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In this short essay, I choose to discuss the two development papers. One is written by Raghabendra Chattopadhyay and Esther Duflo (CD), the other is written by Joseph Kaboski and Robert Townsend (KT). There are three parts in this essay. CD is discussed in part 1, KT in part 2 and I compare the two papers in part 3.

Part 1 (CD)

Summary

The main objective of this paper is to demonstrate the importance of the gender in the process of policy making. The paper argues that direct manipulation of the identity of the policymakers can have important effects on policy. Their argument is based on the empirical evidence showing that the political reservation for women in India affects the public goods provided and the women leaders tend to spend more on the infrastructure that is relevant to the needs of their own gender.

Identification Strategy

In this paper, the authors first present three stylized facts in the current political environment: (1) women are generally under represented in political parties; (2) there are more and more women joining in the group of policymakers; (3) there are over 30 countries in the world have fixed quotas for women in assemblies or on parties' candidates. These facts tell us the importance of studying the effect of gender on policymaking. In the literature, it has been proven that men and women have different political preferences. However, this fact alone cannot be used to guide policymakers. Therefore, it will be useful if one can show that the quotas for women in political positions can have a predicted influence in policymaking. This therefore becomes the problem to be solved in this paper.

There are two major things to be proved. First, the authors have to show that there is some way to make quotas for women in political positions that can have predicted influence in policy making. Second, the authors have to show that the predicted influence is due only to the gender. Because there might be some byproducts associated by imposing quotas for women which might contribute to influence policy making. The authors argue that the implement of the reservation policy of council positions in India can be used to prove both of the points.

The reservation policy is imposed in randomly selected villages, this largely solves the selection bias issue. It argues in the paper that the reservation policy which makes the women the village chief can result in more investment on public goods which are more relevant to women's preferences. To show this, the authors assume that the men's and women's revealed preferences can be represented by the requests and complaints made to the village council. If, for a certain public good, the investment made on this good is more in the village with reservation policy and this public good is revealed to be preferred by women, then one can argue that the reservation policy which makes the women the village chief can result in more investment on public goods which are more relevant to women's preferences, as desired. The authors prove in the paper that it is indeed the case with various specifications of the model. To prove the second major point, which is the predicted influence is due only to the gender. The author proposes several possible effects that are associated with this policy and might influence the public goods investment, and show that after controlling for these effects one can still observe the same result as before. Therefore, gender is the only factor that contributes to the difference in public investment.

Comments and Questions

In this section, I talk about the questions I find after reading this paper. (1) The empirical analysis is done based on small villages, which only contains 1000 something people. But in the conclusion, the authors generalize the result to all political environment, which means the reservation policy in large cities will have the same effect. I find this problematic, because as it says in the paper in each of the village, the chief is the only full-time appointed member in the entire council. In this case, once a woman is elected she will significantly increase the local women's bargaining power over different policies if she has the typical woman's preference. Because she will have more time spent on figuring out how to make the policy in favor of women compared to other council members, who might have to do some other jobs to make a living. But this is not necessarily true in large cities where all members with the power of determining a policy are full-time employers. (2) In proving the woman leader fulfills the needs of women, the author take the difference between the fraction of complaints from women and men about the same issue and the estimated coefficient shows that in village with reservation policy the public expenditure is made more on what the women complain

about. Since the leader of the village without the policy is male more than 90 percent of the time, it could be that the fact that the villages with policy spend more money on women's favorites are spending more in total compared to the villages without the policy. One way to explain this might be corruption when the village chief is male, who is probably more experienced. In the last section of the paper, the authors partially address the issue by controlling for the fact that women leaders are new comers, but I think it would be more convincing if the authors could include the average total spending in villages with and without policy in the table. (3) In proving the women leaders are not in general more likely to respond to complaints, the authors construct two variables. One is the difference between an indicator for whether issue i is brought by women in village j and an indicator for whether issue i is brought by men in village j; the other is the sum of these two indicators. From the statistics provided in the paper, I learn that it is very rare that some issue is only brought by men not women or by women not by men. In other words, this first variable will be taking on the value of 0 most of the time and the linear model estimated might be suffering from multicollinearity. Therefore, the insignificant coefficient estimates on these two variables might be due to multicollinearity. Because there is no description of how these two variables are constructed, it might not be the case as just described. However, I would suggest the authors to provide the breakdown of the complaints from men by villages with and without the policy. If the complaints from men are generally higher in villages with policy, then it might be an indicator that the women leaders are generally more responsive to complaints. This is a major point in this paper which should be studied and analyzed completely. Because if the additional expenditure in villages with policy is simply due to the fact that women are more responsive to complaints, then the result from this paper is invalid and the statistical model has to be modified quite a bit to identify the desired effect.

Part 2 (KT)

Summary

There are two main objectives of this paper. First, the authors demonstrate the usefulness of the structural model, especially the authors stress the advantages of using the structural modeling versus the conventional reduced form estimation procedure. Second, the authors conclude that

overall the Baht Village Fund program carried out by the Thai government is more costly than a simple direct transfer policy through a cost-benefit analysis based on the structural model derived in the paper.

Identification Strategy

To show that the microfinance strategy employed by the Thai government is more costly compared to an alternative direct transfer strategy, the authors need to provide a valid measure of the "benefit" and "cost" associated with these two different programs. This is done as follows. First, a structural model is constructed based on the observed facts from the available data. Next, the parameters of the model are estimated by minimizing some measure of distance between the observed values and the derived values from the model. Third, the authors claim a good fit of the constructed model by showing that the model can generate similar average values to the observed values given the estimated parameters. Forth, the authors incorporate the microfinance intervention in the model and generate the "simulated" data from the model. The authors show that the simulated data can be used to show the similar relations among the variables of interest to the result if the observed data is use. This procedure further justifies in some sense the correctness of the proposed model. Lastly, the authors carry out an experiment of switching the microfinance strategy to a simple direct transfer strategy controlling for the utility defined in the model.

Based on the utility measure(benefit) in the model, the authors conclude that the microfinance strategy is more costly than the alternative direct transfer strategy in this case. Along the way, the authors demonstrate that there are several estimates needed in getting the desired result which cannot be obtained in the conventional reduced form estimation procedure. Therefore, the advantages of using the structural modeling approach is shown.

Comments and Questions

One of the places that I find very interesting in this paper is the way it modifies the observed data before estimating the structural model. In doing so the estimation can be done with the variables that the model requires. I think this can be a very useful point when doing all estimation work in economics. However, I also found the following claims made in the paper are not as convincing as the other parts. (1) In comparing the microfinance strategy to the alternative direct transfer strategy, the

authors use the utility as a measure of the benefits for these two policies. The proposed model doesn't pass the overidentification test, so it is in some sense rejected by the real data. The authors nevertheless use the utility function as a valid measure on the ground that the generated average data from the model looks similar to the actual and the out-of-sample predictability also looks similar. My concern comes from the fact that the authors implicitly assume the proposed model work equally well if the alternative direct transfer strategy were used in reality instead of microfinancing. Because there seems to be lack of some criterion of to what extent the proposed model can be modified so that it generates reasonable results. (2) In the model the authors assume the 1 million Bacht credit will be available to all the households in the villages forever. But the authors provide no evidence in the paper to show this. For example, it is shown in the paper that there are around 15 percent default rate, but the authors do not supply any evidence that the Thai banks will continue to provide credits to the villages so that there will always be 1 millon Bacht to be lent in every year. (3) It mentions in the paper that the money in the program is initially not needed to be repaid by the government. This fact raises a concern that if the local villagers are in charge of lending out the loans, then how the earned interests and even the principal money is distributed between the local villagers and Thai banks might be important in this context. But this is not discussed in the paper. (4) Based on the estimated results from the model, we see the increase in consumption associated with newly available credits but not so with investment. The authors claim that the investment should increase on average. The model does not predict this is simply because the sample size is not large enough. I found this point is made without very sound evidence.

Part 3

Since these two papers are on completely different topics, I will compare the methodologies they use. In other words, I will compare the conventional reduced form estimation procedure to the structural modeling and estimation approach. I describe the estimation in CD as a reduced form estimation even though there is no explicit derivation from any structural model of the estimated equation in that paper. Because according to the paper written by Michael Keane, "Structural vs. atheoretic approaches to econometrics", any estimation equation in economics should

come from some sort of structural model or economic theory for the estimated coefficients to have any meaningful interpretation. Apparently, in CD, the estimated coefficients are assigned economic meanings. Therefore, the paper takes essentially a reduced form estimation procedure. I first summarize my understanding of the general steps one should take when applying these two different methods.

Reduced form estimation:
Step 1. Construct a theoretical model to describe and abstract an economy which highlights the variable of interest.
Step 2. Derive the testable hypothesis and estimation equation from the underlying theoretical model.
Step 3. Predict the coefficients of the estimation equation based on the proposed theory.
Step 4. Compare the predicted and estimated coefficients to discuss the validity of the proposed model and draw interesting positive or normative conclusions.

Structural approach:
Step 1. Construct a theoretical model to describe and abstract an economy which highlights the variable of interest.
Step 2. Estimate the parameters of the model such that derived variables from the model match the observed values as close as possible.
Step 3. Generate the variables of interest from the model and compare to the observed values to justify the validity of the model.
Step 4. Check the out-of-sample predictability of the estimated model to further justify the validity of the model. This step could be done by using reduced form estimation from the observed and simulated data.
Step 5. Draw interesting positive or normative conclusions based on estimated parameters or coefficients from any reduced form estimation.
Step 6. Experiment different policies by manipulating the model.

As shown above, it is generally more complicated to apply the structural approach compared to the reduced form estimation. It might look at the first glance that the structural approach is more general because it includes the reduced form estimation in step 4. However, it is not necessary that one of the two methods is strictly better. First, the biggest shortage of the structural approach might be that the demand for a much richer dataset compared to what is needed for reduced form estimation. Because of this, the reduced form estimation approach will be useful

when the observed data is not enough to apply the structural approach. Second, the seemingly superior feature of the structural approach in step 6 is not as justified as the previous steps. Because in doing so, one has to assume the modified model also describes some new situation which is not tested in any of the steps. Therefore, this gives one free room to go further on the predictability of the proposed model but at the same time this might also generate invalid conclusions if used without caution. Third, as noted in KT, the model constructed is actually based on some reduced form estimation results. Therefore, these two methods are not independent of one another.

In summary, after reading these two papers, I started appreciating the power of the structural approach. As argued in KT, the ability of estimating more parameters of the model itself might be a good reason to choose the structural approach over the reduced form estimation. However, I also learned that there are the numerous methods involved in actually carrying out the structural modeling and estimation approach. I feel it takes a comprehensive study of both of these two approaches to appropriately choose one over the other.