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Explaining Charter School Effectiveness
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The authors attempt to determine the effect of attending a charter school on a student's educational attainment (as measured by test scores). They observe data on students in public middle and high schools in both urban and non-urban settings in the state of Massachusetts.

The authors point out that attending a charter school is only a partial random variable. When charter schools are over-subscribed, they resort to a random lottery to determine who is admitted. However, if a student (or a student's family) does not believe there will be a significant improvement in the educational quality they receive if they switch from a 'regular' public school to a charter school, then they will not enter the lottery. Additionally, even if they do enter the lottery and win entry into the charter school, they can still reject the offer. Therefore, whether a student attends a charter school or a regular school is partially determined by their original test scores (which is correlated with the educational quality of the regular school), and is therefore endogenous. To correct for this, the authors run a reduced form regression on the variable of interest, years in a charter school, and use the lottery results as an instrument. Since the lottery result is a random variable (excluding children with siblings in charter schools and out-of-town students both of which have biased lottery results), it cannot be correlated with the dependent variable, test scores. It is also obviously correlated with the endogenous variable, years in charter schools, since a positive lottery result is required to enroll in the charter school. The authors therefore argue that it makes for a good instrument on the endogenous independent variable.

In addition to determining the effectiveness of charter schools in general, the authors also attempt to isolate whether or not there is a difference between urban charter schools and non-urban charter schools. This is done simply by adding in two dummy variables, one for years in urban charter schools and one for years in non-urban charter schools. They find that attending a charter school in an urban area has a much greater effect on increasing test scores than attending a charter school in a non-urban area.

Next, they set up a standard difference-in-differences (DiD) model, to isolate the difference in the treatment effect between urban and non-urban charter schools. The DiD model is calculated by taking the difference in test scores between urban and non-urban treated students (charter school attendees) and subtracting the difference in test scores between urban and non-urban non-treated students. They find that urban charter schools significantly increase test scores compared to urban non-charter schools. Conversely, treatment for non-urban charter schools actually decreases testing scores, compared with non-urban, non-charter schools.

One of the reasons for this finding could be demographics, and student characteristics were not controlled for in the DiD equation. To account for differences in demographics, the authors employ a Blinder- Oaxaca decomposition, which decomposes the difference between urban and non-urban charter school treatment effects into two parts: the between portion, which measures the treatment effect differences based on the different demographics of students in urban and non-urban schools, and the within portion, which captures the treatment effect due to differences in effects within the demographic groups. Here they find major differences between urban and non-urban charter settings. While the between and within group differences in demographics are both relatively equal in explaining the treatment effect in urban settings, the within differences are much more important in explaining the treatment effect in non-urban settings.

To discover why urban charter schools have a higher treatment effect than non-urban charter schools, the authors look at the individual characteristics of the schools. Not surprisingly, they find that increased time in the classroom to be positive and significant in explaining performance- the longer children learn for, the better they perform. Even when controlling for school characteristics, they do find that urban schools still outperform non-urban schools in some of their regressions. To further isolate the reasons why urban schools are more successful, they look at disciplinary statistics. The authors state "the results for truancy suggest that attendance at an urban charter high school reduces days of unauthorized absence". It is not clear if this is true. Are they more likely to be punished because there are greater discipline levels at these schools (as described by the *No Excuses* policy)? Or is it because they are doing more things worthy of punishment? I believe it is very important to determine what is causing the increase in punishments and I'm not convinced by the authors' explanation.

It appears to me like there may be a different type of selection problem that can be causing the differences in the treatment effect that the authors do not test for. It is possible that non-urban public schools are of sufficient quality that most students with average or above average "ability" will be able to succeed if they attend them. Therefore, the only students that do not succeed (as measured by low test scores) are those with very low ability, who may not succeed at any public school. Thus, regardless of which students switch from a non-urban public school to a non-urban charter school, there will be no increase in performance. Conversely, public urban schools are not of sufficient quality for average and above average students to succeed. Therefore, when a random sample of students switch from an urban public school to an urban charter school, those with average and above average ability will switch from not succeeding to succeeding. If this explanation is true, the demographics of the urban students and non-urban students need not be tested, as the populations can be identical for this pattern to be causing the effects the authors observe. This could also be tested using a relatively straightforward DiD procedure.

Matching, Sorting and Wages

Jeremy Lise, Costas Meghir, Jean-Marc Robin

In this paper, the authors set up a search model with heterogeneous agents and productivity shocks to determine how labor market regulations affect the equilibrium. The purpose of the model is to see how various variables, such as the equilibrium wage, profits, and employment, are affected by common U.S. labor market policies, including experience rated unemployment insurance, a minimum wage, and severance pay. They implement a few improvements over standard search models to make their model a bit more realistic and also allow for the possibility that regulations can be welfare increasing, including the addition of complementarities between the workers' human capital and the jobs' productivity parameter.

In the first part of the paper, the authors describe situations where an agent and a job will be matched, when they will become un-matched, and how wages are determined. Agents and open jobs randomly find each other, and a match occurs when a surplus would be created through the pairing. The surplus is a function of the agent's human capital, and the job's productivity parameter. When a match occurs, the wage is determined by a Nash Bargaining parameter, which the authors determine using their labor market data. Once a match is formed, it will persist at the current wage until a productivity shock occurs, or until the agent receives another offer for an unfilled job. If the former occurs, wages are renegotiated if a surplus is still possible. If a surplus is not possible, the agent becomes unemployed and the job becomes either vacant or closes. If the latter occurs, the agent can renegotiate their current wage with the job, or they can accept the new job if they can receive a wage higher than the highest possible wage their current job can offer them. The authors then use the surplus function, the total number of jobs, and the distribution of matches to define the equilibrium.

After defining the model, the authors attempt to estimate its parameters using labor force data on white males from 1979-2002 (I found it bizarre that the authors refer to the agents using female pronouns throughout the paper, but then remove women from their dataset without any explanation), and a CES production function. They use simulated method of moments to estimate the parameter values. I do not understand much of what is going on with regard to the actual estimation, and so I can't critically analyze the model's results. The authors state that the model they created fits very well to the actual data. They state that their model replicates, amongst other things, the increase in wages from switching jobs, and the lower wage growth for staying at your current job. They also conclude that high skills workers are fundamentally different from low skill workers in the labor market. Higher skilled workers will sort themselves into the most productive jobs, where the surplus is greatest for both the worker (via the wage) and the job (via profit). The same is not true for lower skilled workers, where there is no evidence of sorting. The authors conclude that labor market regulations, which protect employment income or set a minimum wage, tend to decrease efficiency by a small amount. Additionally, even a low minimum wage can increase unemployment significantly, mostly affecting the lowest quantile of skilled workers. It also works as a conduit for transferring wealth from chronically unemployed, low-skill workers, (who would not be unemployed if not for the minimum wage) to workers in the skill quantile just above them.

Comparing Estimation Techniques

When choosing what estimation technique to use, an economist has to consider many things: what data is available, how developed is the underlying theory, who is the intended audience, and how much will a more complex estimation procedure improve the quality of the results, to name just a few. In the Lise et al. paper, the authors took advantage of a very well developed search model literature. The model which they built, and subsequently used for estimation, was built upon a fairly well accepted model in the macro-labor literature, which has frequently been tweaked and improved upon by different researchers. For this reason, it seems natural to attempt to estimate the model structurally. Conversely, I do not imagine that there is such a robust theoretical literature describing the effectiveness of different schools based on characteristics of student demographics and alternative school choices. This left Angrist et al. with little choice but to use standard least squares and quasi-experimental estimation techniques to test their hypotheses.

However, the existence of a robust theoretical model should not automatically lead a researcher to use a structural model. Although I do not have much experience with structural models, I don't think it would be controversial to say that they are typically much more difficult to understand, and the procedures used to estimate them are understood by far fewer economists than least squares or maximum likelihood techniques, which are covered in most undergraduate or master's level economics programs. Science is a collaborative sport, where new research is layered upon the past work of others. Because of this, a researcher should consider how much more we can learn from a structural model, over using a least squares approach, and if that justifies the added complexity.

With that being said, my personal opinion is that the techniques that each of the authors used were appropriate for their respective topics. The least squares regression techniques employed by Angrist et al. seemed effective in explaining why urban charter schools are more effective than non-urban charter schools. The quasi-experiments they employed via difference-in-difference regressions are useful at parsing causality from correlations when there is unobserved heterogeneity in the sample. Likewise, Lise et al. were attempting to test a theoretical search model using available labor market data. Many macro researchers would typically resort to calibration to do so. It is my opinion that calibration can be useful in determining the direction of an effect, but I don't think it can accurately determine the magnitude or statistical significance of an effect within any reasonable degree of certainty. Structural estimation, which appears to me to be a fusion between least squares and calibration, seems to be a much more accurate technique for estimating these types of models. As I mentioned, the added complexity seems to be the only drawback to using structural estimation, but with complexity should come greater accuracy and explanatory power.

Finally, I believe that anytime you can get a theoretical model to agree with actual real world data, that is a powerful result. While linear regressions do essentially just that, by minimizing the difference between a predicted value and actual data, it is not normally the case that we believe that the regression model describes how the real world actually works (with the Mincer equation being one notable exception). Structural models, on the other hand, are actual attempts to describe what the model which is actually generating the data looks like, rather than simply showing statistical relationships. For this reason, ceteris paribus (which is usually not the case), structural models should be the preferred estimation technique.