crisis. Obviously, this is not one "ordinary" financial crisis among others, but *the great recession*. So, if one believes in the results of this study and several others, then the implications are serious even if one holds the opinion that they cannot be generalized at all.

Of course, evidence from other crises would be interesting. That is exactly one reason, why I cite Stiglitz (2000) in the conclusion, who summarizes some experiences from the East Asian Crisis. And I am looking forward to more research in this field. And I will certain be engaged in such research.

[3] Christiansen, Lone, Martin Schindler, Thierry Tressel (2013), Growth and structural reforms: A new assessment, Journal of International Economics, Volume 89, Issue 2, March 2013, Pages 347-356.
Abiad, Abdul, Nienke Oomes and Kenichi Ueda (2008) The Quality Effect: Does Financial Liberalization Improve the Allocation of Capital?, Journal of Development Economics 87(2), 270–282

Finally with regard to the strength of the paper, the referee writes: "*The empirical work has some merit relative to previous (cited) work on the crisis. Some controls are added to simple regression analysis, and the paper includes additional dimensions of economic performance*."

Let me summarize again, the aspects that are new compared to previous studies:

- Previous study analyze GDP growth only. My paper also includes employment growth rates (2008-2010) and average budget deficits (2008-2010). Hence, it is taken into account that the great recession means not only a large drop in output, but also labour market and fiscal crises in many countries.
- Previous studies only include GDP growth rates of 2008 and 2009. Here, the GDP growth rates of 2008 to 2011 are included. Hence, not only the very short-run effects are analyzed but also the medium-run.
- Important "econometric problems" such as functional form assumptions and outliers are discussed. For example, China (highly regulated and relatively high growth) or Ireland (liberalized and large drop in GDP) may be interpreted as outliers.

Let me summarize: I am willing to react to the criticisms of the second referee as follows:

1. I do agree that the conclusion can be formulated more carefully and generalizations should be avoided (see the modification above).
2. I do not agree that causality requires the knowledge about "channels". But I agree, that these must be analyzed for detailed policy conclusions in future research. Nevertheless, I will put more emphasize on my result that the "size of the financial markets" – controlling for financial liberalization – did not matter. This can be interpreted such as that the effect did work through some qualitative / structural features of financial markets. Hence, I can provide some indications for a channel at least.
3. I do agree that I should put more emphasize on the dimension "strength of capital regulation and supervision" of the index in the conclusion. That is, indeed, an important information for the reader which is not clear enough in the current version of the paper.

Nevertheless, the results presented in the paper are so significant that they can stand on their own without including more crises / time periods in the empirical analyses. **Hence, some minor revisions of the text can meet the referee's criticisms.**

However, since the referee states that he or she "... *cannot recommend publication in spite of presentation of interesting data and some improvement on related previous empirical work*" one may wonder whether she or he simply does not like the empirical results. This would be understandable since we all (as economists) have been conditioned to believe in the welfare gains of financial liberalizations – also from dozens of empirical studies indicating the positive effects.[4]

Of course, the results are controversial – and that is exactly the main reason why they deserve to be published.

[4] See, for example, the overview in Levine, R. (2005). Finance and Growth: Theory and Evidence, in: P. Aghion and S. Durlauf (ed.). Handbook of Economic Growth, edition 1, vol. 1, chap. 12. Elsevier.