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Causal Determination for Social Policy:
Counterfactuals, Natural Experiments, Population Shifts

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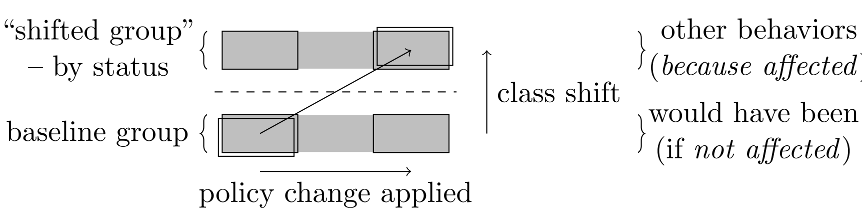
Overview

This paper shows how the causal effects of policy are determined in the public policy arena.

By comparing otherwise similar groups, and proving that the *only* change one of the groups experienced was due to a policy change, one may determine the *effect* of that policy (Chart 1, explained below). One says, *counterfactually*, had the policy change *not occurred* the effect *would not take place*. For instance, what if all the policies supporting the sexual revolution of the 1960s on had instead been configured to work against it?

Go now to the final section, "Randomized Assignment: Clinical Trials, Example," page 7, for an intuitive example on how each of these 'groups' and the 'policy change' interrelate.

Chart 1 Determination of *Effect* of a Policy Change: Counterfactual Situations



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Structure of the Paper

After the general section on counterfactuals, the following sections describe four paths to determining causality:

- Natural Experiments
- Population Shifts
- Instrument Variables
- Randomized Assignment: Clinical Trials, Example

In each of these sections we include an example of the method. Note, instrument variables are a highly technical means for determining causality.

Counterfactuals

For a given population, a counterfactual asks,

> "What if an identified policy [change] had not happened?"

Or – what it similar – "What if policy had effected some other change?" If there is a plausible way to answer the question by comparing a statistically "identical" group[1] to the group experiencing this change, the situation of a "social experiment" has been set up: the analyst may quantify the effect of some change not occurring, or occurring differently.[2] For instance, as above, what were the economic *effects* of marriage being traded off for single status after the 1960s? How would economic agents' behaviors be different had this trade-off not occurred?

[1]This group is known as the 'control' group.

[2]Implicitly, social science assumes populations of men are at least somehow comparable. That is, there are commonalities to men which when paired with identifiable changes, the system is mutually investigatable. Social science does not take for granted that these commonalities are *identifiable* (so-called propensity score matching techniques assume this). Nor does social science take for granted that these commonalities have been produced in any given comparison set (as above): the onus is on the investigator to demonstrate commonalities have been preserved. The last section on so-called instrument variables describes one technique for the preservation of commonalities, pre- and post- effected change. Within that regimen it must be showed that so-called externality restrictions have been preserved to guarantee the production of comparison sets.

Social science normally assumes – without loss of generality – that these comparisons are quantifiable, though this assumption can be relaxed.

Chart 1 illustrates this situation generically. A policy change is applied (moving from left to right, following the lower arrow), resulting in a distinguishing of three sub-populations. The boxes demark the sub-populations: a population which remains along the lower gray band, a population which remains along the upper gray band, and a population which traverses diagonally lower-left to upper-right. The population which runs along the lower gray band is the baseline unaffected, or 'control,' population. The policy change does not affect this group. The population which runs along the upper gray band is another unchanging population. This population has a shifted status relative to the baseline group. It is this population – with its associated measureable behaviors – that the *affected* population[3] conforms to *after the policy change has been applied.* The affected population is the box that moves along the diagonal arrow: from lower left (behaving pre-policy change as the baseline, control group does[4]) to upper right (behaving post-policy change as the other shifted group does). That is, for the affected population, there is a class shift (following the arrow on the right) *because of* the policy change.

Natural Experiments

Simple examples of "quasi-natural experiments"[5] are those of the Great Society welfare program roll-outs across the states over the 1960s and 1970s. Also, AFDC-TANF welfare reform of the 1990s conforms to this model of "natural experiment."

In cases such as these, the policy change is literally a policy change: This change occurs pseudo-randomly across geographic areas – say U.S. states. Some areas implement a policy change early, others later. The changes occur at pseudo-random times. It is usual that over time all areas implement the change. Thus, with proper statistical summarization, one can 1) collect the pseudo-random changes applied, 2) track baseline levels (those areas who have not yet implemented the change), 3) measure the change in behaviors as part of the consequent (pseudo-random) shifts in group status, and thus 4) determine the effect of those policy changes on the changing behaviors.

[3]This population is often called the treated population.

[4]That is, prior to the policy change. This "prior" is not necessarily prior in time, it is prior to application, hence prior to effect. This is the essence of the counterfactual: if policy application *had not occurred* these groups are (would continue being) identical.

[5]We shorten this technical term to "natural experiments" for ease of exposition.

Population Shifts

When one re-compares an entire demographic set to itself, the statistical identity of the two groups is immediate.[6] Thus, watching a population evolve through a policy change or through cultural choice can give rise to a counterfactual situation. Here the baseline class is that part of the population which is not changing (i.e. is invariant) during the evolution, while the policy change or cultural shift causes some part of the population to move out of the class. Likewise, the class shifted into is a different part of the population which is invariant while the policy change is taking place. Now, some part of the population moves into that class as a result of the policy change or cultural shift. It is this part of the population – the shifting part – which is the affected population. The policy change has moved the affected population from the baseline group to the "shifted group." The policy change has caused the class shift, along with associated behavior changes.

Causation obtains so long as the forces on the population are characterized (as either policy changes or cultural choices) and each of the groups (baseline class and shifted-into class) maintain their own behavior irrespective of the population evolution and policy/culture change. That is, causation obtains so long as the population class' characteristics do not change – the population may only evolve across the classes.[7]

In *Non-Marriage Reduces U.S. Labor Participation*[8] the "policy change" is really a cultural change: The population moves out of marriage (the baseline group status, which would have been continued had the cultural change not occurred) and into singlehood (the "shifted group" status). This occurs over the decades. The class shift from married to single is accompanied by a behavior shift: There is a persistent gap in the propensity to work between these two classes. This gap persists over the decades, and it persists across highly varied macroeconomic environments. This shift holds irrespective of

[6] This is a strengthening and formalizing of the point made in George Akerlof, "Men Without Children," *The Economic Journal* 108 (Mar. 1998): 287–309.

[7] Of course, as populations move through time older members are removed (e.g., through death) and newer members are added, (e.g., through birth). Explicitly, then, there is potential *cohort variation*. The unimportance of cohort variation must be demonstrated by the analyst. In the cases drawn on below this is demonstrated by considering, and rejecting, the possibility that other concurrent factors may be at play.

[8] Henry Potrykus and Patrick Fagan, *Non-Marriage Reduces U.S. Labor Participation: The Abandonment of Marriage Puts America at Risk of a Depression*, available at `marri.us/labor-slump`, techreport (MARRI, 2012).

time, irrespective of occupation type,[9] and irrespective of age.[10] That age does not matter is shown in that report to contradict the argument that there is variation in behavior between the cohorts: each age-group cohort is tested again and again, and the behavior (the gap in propensity to work) is found to hold cross-cohort and cross-time. That time does not matter shows the robustness of the finding, to business cycles.

This robustness of the gap in working to business cycles is further exploited. A plausible counter-argument to the population shift argument is that the U.S. labor market was simply becoming "tougher." Hence the reverse causality direction (tougher job markets with less employment opportunity reducing the ability for males to attract a mate) might *ex ante* be operative.[11] However this argument is obviated by the fact that the 1990s was the period of greatest *consistent* expansion of the U.S. economy,[12] that there were great periods of expansion at other times as well, and that at none of these periods was any marriage level recovered – even in part – by these males.[13]

Hence, for *Non-Marriage Reduces U.S. Labor Participation*, it was only the cultural change that caused the "affected group" to move from the baseline group into the "shifted group": the cultural change caused the change in labor participation (decrease) of those men who would otherwise have been married but, *because of* the cultural change, took on single status along with its associated behavior (lower labor participation).[14]

[9] All the while, there have been massive flows into and out-of certain occupation types.

[10] This last fact is mentioned but not graphed in the paper.

[11] We eschew making the (important) argument that lower "quality" women exist to match with these lower "quality" men: In the past lower human capital level men did marry. We readily conjecture that this is a mechanism for American "upward mobility." (This in the sense of both "quality" formation and human capital formation. The latter is both quantifiable and provable: see Henry Potrykus and Patrick Fagan, *The Divorce Revolution Perpetually Reduces U.S. Economic Growth*, available at `marri.us/productivity-divorce`, techreport (MARRI, 2012).) This is a mechanism sorely lacking in today's society, cf. Potrykus and Fagan, *Non-Marriage Reduces U.S. Labor Participation: The Abandonment of Marriage Puts America at Risk of a Depression.*

[12] Cf. Chart 2 in Henry Potrykus and Patrick Fagan, *Decline in Economic Growth: Human Capital & Population Change*, available at `marri.us/human-capital`, techreport (MARRI, 2011)

[13] Furthermore, lower "quality" men, which might be identified with lower skill professions, benefited massively by the construction boom of the 1990s and 2000s. See Kerwin Charles, Erik Hurst, and Matthew Notowidigdo, *Manufacturing Busts, Housing Booms, and Declining Employment: A Structural Explanation*, techreport (University of Chicago, 2012).

[14] A similar but somewhat more technically-based argument is made concerning male *productivity gains* in another of our papers. See Potrykus and Fagan, *The Divorce Revo-*

Instrument Variables

A third method of finding a change that goes along *only* some single well-defined policy channel is via the use of so-called instrument variables. Within the context of the class shift, one finds a variable (the instrument) which affects *only* the specific policy channel. Below we give a more policy-oriented example, but a classical instrument affecting fish markets[15] is *the weather*. The weather out on the sea affects fish supply (which is like our "policy channel"), but it plausibly does not affect fish demand in inland markets. Hence behaviors (fish consumption) are only affected through this single channel.

Likewise, in the general setting, the policy channel is influenced – and *only* it is influenced – by the instrument. Imagine a change seen simultaneous in class shift and in the instrument. In this case (class behavior change seen simultaneous with instrument change), the change in class behavior must be because the instrument influenced the policy direction. This again is because *only* the policy channel is influenced by the instrument. Concordantly the policy channel must be the channel through which a change in class behaviors occurred. It is the *only* channel through which the class behavior change was seen simultaneous with instrument change. Thus the shift in class behaviors occurred *only through*, and so *because of*, the policy channel change. The instrument isolates the effect of policy action (which is through the only variable the instrument influences) on class behavior shifts.[16]

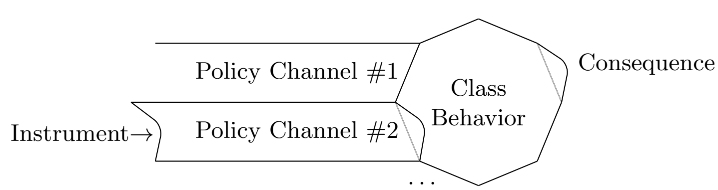
An example of a commonly-employed instrument variable is the distance

lution Perpetually Reduces U.S. Economic Growth.

Workforce productivity growth is important to overall economic growth, hence the strength of the macroeconomy. See Potrykus and Fagan, *The Divorce Revolution Perpetually Reduces U.S. Economic Growth*, and Potrykus and Fagan, *Decline in Economic Growth: Human Capital & Population Change.*

[15]Bear with us. This example has endured in the pedagogic literature because of its simplicity.

[16]The "deformation" of the policy channel – and through the policy channel, class behavior – may be depicted as follows:



to an abortion facility. It has been shown that mothers are less likely to procure abortions the further the mothers are from those places.[17] Thus the effect of abortion on society may be found by measuring a social outcome relative to the distance from abortion facilities.[18]

We do not elaborate further on this statistical technique, and say only that it is one of the standard methods of determining causality in advanced econometrics.

Randomized Assignment
Clinical Trials, Example

The classical case of determining the effect of a "policy change" on a population is that of clinical trials. In such trials patients are randomly assigned to a treatment group – receiving the drug being tested, say Insulin, and a control group – receiving a placebo, i.e. no treatment. The baseline group is this control group. The affected group is the treatment group. The "shifted group" would be a group having Insulin levels determined by the Insulin regimen (e.g. all members having levels as a normal body might secrete the hormone). The *effects of the regimen* would be found on that treatment group, as it *as a group* shifts from baseline to "shifted" status. If the affected group and the baseline group were large and contained within it diabetics (those not able to metabolize sugars because of an abnormal amount of Insulin delivered by their body), one would find the health of the affected group (treatment group) improving relative to the baseline group (control group). This assumes of course that there are not other, negative effects of the Insulin regimen. In this case, the control group, containing a (perhaps small) sub-population lacking Insulin relative to the shifted group, has lower health outcomes: all the ills the sub-population with diabetes must bear. These improving health outcomes are the associated "other behaviors" seen especially in the sub-population of affected population with diabetes. The

[17] This is actually re-proved *every time* this variable is employed in the above statistical procedure.

[18] That placement (supply) of abortion facilities is not reactive to abortion demand is an important point that must be addressed to make this "instrument" *not* be related to the other factors determining abortion demand (hence procurement). See Rebecca Blank, Christine George, and Rebecca London, "State abortion rates: The impact of policies, providers, politics, demographics, and economic environment," *Journal of Health Economics* 15 (1996): 513–553 for one way to accomplish this. In the instance of inter-state distances and travel this is plausibly the case. (This latter methodology is at root an aggregation scheme.)

health of the diabetic sub-population improves, and both they and the rest of the affected population would have otherwise remained in the baseline group.

Hence, we have shown in this classical instance (application of a drug regimen to identified populations), the sense of causality found in counterfactual studies.