

Referee replies for the manuscript

The real versus the financial economy: A global tale of stability versus volatility
by P. Mundt, N. Förster, S. Alfarano, and M. Milakovic.

We are indebted to the referees for their constructive suggestions. We have taken careful consideration of all comments and hope to meet the referees' expectations.

Before commenting on their suggestions in more detail below, we would first like to address an issue that has been raised by various participants at the 2013 WEHIA meeting in Reykjavik, and which we also feel should be addressed in this paper.

The time period 1997-2011 that we consider in our study contains a period of considerable "securitization" of the global economy that starts in the 1990s, but it also contains the global economic and banking crises that began in 2007. We received several questions along the lines of "Does the average equality of returns to financial and real economic activity also hold without this realignment period after 2007?" To this end, we have split the sample into two parts, namely into the period 1997-2006, and into the subsequent period 2007-2011 that contains the most recent crisis. Reproducing the median regressions (Figures 3 and 11) for the period 1997-2006, we find a slope coefficient of 1.59 ± 0.53 for the long-lived corporations and 1.44 ± 0.66 for the entire sample (both parameters are statistically significant at the 5% level), while for the turbulent period 2007-2011 no robust relationship between the returns to both types of investment could be observed (most likely because the sample is very small). Perhaps somewhat surprisingly, the results for the years prior to the crisis thus turn out to be consistent with the findings reported in section 3 for the entire sample period. Therefore we tend to reject the hypothesis that the average equality of returns is only due to the presence of the crisis in our sample, and have added a corresponding comment at the beginning of the final section ("Discussion").

Referee 1: Their analysis could be improved in several ways. First, given the vast size of their empirical database, they should analyze the relationship of corporate profits with the growth of GNP of the relevant economies.

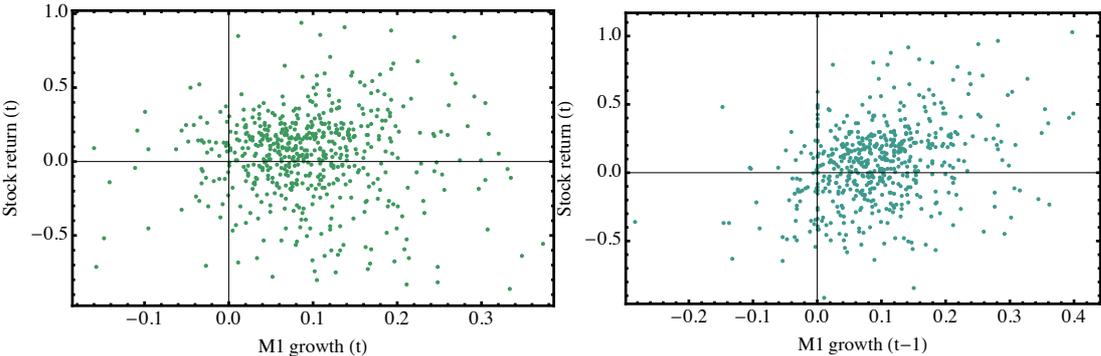
We very much appreciate this comment (although we feel that the relationship between corporate profits and GNP growth is beyond the scope of this paper), particularly with regard to Gabaix's recent granularity hypothesis, which implies that the idiosyncratic destinies of the publicly traded corporations in our sample are of crucial importance for GNP growth and the variations therein. We strongly believe that "aggregating upwards" from granular corporations to NIPA accounting figures is crucial in the quest for the "proper microfoundations of macroeconomics" (see also Acemoglu et al. "The network origin of aggregate fluctuations," *Econometrica*, 2012 and Carvalho and Gabaix, "The Great Diversification and Its Undoing," *American Economic Review*, 2013). Interested readers might care to know that we are currently investigating the granular fluctuations of corporate profitability from this perspective in much more detail, and also with a considerably more sophisticated quantitative approach (see <http://tinyurl.com/pdzsr5x>).

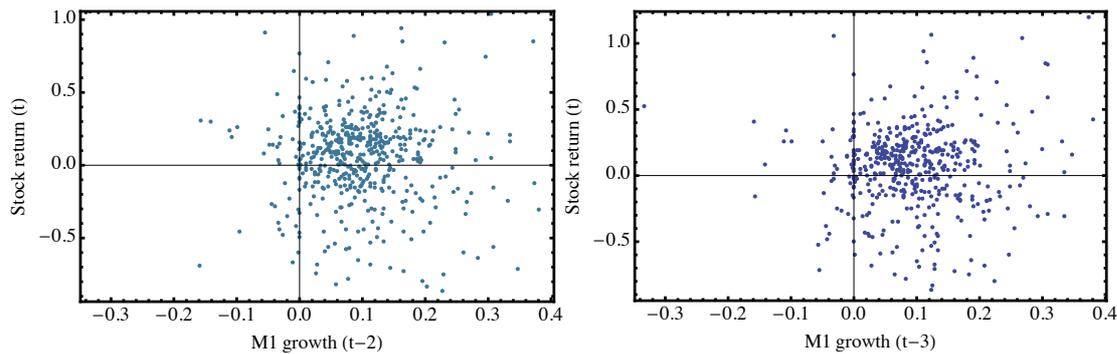
Referee 1: Second, they should analyze more in depth the relationship between risk and returns. Their result shows that in the long run the usual assumptions on risk remuneration are not respected, a fact that deserves analysis.

Clearly, the finding that real and financial investments yield the same return on average but stock returns are more volatile raises the question why investors would seek out financial investments and do not demand a higher risk premium for the commitment of capital to financial activity (which is why we quoted the paper by Shackle in the final section). At this point we can only speculate that financial investors have a certain preference for the higher liquidity of financial investments that seems to compensate for the additional risk. We agree with the referee that this issue deserves deeper attention, and we hope to address it in future research.

Referee 1: Finally, they seem to ignore the process of money generation and the banking system. Much of the volatility of financial markets comes from the availability of money.

The purpose of the scatterplots shown below is to compare the average returns to financial and real investments and their respective volatilities, rather than to discuss possible explanations for the “excess volatility” observed in financial markets, which we merely could speculate about at the present time. The nexus between stock prices and macroeconomic fundamentals has been studied, for instance, by Schwert in his 1989 *Journal of Finance* paper “Why does stock market volatility change over time?” He concludes that the relation between the volatility in money growth and the volatility of financial returns is, if present at all, quite weak. We have also checked whether our data indicate some correlation between money growth and financial returns. The scatterplots below illustrate combinations of yearly stock returns and M1 growth rates. Visual inspection of these plots indicates that there is no clear relationship between the two quantities. (The plots for the monetary aggregates M2 and M3 look very similar, but we are of course aware that monetary aggregates do not provide a complete measurement of credit conditions.)





*Referee 2: The profit rate was often discussed in non-orthodox economics, including Marxist literature. It is not the case of fully reviewing this literature here, but at least, the authors could include a pointer to: Emmanuel Farjoun and Moshé Machover, *Laws of chaos: A probabilistic approach to political economy*. London: Verso, 1983.*

A reference to the book by Farjoun and Machover was incorporated into a footnote on page 3 of the paper.

*Referee 2: When mentioning the statistical equilibrium framework, a pointer to the papers by D. Foley and to the book by Ubaldo Garibaldi and Enrico Scalas, *Finitary probabilistic methods in econophysics*. Cambridge: Cambridge University Press, 2010, could be useful for the reader.*

A reference to the textbook by Garibaldi and Scalas and the Foley paper “A statistical equilibrium theory of markets,” *Journal of Economic Theory*, 1994, has been added in a footnote on page 3 of the revised version of the paper.

Referee 2: I suggest using the annual percentage change in market value instead of the logarithmic return. This would mean repeating the analyses for this variable. Alternatively, the authors could quantify their claim that log-returns should approximate the annual percentage change in market value in an appropriate table. Log-returns are a good approximation of percentage changes if index or price changes are small compared with the baseline. I do expect results to be robust with respect to this variable change, but, here, there is no major reason for preferring an additive random variable that approximates the quantity of interest.

In the pertinent growth rate literature, it is customary to study firm or GDP growth as logarithmic differences (see, e.g., the various contributions by Bottazzi and co-authors in the field of industrial dynamics, or by Fagiolo et al. “Are output growth-rate distributions fat-tailed? Some evidence from OECD countries,” *Journal of Applied Econometrics*, 2008 or by Stanley et al. “Scaling behavior in the growth of companies”, *Nature*, 1996). We do not claim that annual percentage changes and logarithmic time differences in market capitalization coincide in absolute terms, but that our results are quite invariant to the method of calculation: regressing the average financial return on the average profit rate (Figures 3 and 11) and using the exact formula instead of the approximation, we obtain an intercept of -0.02 ± 0.03 with a p-value of 0.66 and a slope parameter of 1.29 ± 0.55 with a p-value of 0.02 for the long-lived firms, while a regression on the entire sample yields a constant term of -0.02 ± 0.02 with a p-value of 0.37 and a slope parameter of 1.57 ± 0.56 with a p-

value 0.01. Very similar results are also obtained in the volatility regressions. Since the numbers are almost identical to those reported in the paper, we would respectfully suggest keeping the definition of the variable unchanged.