This paper makes an interesting contribution to the literature on the effect of technological change on cultural norms. Concretely, the paper argues that changes in contraceptive technology are the main driving force behind the large increase in (attitudes towards) premarital sex for teenage girls over the course of the 20th century. The paper starts by presenting many interesting facts, then develops a search model of partner choice and sexual behavior, calibrates the model, and finally shows how the calibrated model is capable of explaining the observed changes over time. In an extension, the increased availability of abortions and the arrival of HIV is added, and it is shown that the extended model does even better in explaining the data.

The paper is interesting, thought-provoking, and the story seems plausible. The data (in particular Section 8) is very interesting and not well-known to most economists. The paper is well-written and the ideas are presented in a clear fashion. The hypothesis put forth in the paper provides a refreshing contrast to the recent economic literature that emphasizes the importance of culture for economic outcomes (e.g. work by Raquel Fernandez, Paola Giuliani, and others) by investigating the opposite causality for a very concrete type of cultural change.

Some particularly interesting findings are the following:

i. The paper can explain the INCREASE in teenage pregnancy that happened in response to the improved birth control technology. The increase seems quite counter-intuitive (but is there in the data), hence this is a success of the model.

ii. The data on attitudes towards premarital sex (figure 2) and in particular how the attitudes are lagging behind the actual behavior (figure 1).

iii. The finding that the invention of the pill played almost no role in the evolution of sexual mores (Figure 10).

iv. The importance of availability of (legal) abortions in explaining the time series of teenage sexual behavior (Figure 12).

One comment I have is that norms about sexual behavior are often gender specific, something that the current model does not address. The authors are well aware that most sexual mores affected women/girls more than men and they present data on attitudes and number of sexual partners for females only. The model is (mostly) calibrated to data from female teenagers. Given that premarital sex and pregnancy obviously involve males, this seems a bit strange. One way of introducing males explicitly would be to allow the (distribution of) costs of premarital sex to differ by gender and to calibrate the model to data from both genders. However, while probably a worthwhile exercise, such an extension might go beyond the scope of this paper. Nevertheless, the authors should at least briefly discuss the issue (probably within the introduction): Why is a model with full gender symmetry the natural starting point and how might results change if males were explicitly taken into account?
“Social Change (From a Macroeconomic Perspective)”

A referee report

Summary of the paper

This paper defines social change as the change over time of the fraction of the population engaging in a specific behavior. The paper argues that technological change can lead to social change. The specific episode studied in the paper is the evolution of contraception and the change in pre-marital sex habits starting from 1900.

Comments

I think that this work studies an important and interesting question, but I also believe that there is room to improve the paper along several dimensions. The paper should be refocused, some parts should be eliminated, some others should be studied more in depth, and some aspects of the data and the fit of the model should be better discussed. Here are some more detailed comments.

1. The paper oversells the social change part. Defining social change as the change of a fraction of people engaging in a certain activity and linking it to technological progress is hardly a shattering insight. Also, it is not clear why one might need a complicated search model to analyze this issue.

The quantitative analysis of the particular historical episode considered (the improvement in contraception and pre-marital sexual mores) is the interesting and important contribution of this paper. The paper acknowledges that the main contribution is a quantitative one at some point, but this is buried in much rhetoric.
I believe that discussing the definition of social change used is fine, but the focus of the paper should be on the quantitative application, and the rhetoric should be pared down considerably. I would start from the title, and then throughout the whole paper. A better title would be “Understanding Social Change: the Rise of Premarital Sex During the 20th Century”. (I don’t particularly care that the current title is one used by a famous sociologist discussing social change in a book.)

Another example is paragraph 1.1. I think that the discussion about attitudes about sex should be dropped. The data about attitudes is never used in the paper, and the model has something to say about behaviors, individual and aggregate, but not about attitudes.

2. The paper is too long and covers material that is unimportant to the key question. All of the parts which are not necessary and important to understand the key quantitative question raised in this paper should be eliminated or drastically shortened. Examples are:

- Cut the quote at the beginning of the paper (page 1).
- Greatly streamline the part discussing the contributions of other papers analyzing social change.
- Reduce the part on the history of technological progress in contraception which is not used in the calibration. I am glad that the authors studied the question in such depth, but I not believe that the reader needs to spend time reading all of the details about douching, about the fact that Knowlton was prosecuted for obscenity, and about the study on the fertility cycle of frogs (page 26).
- Page 30 and 31. Drop the discussion about the analogy between the choice of one method of contraception over another, and aggregate total factor productivity.

Drop footnote 17 at page 36. Using the fact that the speed of adjustment of neoclassical growth model is fast does not, in any way, provide any support for the validity of the speed of adjustment of this model. First, this model and its dynamics should be supported by relevant data. Second, the neoclassical growth
model does display interesting dynamics at the business cycle frequencies, while this paper (and the particular question addressed in it) has nothing interesting to say about quarterly movements.

- Figure 5 is also not very interesting, nor insightful, and should be dropped. We don’t even need two social classes to discuss the specific definition of social change adopted in this paper.

- Figure 7 is not that insightful and should be dropped. This kind of behavior is obvious from the assumption that people can have different sex drives in a couple, and from other assumptions previously discussed in the paper.

- Section 11 on the proposed extension of the frequency of sex should be dropped. This detracts from the main focus of this paper and serves no useful purpose here. Maybe the authors could expand it and transform it into a separate paper which studies the transmission of sexually transmitted diseases.

3. The paper does not seem to have much interesting dynamics. The only reason why today’s state matters for tomorrow (outside of a steady state) is that there are some “mixed” couples in which one of the partners wants to have sex, while the other one does not.

This element crucially depends on what is assumed about the utility of being single for this period, and the utility of being together in a match but not having sex this period (two parameters exogenously assumed). The authors show that these parameters do not matter for the steady states. However, they could matter for the speed of adjustment to the steady state. If they don’t, this should be made explicit: all that matters is a sequence of steady states, there is no relevant transition taking place.

4. Data work. Although the authors have done a good job with the data, some gray areas remain.

- Apparently the authors were not able to find data on the fraction of people using different types of contraception in 1900, and used the same fractions they have data for in 1960-1964. Why is this a good assumption? This crucially determines the smooth, slow drop in the resulting series of computed risk of pregnancy (Figure
6, page 31), which in turns is key in determining the fit of the model over the first 60 years of the period (out of a total of 100 years in total which are studied).

- Abortion: according to figure 11 (page 44), in the mid seventies about 50% of the pregnancies ended up in abortion. What data set do these numbers come from? To what women’s age group do they refer to?
- HIV: figure 11 (page 44), where do these data come from? Do they refer to the relevant population in this paper, the one of teenagers? My impression is that early on almost all of the contagion was driven by sharing needles, while heterosexual intercourse was a trivial factor.

5. Fit of the model and what we really learn from this paper

The paper does an interesting counterfactual: what would have happened without the pill? The conclusion is, not much. The paper thus concludes that the big driving force in the sexual revolution was the improvement in all forms of contraception.

This conclusion about the pill is interesting, but hardly surprising, since the data that are the input into the model show that the pill was about as effective as alternative methods, and was not widely used by teenagers anyway.

Importantly, the fit of the model without abortion and HIV is rather poor (see figure 8, page 37). The end points (the steady states) are matched by construction, and (as I have already mentioned previously) I have my doubts about the fit of the first 60 years, given that the authors have no data on the fractions of teenagers using various contraception methods between 1900 and 1960. The model without abortion and HIV is significantly off in the timing of the changes in premarital sex behavior after 1960. I thus don’t think that this version of the model should used to do counterfactuals.

The model with abortion and HIV does fit the data much better, though. This raises the question of what is important to fit the data well. Based on what is currently in the paper, it seems that the introduction of abortion is what drove the sexual revolution.
I think that the authors should first do a better job of explaining which features of the cost of contraception, abortion and HIV help the model fit the data well. For example a “build up exercise” first using figure 8, then a version of the model with abortion, and finally a version of the model with HIV costs. These three figures could then be used to discuss which are the important features driving what sex behaviors over the various subperiods.

Then, the complete model, the one which fits better, could be used for counterfactual analysis, such as eliminating the discovery of the pill.

The framework does a poor job of fitting teenage pregnancies (figure 9, page 38). The paper is off by a factor of two. This matters because it should come out mechanically from the cost of contraception and sexual behavior (the focus of this paper). The discussion about frequency of sexual intercourse in the last section is probably a defense of the model in this regard, a footnote would be sufficient for that.

6. Modeling choices and justifications

- It’s not clear what “matching” buys us in the context of the specific question asked in this paper, especially since the paper misses of the number of sexual partners anyway.

- The assumption that the adult’s world is, by construction, the same as the adolescent world is not well justified (simplicity) and it is not clear that this is an innocuous assumption. What happens if we simply assumes a constant value of exiting teenage years (normalized to 0)? Discussing this would be helpful in understanding the role of any hidden assumption.

- Paragraph 10.2.1: the comparison between the standard search model and the two-classes framework in this paper. The authors tried to calibrate the model with undirected search, in which search is less efficient and claim that it is not possible to use this version of the model to match both the initial and final steady state. This only raises my doubts about the role of search in this model. I would like the authors to clarify this aspect of the model. What happens if there is a single class and little or no search frictions? Does the failure depend on assumptions depend on $u$ and $w$ (utilities from being single and in an abstinent relationship), which
become very important under indirect search since they affect the
types of matches that are formed?

7. The notation is poor, to say the least. It took me several tries to keep
track of what is what. There must be a better way to keep track, for
example using subscripts and superscripts.