Comment

Catherine Rodriguez Orgales: In their paper, Llach, Adrogué, and Gigaglia study the important question of the long-run impact that extending school hours can have on students in several aspects of their adult life. Specifically, the authors use a policy experiment from Argentina, carried out in the late sixties and early seventies, which doubled the hours spent by students in public primary schools. This policy increased school day length from four to eight hours (including two hours for lunch) per day. To carry out the analysis, the authors designed and applied a random survey to 380 students who graduated from primary schools in 1977, seven years after the policy was generalized. Aside from collecting information on the socioeconomic characteristics of the individuals' parents while they were in school and on their school experiences, the authors also asked participants about adult outcomes, such as education attained, job status, income, and marriage.

The results can be divided into two sets, depending on the methodology used by the authors. Under an OLS approach they find mixed results. Even though increasing primary school hours increases the probability of concluding the second tertiary study, it has a negative impact on both high school grade repetition and conclusion of postgraduate studies. The authors find no other effect on any of the other outcome variables of interest. The results based on propensity score matching are quite different. Increasing primary school instruction time increases by 21 percent the probability that a student finishes high school as well as students' access to and timely conclusion of a second tertiary study. Dividing the sample according to socioeconomic status, the authors argue that the former effect is driven by the effect the program had on low-income students. As in the OLS results, it appears that such policy has a negative effect on the conclusion of postgraduate studies.

I believe this paper is a valuable contribution in many ways. First, and probably most important, the ideal school day length is an important and relevant question that has been in the minds of policymakers and education

researchers in different countries and at various times. However, no consensus has yet been reached, and we still do not know what the appropriate school day length is.[1]

The second contribution of this paper is its focus on Latin America. With the exception of Bellei (2009), most of the previous research, such as Carroll (1963), Card and Krueger (1992), Marcotte (2007), and Pischke (2007), focused on the effect of school time on students from developed countries. The low quality of education attained by Latin American students, evident in the international tests they have taken, makes it imperative to find new mechanisms to help improve this crucial aspect of these education systems. Though the studies cited in this paper provide mixed evidence regarding the effect of school length on quality of education, the Bellei study actually finds a small but positive effect of increased school hours in Chile. Given that only this study has concentrated on a Latin American country, additional evidence on the subject is always welcome.

The third contribution of the present paper is its analysis of the long-term impact of school day length. Specifically, Llach and his coworkers evaluate the impact of longer primary school days on adult outcome variables such as high school and college graduation, wages, and job turnover, among others. This set of variables clearly has been underexamined worldwide, and even though Pischke (2007) does analyze some of them, his research is based on a policy implemented in a developed country. To the best of my knowledge, this paper is the first to focus on the long-run impacts of school day length in a developing country.

Finally, I believe that the fourth contribution of this paper is the construction of the data set, the sample design, and questionnaire elaboration used to empirically evaluate this policy experiment. We definitely need more of these initiatives in the study of development topics in Latin America.

Nonetheless, I have a few comments on the results found, their comparisons with other programs, and their external validity. My first comment is related to the first stage of the matching estimation. Given that this is the authors' preferred methodology, it would have been important to know which are the variables that the authors believe determine the probability of attending a double-shift school and whether they are indeed good predictors of the latter. The validity of the matching results depends crucially on the relevance of

1. For example, according to the Organization for Economic Cooperation and Development (2008), the difference between the countries with the lowest and highest number of total intended hours is more than 2,500 hours a year.

the control variables in the first stage, but the reader has no information about this. It would also have been valuable to know the number of observations that are left in each of the estimations, especially when individuals are divided according to socioeconomic groups. This is important because for some questions, only thirteen people gave an answer in the whole survey, and hence, after dividing the sample according to socioeconomic status and matching individuals, I seriously doubt any true conclusion can be obtained from such a small sample size.

Second, it would have been very enriching to have more details about the policy and its implementation. How were the participating schools chosen? The authors mention that the schools were evenly distributed across school districts; it must be stressed, however, that this does not imply randomness in implementation per se.[2] Actually, the fact that the pretreatment variables of the control and treatment groups differ significantly provides evidence in favor of the hypothesis that the schools were not randomly chosen. Another question that emerges is how the authorities managed to change from a single- to a double-shift program in the participating schools? Was additional infrastructure needed? Were additional teachers hired? If they were, did they have the same level of education and experience as the original teachers did? I believe that these are all crucial details that help the reader get an idea of the impact that the program may or may not have had. Although the authors briefly mention a complementary policy of extending the working period of teachers, many of these doubts remain. For instance, if more but less-prepared teachers were hired and the curriculum did not change, as is suggested by the authors, it is difficult to expect an improvement in the quality of education received by the students. Instead, given that the students had less idle time, one could have expected to observe a reduction in smoking, drug consumption, or criminal involvement, such as that found by Jacob and Lefgren (2003). All the aforementioned could be considered interesting outcome variables in future research. However, without a more careful and detailed description of the policy and its implementation, it is difficult for the reader to know exactly which results are interesting and what to expect about them.

Moreover, a detailed description and understanding of the program's implementation could also give the authors a possible instrument to explain the participation in double-shift schools, one that would be useful for future research.

2. For instance, the fact that 50 percent of the schools in a given district were chosen clearly does not guarantee that they were chosen randomly. For example, the authorities could have chosen the 50 percent poorest or richest schools.

There could be many possible candidates, such as whether siblings or other relatives were already attending the school or the distance from students' homes to the participating schools. Alternatively, from appendix A, it can be observed that even though the share of schools where the policy was implemented was relatively constant, the share of 1980 primary enrollment was not. This could, in principle, serve as an instrument if Argentinean households do not choose neighborhoods according to the type of schools available for their children. Future research on the topic could focus on this alternative estimation methodology given that the main assumption under both OLS and matching estimators is that self-selection of individuals is only based on observable characteristics. This is, of course, a strong assumption, especially if the group of control variables is not complete and detailed.

Third, it is important to understand the magnitude of the effects found by the authors and any possible bias from the survey methodology. Specifically, the authors mention that the survey was conducted among students who graduated from primary school in 1977. These are therefore not necessarily those who started school in 1971, when the authors claim the policy was generally applied. The choice of this cohort could therefore present more problems of self-selection than the authors actually acknowledge and hence could also explain part of the results found. Similarly, of the intended interviews, how many were successful and how many were not? These are all important details that could help in understanding the results found. For instance, I would have expected that a clear effect of the policy, if it was indeed randomly applied, would have been a reduction in repetition rate. This is because even if the policy did not change the curriculum, students would have spent more time in school, and their teachers would have had more opportunity to reinforce the topics learned or help students address difficulties. The results, however, do not support this hypothesis. The question that then emerges is whether this is so because some of the sampled individuals were part of the self-selected groups that got into the treated schools before 1971 and thereby naturally increased the repetition rate for the treatment group.

Finally, when evaluating the impact of this policy change, the reader must exercise caution when analyzing the results. First, the authors claim that the most important effect is the 21 percent increase in the rate of secondary school graduation. This is a significant effect, and one I have not seen before in other supply-side education interventions. I wonder how this effect is compared with the effects obtained through other education programs implemented in Argentina. I have a similar comment for their estimate of a secondary school graduation expenditure elasticity of 0.5. Although further details are needed

on how such a figure is obtained, I believe this is a high number.[3] Finally, the authors conclude that given that no effects on tertiary graduation, wages, employment, and knowledge of a second language were found, the content and learning quality of the double-shift schools were not good. I think this is a strong assumption, and one that cannot be directly obtained from such analysis. First, if no effect on quality was achieved, through what channel do the authors attribute the 21 percent increase in the high school graduation rate? Second, the fact that no effect on income and employment is obtained could be consistent with a story in which schooling is simply a signal for the employer, and what is taken into account are the grades approved and not their quality, as Pischke (2007) suggests. Hence such a conclusion should be tested through other channels, such as results in exams or evaluation of the education and experience of participating teachers.

As I commented earlier, I believe the line of research taken by these authors is very interesting and relevant. I hope the authors can continue with it and that future work can shed more light on the effect of this very important but understudied policy variable in Latin America.

3. For instance, when estimating the increase in educational expenditures, do they include only public spending in education, or does it include private spending too?

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