Contexts, Compliance and Imperfect Recall - Using Two-Sample IV to Estimate Average Causal Effects of Child Human Capital Investments

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Abstract

Recently, much research has focused on determining critical and sensitive periods in child development. In this context, the use of non-experimental data can lead to biased estimates if parental investments and human capital outcomes jointly depend on unobserved confounders. To deal with this problem, many studies relate so-called contextual variation, in the form of events that are exogenous from the point of view of the individual, to later human capital outcomes. Yet, being a child when a contextual shock materializes does not necessarily imply individual suffering, such that the average causal effect of early-life investments on the individual cannot be determined from contextual information alone. This paper explains how instrumental variable estimation can be use to obtain causal estimates from contextual variation and information about individual human capital in-
vestments. It then discusses how combining information from two different samples can be used to compute causal effects in the presence of recall bias.

Introduction

Human capital is vital to individual success in life and constitutes one of the most valuable economic resources. But how and when does it develop, and what are the most important human capital investments? Finding answers to these questions is not an easy task. First, experiments are ideal from a scientific point of view, but not always ethical or possible. Second, recall bias is omnipresent, since most individuals do not fully recollect investments that took place in early childhood. In this context, the use of non-experimental data can lead to biased estimates, e.g. if childhood investments are imperfectly recalled or if outcomes and investments are jointly determined by unobserved confounders.

To deal with these problems, numerous studies have examined the effect of so-called contextual variation, i.e. events that are exogenous from the point of view of the individual child, on health and socio-economic outcomes later in life. These studies mostly compare outcomes of individuals who experienced a certain exogenous shock during childhood to outcomes of other individuals. Contextual variations that have been analyzed in this literature range from exposure to radioactive fallout, terrorist attacks, flu pandemics, famines and changes in the supply of fast food restaurants to weather shocks (Almond, Edlund, and Palme, 2009; Camacho, 2008; Almond, 2006; Currie, DellaVigna, Moretti, and Pathania, 2010; Maccini and Yang, 2009). The rationale for using such contextual variation is to determine age periods during which educational and health investments are particularly important.

However, having experienced a contextual change early in life does not necessarily imply that the human capital investment process is affected and vice versa.
Therefore, estimates from contextual variation only inform us about population-wide effects. Most times however, the estimate of interest is the average causal effect of certain childhood investments (the average causal effect on the individual). The difference between the population-wide, or Intention-To-Treat (ITT) effect, and the average causal effect on the individual is well-known and in general it is possible to use the contextual variation as an Instrument Variable (IV) for the treatment of interest, in order to obtain the average causal effect. Data on outcomes, linked to important contextual variables, usually contain detailed location information, allowing researchers to make statements about changes in a child’s living environment. Such data, however, often lack information about individual investments. In other cases, information about human capital investments is available, but subject to recall bias if experienced at very young ages.

This article, summarizes how instrumental variable estimation can be used in a Local Average Treatment Effects (LATE) framework to obtain causal estimates from contextual variation and information about individual suffering for the case where both variables are binary. It then discusses how average causal effects can be computed in the presence of imperfect recall or if information about compliance is entirely missing and has to be retrieved from another data source.

Contextual Effects

Exposure to contextual variation around birth or during childhood allows researchers to estimate so-called reduced form or ITT effects. Take the example of fast food restaurants from Currie, DellaVigna, Moretti, and Pathania (2010). If we are interested in the effect of unhealthy food consumption on child obesity, it might not be possible to conduct an experiment, where French fries and ham...

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1 See also the seminal paper by Angrist and Pischke (2008) and the exposition in Imbens and Angrist (1994). In the language of clinical trials, the ITT effect compares individuals who were offered a treatment to those who were not.
burgers are randomly allocated to some families with small children, but not to
others. It might however be possible to make use of some random event at the
macro level, such as the opening of a fast food restaurant nearby, which lowers the
monetary and non-monetary costs of accessing unhealthy food. In this case the
nearby opening of a fast food restaurant is the contextual variation \( Z \in \{0, 1\} \),
which randomly affects the actual human capital investment variable (eating
more than a threshold amount of unhealthy food), denoted by \( D \in \{0, 1\} \), after
conditioning on some covariates \( X \). This can be helpful, if eating unhealthy food
\( (D) \) and child obesity \( (Y) \) jointly depend on unobserved confounders, such as
parenting practices (see figure 1 for a causal graph in the spirit of Pearl 2000).
If \( Z \) is randomly assigned and if it moves all individuals ‘in the same direction’,
that is, easier access to unhealthy food does not induce anyone to reduce its con-
sumption (in IV-talk this means that there are no defiers), the ITT effect can
be obtained by looking at the difference in average outcomes between individuals
who were exposed and those who were unexposed to the contextual variation \( Z \):

\[
E[Y|X = x, Z = 1] - E[Y|X = x, Z = 0]
\]

This estimate has a causal interpretation: it will tell us the causal effect of
the offer of treatment, i.e. the effect of living close to a place where a fast food
restaurant opens its doors.

While the above effect is easily estimated and causal, two problems remain.
First, if we are interested in the average causal effect of unhealthy food consump-
tion, information about low-cost access to unhealthy food might not be very
informative. In the example above, not all families who live near a fast food
restaurant will actually visit the restaurant and some families who live very far
away from a fast food restaurant might eat lots of unhealthy food. Similarly,
living close to a fast food restaurant might simply lead to substitution away from
unhealthy food prepared at home, but might not change the overall amount of unhealthy food consumed (Currie, DellaVigna, Moretti, and Pathania 2010). In other words, while proximity to a fast food restaurant reduces the costs of accessing unhealthy food, it will only induce some families to change their eating habits. The observed difference in child obesity rates between families who live close to a fast food restaurant and others will then only provide a qualitative assessment of the average causal effect of eating unhealthy food for the individual child.

Second, selection into the treatment of interest, i.e. eating unhealthy food, is likely to be correlated with unobserved characteristics and therefore endogenous. If information about actual treatment take-up, that is actual fast food consumption, is available, the contextual variation can, however, serve as an instrument for the treatment of interest.

**Instrumental Variable Estimation**

Because not everybody changes their behavior in response to the contextual variation, the ITT effect is too small relative to the average causal effect. The effect on the individual can, however, be obtained by dividing the ITT effect by the difference in compliance (or take-up) rates between treatment and control groups. Under the classical IV assumptions, this gives the LATE, i.e. the average difference in potential outcomes for those individuals whose human capital investments were affected by the instrument (the compliers):

\[
E[Y^1 - Y^0|X = x, DZ = 1 > DZ = 0] = \frac{E[Y|X = x, Z = 1] - E[Y|X = x, Z = 0]}{E[D|X = x, Z = 1] - E[D|X = x, Z = 0]}
\]

(2)

\(^3\)Note that the effect of easy access to fast food (in the form of a fast food restaurant in the neighborhood) might itself be a policy relevant effect.
The assumptions under which this holds are that \( Z \) does not have a direct effect on \( Y \), other than through its (nonzero) effect on \( D \) (a fast food restaurant in the area influences child obesity only because it increases the consumption of unhealthy food); that there are no defiers in the sample; that the probability of compliance is not affected by the realization of \( Z \) (individuals in neighborhoods where a fast food restaurant opens are equally likely to increase their consumption of fast food as individuals in other neighborhoods would be were there a fast food restaurant opening in their neighborhood); and that the support of the covariates in the subpopulations of individuals affected and unaffected by the contextual variation is the same.

It is important to note the limitations of estimating a local effect. Unless the (hypothetical) difference in potential outcomes \( Y^1 - Y^0 \) is the same in the entire population, this implies that the estimated effect only holds for those individuals whose human capital investment process is affected by the contextual change. Hence agents induced to treatment by a given \( Z \) (opening of a fast food restaurant) need not be the same agents induced to treatment by an unrelated policy change (such as allowing schools to serve fast food for lunch). Therefore, researchers need to ask about the external validity and policy relevance of the LATE they estimate, unless the instrument-induced effect of treatment is the policy-relevant effect in question.\(^4\)

Equation \( 2 \) gives the LATE for every value of \( X \). Frölich (2007) shows that the average effect for the subpopulation of compliers can be obtained from:

\[
E[Y^1 - Y^0|D_{Z=1} > D_{Z=0}] = \frac{\int E[Y|X = x, Z = 1] - E[Y|X = x, Z = 0]}{\int E[D|X = x, Z = 1] - E[D|X = x, Z = 0]} f(x) dx.
\] (3)

Looking at equations \( 2 \) and \( 3 \), it becomes obvious that the LATE is simply the ITT effect (equation \( 1 \)) scaled by the rate of compliance. The magnitude by which the average causal effect exceeds the reduced-form population effect hinges on the

\(^4\)Recent research shows that a linear marginal treatment effects model (from which information about treatment effects for different parts of the population can be inferred) can be identified even with a single binary instrument. Mogstad, Brinch, and Wiswall (2014).
percentage of individuals whose human capital investment process is affected by
the contextual variation. Only at the extreme, if compliance to the contextual
variation is perfect, the reduced-form effect conforms to the individual effect.

**Missing Compliance Information or Imperfect Recall**

To obtain the average causal effect from the ITT (numerator of equation 2),
researchers thus need information about take-up of the treatment of interest
(denominator of equation 2). In the above example, this would imply information
about actual fast food consumption per child. The most straightforward way of
obtaining such information is by conducting a survey which asks children about
their fast food consumption. However, obtaining such compliance information
is not always easy. Often, quite some time can pass between the contextual
change and the compliance survey. This can, for instance, be the case when the
contextual variation is a famine or a flue pandemic during childhood and the
outcome of interest is adult health. In such cases, the parents of the children
might long be dead and recall of the surviving children might depend on the
age period they are asked about. In general, recall of a any investment may be
difficult if this period took place before age 4.

Take a second example where the contextual variation (Z) is a famine and
where undernutrition is the treatment (D). For this case, van den Berg, Pinger,
and Schoch (2014) find that children aged 6-16 during a famine are more than
twice as likely to report hunger than younger children (see figure 2).

To deal with such a situation, data combination can be useful. Equation 3
corresponds to the ratio of two estimators, which implies that the numerator and
the denominator can be estimated from two different samples. Thus, if children
before age 4 do not remember the investment, a compliance estimate from older
children can be obtained, as long as $E[D|X = x, Z = 1] - E[D|X = x, Z = 0]$ is
the same in both samples\(^5\). This assumption is not innocuous. Yet, in many cases it is possible to induce whether the denominator is larger or smaller in the substitute sample when compared to the original sample. If it is larger, then the two-sample estimate provides a lower bound for the average causal effect.

**Conclusion**

Research on the formation of human capital is difficult, because non-experimental measures of childhood investments are almost never free of confounding. Therefore, many recent papers have relied on contextual changes as quasi-experimental settings, which allow researchers to compare outcomes of individuals who did and who did not experience an exogenous shock during childhood. However, those comparisons only provide the so-called ITT. To obtain the average causal effect, they have to be scaled upwards unless compliance is perfect. Two-sample IV estimators make the scaling possible, even if the information about compliance stems from a different data source than the ITT estimate.

**References**


\(^5\)This can be done by using either a non-parametric Wald estimator or the parametric two-stage least squares estimator (2S2SLS). The 2S2SLS estimator has been developed by Angrist and Krueger (1992) and Arellano and Meghir (1992); Inoue and Solon (2010) adjust the estimator for use in small samples.


Appendix

Figure 1: Relationship between contextual variable (Z), treatment (D), outcome (Y) and unobserved characteristics (V, U)

Figure 2: Probability to report hunger conditional on famine experience at respective age (reprinted from van den Berg, Pinger, and Schoch, 2014)