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Viewpoint

The case for uniform controls in drug policy studies

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One of the greatest threats to the validity and reliability of drug policy research studies is the selective inclusion of controls. Policies are not formulated in a vacuum, and the inclusion of a rich, comprehensive array of demographic controls reduces the possibility that a correlation in the data is better explained by endogenous factors than the studied independent variable. Whether intentional or not, exclusion (or inclusion) of controls can make spurious relationships appear significant (or vice versa). This issue is important in studies of drug policy as demographics contribute significantly to usage rates and attitudes (Kandel, Chen, Warner, Kessler, & Grant, 1997).

However, in drug policy studies, there is significant and often unexplained variability in the demographic controls included in statistical models. Most recent American studies use well-known, publicly available datasets (oftentimes from the U.S. Census Bureau) with reliable, uniformly reported data for demographic controls, and yet different researchers routinely choose different controls without explanation.

As part of a research project, I recently reviewed the literature related to American counties with restrictive, semi-restrictive, or non-restrictive alcohol sale laws, often referred to as dry, moist, or wet counties. As expected, virtually every published study included a statistical model with demographic controls. Demographic controls represent subjective and fundamental assumptions of an empirical study, and one could expect that a certain set of demographic controls would be included uniformly in virtually all studies related to this specific topic. Indeed, certain demographic controls, such as population density and percent of people who belong to religious institutions, were included in virtually all the published studies I reviewed. However, the inclusion of other controls, such as political party affiliation, was inconsistent. Political party affiliation was included as a control in some studies (see, for example, Anderson, Crost, & Rees, 2014; Billings, 2014; Brown, Jewell, & Richer, 1996; Frendreis & Tatalovich, 2010; Gotwalt, 2008) and not in others (see, for example, Stewart, Reese, & Brewer, 2004), and no study I read provided a compelling reason for the variability. Despite the promise of one author (Billings, 2014) who included political party affiliation as a control that "the motivation for most of these [control] variables [including political party affiliation] is based on existing literature," two of the studies he referenced, Gyimah-Brempong (2001) and Brown et al. (1996), did not include political affiliation as a control and the third, Baughman, Conlin, Dickert-Conlin, and Pepper (2001), provided an unsatisfying explanation for why political affiliation was included as a control, which was itself contingent upon other control variables: "The percentage of voters registered in each political party is typically used to measure voter ideology or sentiment not reflected in other taste variables."

In the same research project, I replicated the results of one of the most well-publicized drug policy studies of 2015, which found that, after controlling for a rich array of demographic factors, wet counties in Kentucky had a significantly higher incidence of meth lab seizures and meth production (Fernandez, Gohmann, & Pinkston, 2015). The study's results received national and international attention and were featured in The Wall Street Journal, The Economist, and The Washington Post. The authors of the study provided me with the data and code used for five of their OLS statistical models for replication purposes. However, the model in their study did not control for political affiliation. When I controlled for percent of the population registered Democrat (using publicly available, county-level panel data from the Kentucky Department of State), the study's results were no longer statistically significant – political affiliation better explained variability of meth lab seizures than county alcohol policy (see Table 1). This result is not due to collinearity of existing controls with political affiliation; the variance inflation factor (VIF) score, a measurement of multicollinearity, for political affiliation was less than 2 (while 5 is considered a conservative threshold and the average VIF of the originally included controls is greater than 7).

Like the authors of several other studies, the Fernandez team did not include an explanation for why political affiliation was excluded. Yet, a reader cannot agree with the results of the study without agreeing to the authors' premise in excluding political affiliation – a logical gap which could easily be overlooked by policy makers or reporters, especially considering that political affiliation is not mentioned in the paper as a possible control. Of course, biased authors in the future may exploit similar opportunities to support correlations that are, in truth, due to a confounding variable.

I propose that seasoned researchers develop a list of demographic controls to be regularly included in American drug policy studies and that peer-reviewers make a point to question authors of the reasoning behind the inclusion/exclusion of those listed controls. This step would not coerce authors to include controls that are irrelevant to their specific study, rather to explicate their assumptions for why excluded controls are, indeed, irrelevant.

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Table 1
Comparison of models with and without political affiliation controls.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 1 TC	Model 2 TC	Model 3 TC	Model 4 TC	Model 5 TC	Model 6 TC
Wet County	−1.509**	−1.434**	−1.289*	−1.772***	−1.746***	−1.624**	−0.887	−0.836	−0.657	−1.167*	−1.190*	−1.004
	(0.591)	(0.608)	(0.664)	(0.597)	(0.637)	(0.682)	(0.641)	(0.656)	(0.705)	(0.653)	(0.690)	(0.727)
Moist County	−1.021*	−1.226**	−1.234**	−1.066**	−1.367***	−1.592***	−0.715	−0.897*	−0.922	−0.814	−1.068**	−1.273**
	(0.521)	(0.533)	(0.571)	(0.513)	(0.525)	(0.570)	(0.525)	(0.538)	(0.573)	(0.521)	(0.538)	(0.579)
Constant	−79.95	−91.65	−107.0*	−67.04	−76.25	−72.50	−124.1**	−132.5**	−154.7***	−115.6*	−120.5**	−125.0**
	(57.01)	(56.87)	(57.46)	(57.60)	(56.74)	(57.97)	(57.65)	(57.69)	(58.43)	(58.94)	(58.44)	(60.05)
Observations	840	840	840	770	770	770	840	840	840	770	770	770
R-Squared	0.182	0.185	0.190	0.191	0.195	0.204	0.197	0.199	0.206	0.204	0.207	0.218
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls in Original Study	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
At Least One Highway	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
Specific Highways	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Common Support	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Added Control: Political Affiliation	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses.
* $p < 0.1$.
*** $p < 0.01$.
** $p < 0.05$.
Note: TC models are exact replicates of Models 1–6 with the exception that TC models include a control variable for political affiliation (percentage of voters registered Democrat).

The problem of variable controls does not only exist within the context of American drug policy studies. The potentially rising significance of drug policy agreements that affect groups of countries, such as United Nations agreements and treaties, prompts researchers to consider how a list of common controls could be made for international drug policy studies, such as when one or several countries adopt nation-wide drug control policy (or policies) to be analyzed compared with other nations. Much of international drug policy research can be challenged or refuted based upon the inclusion of controls. For many international studies, a control is a case comparison city or jurisdiction, such as when Reinarman, Cohen, and Hendrien (2004) compared cannabis policies in San Francisco versus those in Amsterdam. These case study comparisons often rely on more assumptions than a reader may expect. For example, Reinarman et al. (2004) state that the Amsterdam and San Francisco were studied together based upon the two cities being "large, highly urbanized port cities with diverse populations of slightly more than 700,000" and that "They are financial and entertainment hubs for larger regional conurbations, and they have long been perceived within their home countries as cosmopolitan, politically liberal, and culturally tolerant." This list seems inclusive, comprehensive and convincing, but the "controls" are qualitative, not quantitative, and do not include major differences in population density (approximately 18,000 people per square mile in San Francisco versus 9000 people per square mile in Amsterdam) or the legality of other industries, such as the presence of legal prostitution in Amsterdam. These assumptions are not necessarily naive – ignoring these demographic controls may be perfectly reasonable in the analysis – but readers and policy analysts should at least be aware that these assumptions were made, if not an explanation for why these assumptions can be made. An internationally accepted list of controls could ensure that readers and policy makers reading this influential study are aware of the assumptions the authors make.

Of course, making a list of controls to be considered in international drug policy research will likely be more difficult than forming a list of controls to be included in drug policy research for the United States, alone. There will undoubtedly be more cultural and demographic variables to consider including: government type, time of war or crisis, overall trust in government, and percentage of the workforce involved in the black/gray market. These cultural variables may differ in more dimensions, such as libertarian versus authoritarian rather than just the traditionally accepted spectrum of liberal versus conservative views in the United States. In addition to considering cultural controls, an international panel of drug policy experts will have to consider different types of governments, histories of oppression for certain populations within countries (different countries have oppressed different populations), and more. Just as with controlled studies of American jurisdictions, these assumptions should be explicated in published research to improve the rigor and reliability of international drug policy studies. To make this task more feasible, a panel may decide to divide geographic areas that are fairly comparable so as to limit the number of controls a researcher would need to consider. For example, it likely does not make sense to compare alcohol control policies in Italy to those in Iran because of vast cultural differences between the two countries. Instead, it would seem more feasible to break up geographic regions that share at least somewhat similar cultures (i.e. comparing Italy with Western, Central, and/or Southern European countries).

When a discussion of why common controls were included/excluded is omitted from an article, policy-makers and other readers – particularly those without an academic background – can easily interpret (or, perhaps, misinterpret) that the analyses are unconditionally correct, even if there exists an overlooked disagreement in the assumptions of the model. In other words, the sin of omission is inconspicuous to even the most critical of readers.

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