Referee Report on
"Aging, Private Health Expenditure and Environmental Quality"
Report based on an Author Blind Manuscript

The author(s) pose a problem whereby it is supposed that agents in different generations have a fundamental conflict when making their consumption-saving decision: young agents are supposed to be pro environmental quality while old agents are presumed to prefer health care services. This conflict is posed in a formal overlapping generations model where there are young agents and old agents — lifetimes are at most two periods. The model has some vaguely specified notion of premature death in the second period (more about this point below). Otherwise, the model is quite conventional. Abatement of pollution costs current resources as does health care. The authors claim their main result runs as follows: There is increasing support for private health expenditures in an aging economy leading to a higher level of capital accumulation and also leading to a higher level of environmental quality provided the "maintenance efforts" are larger than consumption externalities.

Some Issues

This paper has several problems. First, there is no convincing argument from the authors that the imagined conflict exists except in their paper. The population structure of the model does NOT include population growth. It is absolutely standard — one old agent and one young agent coexist at each time. The authors want readers to believe there is an uncertain lifetime once agents hit old age. For modeling purposes the authors assume a positive probability the young agent doesn't actually survive to start old age. A survival probality is used much like a discount factor in the formal model machinery. So, here's my first question: what is the sense of market clearing when the young agent doesn't actually consume in the second period? Where do the goods go? To whom are they delivered? My first recommendation is to get rid of this uncertain lifetime setup altogether and simply replace p by a discount factor and use time preference instead.

There are no firms in this model despite what the authors assert. There are no transactions costs — there is just a production sector realizing zero profits in equilibrium.

Re. the definition of equilibrium in Section 3.3 — it is asserted that only old agents maximize their utility function. This is, of course, absurd. Likely a typo?

There is a curious statement at the bottom of a page with italiced *Private health expenditure support:* followed by some computations on the next page. Those computations, which require proving the determinant of D is negative, yield the statement:

> "In aging population, the positive effects of supporting healthcare on capital accumulation overcome the negative ones."

This statment makes no sense whatsoever. First, it is, at best, about a steady state comparative statics exercise. Presumably, the determinantal condition (derived earlier in section 3.4) is some sort of stability condition for a linear approximation model for the steady state as best I can tell from APPENDIX B. This condition is then used to sign steady state comparative statics results. However, it is absolutely unclear to this reader how the quoted result follows. First, where is the aging population in a steady state? The problem is, at this point, effectively a static one. Indeed, if we take the story about uncertain lifetimes seriously the population is declining, not aging! In any event, health care is not a

state variable as imagined in the italicized statement, but an endogenous variable chosen via the agents optimization problem.

Some arguments in the appendices turn on nonnegativity of E, environmental quality. I don't think that log utility is the issue — it is a constraint that is no different from requiring a nonnegative capital stock at each time independently of the functional form of the utility function.

Final Thoughts.

This paper is poorly crafted and edited. Proofs and derivations should be organized as formal theorems in the appendices so that readers can readily see what is the main result of such arguments. A language editor should be employed to go through the paper's exposition tighten the presentation (e.g. Section 3.1 shows first-order conditions without saying these are first-order conditions until afterwards). Incidently, the word *efficient* appears in the following paragraph of Section 3.1 line 6 from the bottom. What does that mean IN THIS PAPER?

There are no page numbers. Fix this.

Steady state comparisons are the only results — comparative dynamics are not presented that track how dynamically determined equilibrium paths change due to the impact of parameter changes. The paper's stated results mislead readers by not be sufficiently explicit that all claims apply to steady states only.

The notion that this model can address conflicts in aging societies stretches credulity. In fact, one could argue the results, such as they are, only follow since there is NO presumption that agents are altruistic. It seems to me that supposing agents are "myopic" with the time frame of a two-period lifetime suggests that generations really do not care much about their successors. For example, suppose each time period is measured as 25 years or so. Are we really that myopic? IF so, what is the evidence? So, if the authors believe that agents are myopic and not altruistic towards future generations, then, and only then, does their stated problem even seem interesting to me. Otherwise, it seems odd that a short-term issue such as health maintenance, which seems to have no lasting effect from one generation to the next, should be balanced against a long-term state variable such as environmental quality which tends to persist or decay over time.

I cannot recommend publication. The paper's conclusions (and the formal model supporting them) are neither presented well nor clearly developed. Any chance of publication would need the authors to undertake a major overhaul.