Chapter 14

Conclusion

As the specific implications of our analysis for economic theory and for Ghanaian macroeconomic policy have been discussed in detail in the previous chapters, and summarized at the end thereof, the purpose of this concluding chapter is not to give yet another summary, but rather to provide a bird's-eye discussion of the overall methodological strategy we have chosen in this thesis.

Our methodology can be summarized as follows: upon formulating the research questions and reviewing the literature on structural adjustment in Ghana, we proceed to a rather in-depth discussion of alternative models used for assessing structural adjustment. While in itself such a literature review is a logical start for research project such as ours, an unusual characteristic for a policy-question thesis was the depth of comparative analysis of alternative schools of CGE modeling.

Then, after compiling the dataset, we used two different techniques to analyze the time series. For monthly data we used orthdox ARIMA-X time series econometrics, while for yearly time series we used a somewhat more unusual technique of fitting alternative functional forms to data for the first two thirds of the period (1990-1997) and judging them by their predictive ability in the last third (1998-2001).

Having done so, we have pulled together all the results of these data investigations in detailed discussions, as is typical for a policy-oriented thesis.

Looking back, we can ask ourselves whether that was in fact a well-chosen path towards our main goal, namely understanding of the Ghanaian economy's structure and behavior in a way that is helpful for evaluating and formulating economic policy. Specifically, did our delving into CGE theory merely serve to satisfy the author's intellectual curiosity, or was it an essential step towards the policy analysis at the end the thesis? Likewise, was the combination of different methods for different parts of the dataset a fruitful choice, or did the imprecision in the yearly time series analysis taint whatever illusory precision we might have gotten out of the econometrics on montly series?

Regarding the first question, we think that the detailed discussion of alternative CGE formulations was essential to all of the subsequent analysis, by providing a series of alternative hypotheses on all parts of the economy. It is true that CGE models cannot be falsified, and therefore are not a research tool but rather an elaborate story-telling medium — but that is the very reason for their value. For the best part of the last three decades CGE models have served as the repository of whatever macroeconomic/sectoral stories their authors thought were key to describing the economies they modeled; thus the CGE literature is a hoard of economic policy lore.

It is true that some of the stories CGE models tell are completely unfounded. However,

these stories were formulated in CGE form because they were well-respected in some economic policy circles (and quite possibly some stories, such as some implications of the full employment assumption, became influential in policy discussions solely because they were part of how CGE models traditionally behaved). If that is the case, such stories need to be carefully understood and refuted through data analysis, rather than ignored. On the other hand, some of the stories CGEs tell *are* true (such as the dominance of money supply in price level determination in the case of Ghana) and in such cases, CGE models deliver these stories already nicely converted to equation form, ready for empirical testing and use with better-designed modeling methods.

In other words, CGE models are incapable of providing any answers; but a good understanding of the CGE literature is quite useful for finding out the right questions.

Thus, detailed examination of CGE models gave us an overview of the alternative causal stories commonly used in describing developing economies such as Ghana. Was our approach to testing these alternative stories adequate to the task, and did it make the most of the data available?

We would like to argue that this was in fact the case. In the case of the yearly series, the number of data points (twelve per series) was just too small to apply any advanced statistical techniques, so optimization was the logical choice, particularly as it allowed us to test nonlinear functional forms as well. Likewise, constraining the estimation to the first two thirds of the data was in our view a useful if somewhat ad hoc method to verify predictive power of the different functional forms, lowering the danger of spurious fit. And we did in fact see that the best-fitting function in the estimation period was not necessarily the best predictor for the validation period. Thus, the chosen methodology allowed us to extract reasonably robust conclusions from comparatively poor data.

The situation was quite different in the case of the monthly series. These did not only have an ample number of data points for rigorous econometrics, but also boasted of a much lower measurement error. Thus, the exchange rate is readily observed with almost no error; the Consumer Price Index and broad money supply less so, but still with much more precision than, say, production of food crops. Thus, we were able to not only find out the causal drivers of these variables (and eliminate important causal driver candidates) but also specify the time profiles of their respective impacts[1]. Thus, due to better data quality, we were able to gain a much more thorough understanding of the Consumer Price Index and broad money supply behavior than was the case for real-side varaibles we investigated.

The remaining question then is, do different levels of precision of the two methods present a problem when one tries to combine their conclusions for policy analysis? Our answer is, briefly, no. Less briefly, this issue is exactly the reason why we think that an informed, careful discussion such as presented in the previous chapters is the optimal way of integrating the insights we have gained about individual behavioral hypotheses – as opposed to a formal model of the whole economy, financial, sectoral and all. The reason we think so is that an informed discussion is a much better instrument for managing widely different levels of uncertainty about different parts of the system.

In a formal model, all variables are *prima facie* created equal - all look like floating point real numbers. It is true that uncertainty in *parameters* can largely be addressed through systematic sensitivity analysis (though in the models of the complexity that we are considering here, the cost, both in terms of modeler effort and computational load, might well be prohibitive). However, systematic sensitivity analysis of a model with respect to alternative causal connections is almost

---

[1]The notable exception was the exchange rate behavior; however this is not very surprising as exchange rates are notorious for being hard to predict, just like investment functions.

impossible, and tends to result in the "anything could happen, really" kind of results. In a careful discussion, on the other hand, it is much easier to keep track of the different statements' different degrees of uncertainty, and to propagate these into conclusions of varying degrees of confidence.

This is not to say formal modeling is useless. When a model's methodology, causal formulations, and data are reasonably robust, it can give more precise insights than a verbal discussion, and sometimes produce surprises that lead to better understanding. The reader will have noticed that interspersed with analysis of the data in this thesis were chapters that laid foundations for a future formal model.

On the other hand, it is amazing how accurate intelligent back-of-the envelope estimates can be; and while substantial uncertainties remain both in the dataset (for example, we don't know sectoral producer prices) and in causal formulations (for example, exchange rate dynamics need more work), a verbal discussion remains the more versatile, and the more honest, method.