Applied Economics
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1. Suppose you are interested in studying if holding political power for longer (*Total Tenure*) increases the probability of establishing a political dynasty (*Post-Relatives*).

a) Explain why the following OLS regression does not allow by itself to identify a causal relationship: $Post-Relatives_i = \alpha + \gamma\ Total\ Tenure_i + \beta X_i + \varepsilon_i$ (where *i* refers to legislator *i*, and X is the set of legislators' characteristics).

Total Tenure may be endogenous. For example, family characteristics may explain both why the first family member had political power, and why the future relatives also have political power.

b) Dal Bó, Dal Bó and Snyder ("Political Dynasties", RESTUD 2009) use a Regression Discontinuity approach in which the outcome of close election is an instrument for tenure length. Please explain potential limitations and problems with using this identification strategy in that application.

The paper by Dal Bó, Dal Bó, and Snyder (2009) uses a regression discontinuity design that relies on the outcome of close elections as an instrument for tenure length. This regression discontinuity approach identifies a causal effect under the assumption that winners and losers of close elections have similar (observable and unobservable) characteristics. Identification would be jeopardized, however, if winning a close election depends on personal characteristics that are also correlated with having future relatives in Congress. For instance, selection would arise if corrupt politicians are more likely to win a close election and they are also more likely to use their influence to push relatives into office.

Besides, the regression discontinuity design estimates a local treatment effect, in this case a treatment effect for a small subpopulation of the sample: those politicians that barely win or lose an election. It may be argued that these politicians are likely to be of lower quality than the average politicians.

c) Explain how you can take advantage of the natural experiment of the random allocation of terms in the Argentine Congress (see "Term Length and the Effort of Politicians", Dal Bó and Rossi (RESTUD 2011)) to identify the causal relationship between political power and the probability of having future relatives with political power.

The random assignment of terms in the foundational Congress of 1983 provides a source of exogenous variation for tenure length in which the instrument (the 2 or 4 years in the random allocation) is likely to satisfy the exclusion restriction.

2. Here you can find Table 3 (First stage by Birth Cohort - Dependent variable: Conscription) and Table 4 (Estimated impact of conscription on crime rates**)** from "Conscription and Crime: Evidence from the Argentine Draft Lottery" (Galiani, Rossi y Schargrodsky, AEJ 2011).

Table 3

Cohort	1958–1962 (1)
Draft Eligible	0.6587***
	(0.0012)
Constant	0.0421***
	(0.0008)
Observations	5,000
Method	OLS

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. Column 1 includes cohort dummies. *** Significant at the 1 percent level.

Table 4

			Dependent Variable: Crime Rate	
Cohort	*1958-62*	*1958-62*	*1958-62*	*1958-62*
	(1)	*(2)*	*(3)*	*(4)*
Draft Eligible	0.0018***	0.0018***		
	(0.0006)	(0.0006)		
Conscription			0.0027***	0.0027***
			(0.0008)	(0.0008)
% Change	3.92	3.96	3.92	3.96
Controls	No	Yes	No	Yes
Observations	5,000	5,000	5,000	5,000
Method	OLS	OLS	2SLS	2SLS

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. All models include cohort dummies. The models in columns (2), and (4) include controls for origin (naturalized or indigenous) and district (the country is divided in 24 districts). In 2SLS models the instrument for *Conscription* is *Draft Eligible*. % Change for 2SLS models is calculated as 100*Estimate/mean crime rate of draft-ineligible men. For intention-to-treat models, percent change is reported as 100 × Wald estimate/mean crime rate of draft-ineligible men.

a) ¿How can you calculate the Local Average Treatment Effect using the information in Table 3 and the information in columns (1) and (2) in Table 4 (that is, without estimating an IV directly)?

You can use the formulae in Angrist (Econometrica 1998) and estimate the LATE as the ratio of the reduce form to the first stage.

b) Explain the interpretation of the IV estimator as a local estimate, and compare this interpretation with the one from a sharp design in a regression discontinuity approach.

In the RD approach, the local refers to an impact around the cutoff. This refers to the external validity of the interpretation (only for individuals close to the cutoff).

In the IV, the local refers to the fact that the estimated effect is only for those individuals that changed their status of treatment due to the instrument (the random assignment).

3) The government of Jamaica is about to start a program called “Women City” that has the objective of building facilities aimed to taking care of women problems (such as providing psychological help after domestic abuses, providing them with health facilities, etc). All women have the right to use the facilities. Provide a design that allows you to identify the causal impact of the program on any of the many outcomes of

interest, such as future health. Please explain which are the treated and control groups, and possible limitations to your design. The program is starting next year.

Since all women have the right to use the facilities provided by the program, the best approach is to use an encouragement design, sending letters to a random sample of women. The intuition is that those women receiving the letter should be more likely to attend the facility. So the idea is to use the randomly assigned encouragement as an instrument for attending the facility. Probably, a limitation is that non-compliance will be too high, affecting the external validity of the impact evaluation analysis. An important feature of the design is that the letter should not include any information that potentially impact directly on the outcome of interest, since in that case the instrument would be violating the exclusion restriction.

More details would be nice. For example, the proposal should explain that the best approach is to obtain a random sample of women from the population (a sample that is big enough, of course) and then split randomly this sample in encourage to treatment (receive the letter) and encourage to control (not receiving the letter). Since the program is starting next year, it is possible to run a pre-treatment survey and a post-treatment survey. At the very least, the sample of women has to be interview in a final survey. Given the random assignment, the pre-treatment survey is not needed for consistency, but would help for efficiency.

An alternative to the encouragement design is to use the distance of the woman's house to the facility as an instrument of treatment (using the facility).

4) You have to review a paper from a scholar that is interested in estimating the causal impact of migration on corruption (measured as paying bribes to public officials). The analysis is done at the household level, using data for 2010. Since migration is potentially endogenous in a model for corruption, the author proposes using migration in the 60s and 70s at the locality level as an instrument for current migration. Please explain if you agree or not with author's identification strategy.

The instrument is likely not to satisfy the exclusion restriction. An issue could arise if the municipality-level migration 40 years ago was driven by local corruption at that time (for example, people emigrated more from more corrupt municipalities), and the local-level variation in corruption has persisted overt time. If this were to be the case, the historical emigration rates would be linked to present-day corruption via past migration.