Reviewer’s report

Title: Moral hazard and double agents. The agency relationship in drug prescription in the Spanish National Health System.

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Reviewer: Atonu Rabbani

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Reviewer’s Comments on “Moral hazard and double agents. The agency relationship in drug prescription in the Spanish National Health System.”

Overall Assessment

In the article, the authors have correctly identified an issue which is central to the whole narrative around comprehensive coverage of health care cost through a mechanism so that the unanticipated and idiosyncratic burden from adverse health shock is shared by a pool of people through pre-payments. Government is regarded essential in this mechanism and most high-income countries the cost associated with health care, e.g. prescription drugs, is facilitated through public expenditure. However, containing the increasing cost of prescription drugs (among others) is important and physicians and their prescription practices are understood to be very important. In that regards, the authors address an important issue and finding the right incentive to make sure the appropriate prescription is written at the lowest cost is very essential for the efficiency of the health care system for any country.

However, it is important to realize that the choice of appropriate drugs by a physician for a specific patient is a complex interplay among many factors and I feel this is not addressed sufficiently in the paper. The physician take into consideration the disease and its severity along with factors that are specific to the patients (e.g. other health conditions, possible contraindications and interaction with other drugs that the person is taking along with other covariates such as age, sex etc.) and these factors are not observed by the researchers given the prescription data they are using for the study. I feel this is an important limitation of the present study.

Major Compulsory Revisions:

1. The identification of implications of an agency relationship between a patient and a doctor comes from exploiting the variation in the cost for a class of drugs between two distinct groups of patients (pensioners and non-pensioners) and whether full co-payment (FC) or reduced co-payments is associated with certain class of drugs. This scheme makes it difficult to interpret the apparent differences in the payments per unit of prescription per class.

The pensioners are very different than non-pensioners in terms of age, disease
patterns and its severity. These unobserved (and uncontrolled for) attributes will surely bias the estimates and the differences in the cost of acquiring drugs will certainly be correlated with these factors. However, given the authors are using pharmacy level data and (as noted) do not have information on the individual patients (see pp.6-7), the associated difference can very well be misinterpreted. Authors should address this issue.

2. It seems that the authors are applying a simplistic model for physician behavior and I am not even sure how exactly the agency paradigm invoked on page 4 (paragraph 1) are derived from the economic theory from reference [1]. The physician’s prescription behavior can best be derived in a set up where patients’ well being, institutional background (incentive structure incorporating monitoring and contractual arrangement based on observable outcomes, probably referred to as “variable incentives” on page 4, paragraph 1, line 1) and market incentives (marketing campaign targeted toward physicians, see comment 6 below). Thus variations in cost of a prescription belonging to a drug class will be result of interactions of all these factors which might be associated with the amount of co-payments a person (or a group of persons) will receive. This would certainly bias the results. While I do not expect that all aspects of the reality can be controlled in the data that the authors are using, this needed to be discussed liberally in the introduction and/or discussion sections.

Minor Essential Revisions:

3. It is not obvious if there are variations in co-payment level across regions for different drug classes. If there are such variations then authors should have some comments on why such variations exist across regions and control for the regional differences (through fixed effects, for example) in the econometric specifications.

4. The choice of drug classes should be justified. There are two drug classes in the paper. The psychotropic drugs are certainly are very discretionary in nature thus they may vary in terms of choice of drugs depending on other factors which may have nothing to do with “agency” motive of the physician. This will certainly bias the results.

5. Looking at the drug class level will certainly hide the fact the portfolio of drugs may be very different between pensioners and non-pensioners and also between classes associated different levels of co-payments. If possible, I would certainly like to see the cost differentials at the drug levels so that heterogeneity of use of drugs within a class is controlled for (beyond random effects at the class level).

6. Some of the institutional features of Spain are probably ignored in the paper. While it is generally understood that direct-to-consumer advertisement for drugs are not permitted in Spain, marketing strategies that target physicians may be permissible and that may change the prescription motive for a physicians. So if such strategies systemically address physicians treating pensioners, one can find the apparent difference in cost for these two groups (i.e. pensioners vs. non-pensioners).
7. Also, since the variations come from variation across different “autonomous regions” one should also address the possibility of the role that migration may play in the observed variations in cost. The “pensioners” may decide to live in the regions where the copayment policies are more liberal and accommodating to their needs. If the sicker pensioners systematically choose based on these regional policies then one can find the apparent variations in the data which again may not have anything to do with the agency motive of doctors.

8. One implication of the finding seems to be switching from FC to RC will make doctors choosing drugs such that patients as well as the system pay less. If so, at least for classes of drugs included in the analysis authors should think about showing the aggregate savings based of some back-of-the-envelope calculation. This will certainly show how important these findings are for aggregate drug spending.

9. While authors mentioned the variable incentives that the physicians receive (p.5), this idea was not followed up later in the analysis or discussion as this may play an important role whether they will act as an agent of the “system”.

10. The statistical model (M1, see page 8 for the model specification and page 20, table 3 for the estimated values) suggest that the mean value for class-region observations with FC is higher compared to the RC. Firstly, it is not obvious what the baseline observation is and how to interpret that. Secondly, authors did not discuss if there were a systematic association between drug classes and copayment status which may lead to higher prescription cost. Thirdly, the level effects may persist even in models M2 and M3 (see page 8 for the models). But that indicator variable was included in the model. Authors should justify this.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

'I declare that I have no competing interests'