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N. 473
First Version VII.2016, This Version VII.2017

Faculté des sciences économiques et sociales
Wirtschafts- und sozialwissenschaftliche Fakultät

Direct and indirect effects based on difference-in-differences with an application to political preferences following the Vietnam draft lottery

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Abstract: This paper proposes a difference-in-differences approach for disentangling a total treatment effect on some outcome into a direct effect as well as an indirect effect operating through a binary intermediate variable – or mediator – within strata defined upon how the mediator reacts to the treatment. Imposing random treatment assignment along with specific common trend (and further) assumptions identifies the direct effects on the always and never takers, whose mediator is not affected by the treatment, as well as the direct and indirect effects on the compliers, whose mediator reacts to the treatment. We provide an empirical application based on the Vietnam draft lottery, where we analyse the impact of the random draft lottery number on political preferences. The results suggest that a high draft risk due to the lottery leads to a relative increase in mild preferences for the Republican Party, but has no effect on strong preferences for either party or on policy contents. Moreover, the increase in Republican support is mostly driven by the direct effect.

Keywords: treatment effects, causal mechanisms, direct and indirect effects, Vietnam War lottery, political preferences

JEL classification: C21, C22, D70, D72

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1 Introduction

Treatment or policy interventions causally affect an outcome of interest through various mechanisms. An example is the Vietnam draft lottery in the US which might have affected outcomes such as political preferences later in life through military service during the Vietnam War or college deferments to avoid conscription. Causal mediation analysis (Robins & Greenland, 1992; Pearl, 2001; Robins, 2003) aims at disentangling the direct effect of some treatment on an outcome from the indirect effects operating through one or more intermediate variables, called mediators.

The main contribution of this paper is to propose a difference-in-differences (DiD) approach under random treatment assignment which separates direct and indirect effects within subpopulations or strata defined upon the reaction of a binary mediator to the treatment. Our approach identifies so-called principal stratum direct and indirect effects (see Rubin, 2004; VanderWeele, 2008). Borrowing from the nomenclature in Angrist, Imbens, and Rubin (1996), we present assumptions that are sufficient to identify direct effects for "always" and "never takers", whose binary mediator is (independently of the treatment) either always or never equal to one, as well as direct and indirect effects on the "compliers", whose mediator value always corresponds to the treatment. Among others, random treatment assignment, monotonicity of the mediator in the treatment, and specific common trend assumptions across strata are imposed for identification.

In contrast to our approach, a good part of the literature on causal mediation analysis assumes conditional exogeneity of the treatment (given observed covariates) and the mediator (given the treatment and the covariates), which requires observing all confounders of the treatment and the mediator. Such "sequential ignorability" is for instance imposed in Petersen, Sinisi, and van der Laan (2006), Flores and Flores-Lagunes (2009), VanderWeele (2009), Imai, Keele, and Yamamoto (2010), Hong (2010), Tchetgen Tchetgen and Shpitser (2012), Zheng and van der Laan (2012), and Huber (2014). Alternatively, relatively few contributions consider identification based on instruments, see for instance Imai, Tingley, and Yamamoto (2013), Yamamoto (2013), and Frölich and Huber (2014). Our paper is to the best of our knowledge the first one to offer an alternative to sequential ignorability and instrumental variable assumptions based on a DiD approach in the context of mediation analysis.

While most mediation studies focus on the total population, comparably few contributions discuss effects in subpopulations (or principal strata, see Frangakis & Rubin, 2002) defined upon the value of the binary mediator as a function of the treatment, see for instance Rubin (2004). Principal stratification in the context of mediation has been criticized for typically not permitting a decomposition of direct and indirect effects among compliers and focussing on subgroups that may be less interesting than the entire population (see VanderWeele, 2008; 2012). We contribute to this discussion by showing that direct and indirect effects on compliers can be identified in a DiD framework under particular conditions and by presenting an empirical application in which the effect on subgroups is relevant for political decision making.

We use our method to investigate the effect of the Vietnam draft lottery in the US on political preferences and attitudes. The mediator is military service during the Vietnam War. Some individuals (the compliers) were induced by the lottery to serve either through being drafted or pre-emptively joining the military in case of a lottery outcome resulting in being drafted (Angrist, 1991), while others avoided the draft (never takers) for example through college deferments (Card & Lemieux, 2001; Kuziemko, 2010; Deuchert & Huber, 2017), or would have served in any case (always takers). We estimate the direct effect of the draft lottery on the never takers, as well as the direct and indirect effects (via military service) on the compliers.

This is a particularly interesting application for several reasons: A recent literature argues that party preferences and political attitudes are endogenous to policy interventions, which is in stark contrast to standard economic theory assuming stable preferences. Erikson and Stoker (2011) estimated the impact of the Vietnam draft lottery on party preferences and concluded that a lottery outcome resulting in a higher risk of being drafted increased the Democratic support. Bergan (2009) found that it also increased preferences for an immediate withdrawal from Vietnam.

In contrast to these results, we find that the draft lottery significantly increased the probability to at least mildly prefer the Republican Party in the total population. When decomposing the average treatment effect into direct and indirect effects within strata, we find statistically significantly positive direct effects on the probability to at least mildly prefer the Republican Party among compliers and never takers, but an insignificant indirect effect among compliers. Moreover, we find no effects on Vietnam War attitudes, general attitudes towards the government, or attitudes towards the civil right movement. Given that we find no effects on the probability to strongly prefer a party, the result is in line with a traditional swing voter interpretation, in which citizens without a stable party attachment in the centre of the policy spectrum update their stated party preferences. In contrast to Erikson and Stoker (2011) and Bergan (2009) our results are in line with traditional microeconomic assumptions of stable preferences.

We also compare the indirect effect on compliers estimated from our model to the local average treatment effect (LATE) estimate on compliers, an estimation method commonly used in the context of the Vietnam lottery (for example Angrist, 1990; Angrist, Chen, & Frandsen, 2010). Our indirect effect is ten times lower and statistically different from the LATE, suggesting that the findings in the literature are not robust across different methods based on common trend or instrumental variable assumptions, respectively.

The remainder of the paper is organized as follows. Section 2 outlines the econometric framework, i.e., the effects of interest and the identifying assumptions underlying our DiD approach. Section 3 presents an empirical application to the Vietnam draft lottery in which the total effects as well as the direct and indirect effects on political preferences and personal views on war and other governmental policies are estimated for various strata. Section 4 concludes.

2 Econometric framework

2.1 Notation and definition of direct and indirect effects

Let Z denote a binary treatment (e.g., being chosen for military service in a draft lottery) and D a binary intermediate variable or mediator that may be a function of Z (e.g., an indicator for actual military service). Furthermore, let T indicate a particular time period: $T = 0$ denotes the baseline period prior to assignment of Z and D, $T = 1$ the follow up period after measuring D and Z in which the effect of the outcome is evaluated. Finally, let Y_t denote the outcome of interest (e.g., political preference) in period $T = t$. Indexing the outcome by the time period $t \in \{1,0\}$ implies that it may be measured both in the baseline period and after the assignment of Z and D. To define the parameters of interest, we make use of the potential outcome notation, see for instance Rubin (1974), and denote by $Y_t(z, d)$ the potential outcome for treatment state $Z = z$ and mediator state $D = d$ in time $T = t$, with $z, d, t \in \{1,0\}$. Furthermore, let $D(z)$ denote the potential mediator as a function of the treatment state $z \in \{1,0\}$. For notational ease, we will not use a time index for D and Z, because each of these parameters are assumed to be measured at a single period between $T = 0$ and $T = 1$ (but not necessarily the same period, as Z causally precedes D). Implicit in this approach is that the treatment and the mediator are equal to zero in or prior to the baseline period.

The average treatment effect (ATE) in the follow up period is defined as $\Delta_1 = E\big[Y_1\big(1, D(1)\big) - Y_1\big(0, D(0)\big)\big]$. That is, the ATE corresponds to the cumulative effect of Z on the outcome that either affects the latter directly (i.e., net of any effect on the mediator) or indirectly through an effect on D. Indeed, the total ATE can be split into so-called natural direct and indirect effects using the notation of Pearl (2001),[1] defined as $\theta_1(z) = E\big[Y_1\big(1, D(z)\big) - Y_1\big(0, D(z)\big)\big]$ and $\delta_1(z) = E\big[Y_1\big(z, D(1)\big) - Y_1\big(z, D(0)\big)\big]$, by adding and subtracting $Y_1\big(1, D(0)\big)$ or $Y_1\big(0, D(1)\big)$:

$$
\begin{aligned}
\Delta_1 &= E\big[Y_1\big(1, D(1)\big) - Y_1\big(0, D(0)\big)\big] \\
&= E\big[Y_1\big(1, D(0)\big) - Y_1\big(0, D(0)\big)\big] + E\big[Y_1\big(1, D(1)\big) - Y_1\big(1, D(0)\big)\big] = \theta_1(0) + \delta_1(1) \\
&= E\big[Y_1\big(1, D(1)\big) - Y_1\big(0, D(1)\big)\big] + E\big[Y_1\big(0, D(1)\big) - Y_1\big(0, D(0)\big)\big] = \theta_1(1) + \delta_1(0)
\end{aligned}
$$

Distinguishing between $\theta_1(1)$ and $\theta_1(0)$ or $\delta_1(1)$ and $\delta_1(0)$, respectively, implies the possibility of interaction effects between Z and D such that the effects could be heterogeneous across values $z = 1$ and $z = 0$. For instance, $\delta_1(1)$ and $\delta_1(0)$ might differ if the military unit (and war experience) an individual is assigned to when being chosen through the draft lottery is different than when joining the army voluntarily without being drafted. This may have an impact on political attitudes. Furthermore, note that if Z was a valid instrument for D that satisfied the exclusion restriction, as for instance assumed in Angrist (1990) in the context of the Vietnam draft lottery, any direct effect $\theta_t(z)$ would be zero and the indirect effect would simplify to $\delta_1(1) = \delta_1(0) = \delta_1$. In our empirical application outlined below, we do not impose this strong assumption, but allow for direct effects.[2]

In our approach we consider the concepts of direct and indirect effects within subgroups or principal strata in the denomination of Frangakis and Rubin (2002) that are defined upon the values of the potential mediator. As outlined in Angrist, Imbens, and Rubin (1996) in the context of instrumental variable-based identification, any individual i in the population belongs to one of four strata, henceforth denoted by τ, according to their potential mediator status (now indexed by i) under either treatment state: always takers (a: $D_i(1) = D_i(0) = 1$) whose mediator is always one, compliers (c: $D_i(1) = 1, D_i(0) = 0$) whose mediator corresponds to the treatment value, defiers

[1] Robins and Greenland (1992) and Robins (2003) refer to the parameters as total or pure direct and indirect effects.
[2] This assumption has for instance already been challenged in Deuchert and Huber (2017).

(d: $D_i(1) = 0, D_i(0) = 1$) whose mediator opposes the treatment value, and never takers (n: $D_i(1) = D_i(0) = 0$) whose mediator is never one. Note that τ cannot be pinned down for any individual, because either $D_i(1)$ or $D_i(0)$ is observed, but never both.

Introducing further stratum-specific notation, let $\Delta_1^\tau = E\left[Y_1(1, D(1)) - Y_1(0, D(0))\middle|\tau\right]$ denote the ATE given $\tau \in \{a, n, c, d\}$; $\theta_1^\tau(z)$ and $\delta_1^\tau(z)$ denote the corresponding direct and indirect effects. Because $D_i(1) = D_i(0) = 0$ for any never taker, the indirect effect for this group is by definition zero ($\delta_1^n(z) = E[Y_1(z, 0) - Y_1(z, 0)|n] = 0$) and $\Delta_1^n = E[Y_1(1,0) - Y_1(0,0)|n] = \theta_1^n(1) = \theta_1^n(0) = \theta_1^n$ corresponds to the direct effect (and an analogous argument applies to the always takers). For the compliers, both direct and indirect effects may exist. Note that $D(z) = z$ due to the definition of compliers. Therefore, $\theta_1^c(z) = E[Y_1(1, z) - Y_1(0, z)|c]$ and $\delta_1^c(z) = E[Y_1(z, 1) - Y_1(z, 0)|c]$, while $\Delta_1^c = E[Y_1(1,1) - Y_1(0,0)|c]$.[3] Furthermore, in the absence of any direct effects, the indirect effects on the compliers are homogenous, $\delta_1^c(1) = \delta_1^c(0) = \delta_1^c$, and correspond to the LATE, defined as the causal effect of D on Y among compliers.

2.2 Identifying assumptions

We subsequently discuss the identifying assumptions along with the effects that may be obtained.[4] We start by assuming independence between the treatment and potential mediators or outcomes:

Assumption 1: Independence of Z and potential mediators/outcomes

$$\{Y_t(z, d), D(z)\} \perp Z, \text{for all } z, d, t \in \{1,0\}$$

Assumption 1 implies that there are no confounders jointly affecting the treatment and the mediator and/or outcome and is satisfied under treatment randomization as in successfully

[3] VanderWeele (2012) and others point out that the presence of a stratum-specific indirect effect cannot be learnt from Δ_1^c, which also includes the direct effect. Our assumptions below allow decomposing Δ_1^c into $\delta_1^c(z)$ and $\theta_1^c(z)$.

[4] Implicit in our discussion is the "stable unit treatment valuation assumption" (SUTVA), see Rubin (1977), ruling out that the potential outcomes of one individual depend on the treatment or mediator state of any other individual.

conducted experiments or (draft) lotteries. Our subsequent identification results could easily be adjusted to the case that independence only holds conditional on a vector of observed covariates. However, for the sake of ease of notation, we do not consider covariates and note that under conditional independence, any result holds within cells defined upon covariate values.

Assumption 2: Weak monotonicity of D in Z

$$\Pr\big(D_i(1) \geq D_i(0)\big) = 1$$

Assumption 2 is standard in the literature on local average treatment effects (see Imbens & Angrist, 1994; Angrist, Imbens, & Rubin, 1996) and rules out the existence of defiers. Defiance seems to be a common behaviour among children but should not be a major concern when adults are faced with a life changing decision, such as joining the army during war times (as in our empirical application).

Assumption 3: No anticipation effect of D and Z in the baseline period

$$E[Y_0(z,d) - Y_0(z',d')|\tau] = 0, \text{for } z, z', d, d' \in \{1,0\}$$

Assumption 3 rules out anticipation effects of the treatment or the mediator w.r.t. to the outcome in the baseline period. This assumption seems plausible if assignment to treatment cannot be foreseen, for example if assignment is the result of a lottery as in our empirical application.

As shown in the online appendix, Assumptions 1 to 3 imply that $E[Y_0(1,1) - Y_0(0,0)|c] = \Delta_0^c = 0 = E(Y_0|Z = 1) - E(Y_0|Z = 0)$. Therefore, a rejection of the testable implication $E(Y_0|Z = 1) - E(Y_0|Z = 0) = 0$ in the data would point to a violation of our identifying assumptions. Furthermore, Assumption 1 allows identifying the average treatment effect in the total population:

$$\Delta_1 = E[Y_1|Z = 1] - E[Y_1|Z = 0].$$

Moreover, Assumptions 1 and 2 yield the strata proportions, which we denote by $p_\tau = \Pr(\tau)$, as functions of the conditional mediator probabilities given the treatment, which we denote by $p_{d|z} = \Pr(D = d|Z = z)$ for d, z in $\{1,0\}$:

$$p_a = p_{1|0}, p_c = p_{1|1} - p_{1|0},\ p_n = p_{0|1}.$$

Finally, under Assumptions 1 to 3, the differences in average baseline outcomes across always or never takers and compliers are identified by:

$$\mathrm{E}[Y_0(0,0)|a] - \mathrm{E}[Y_0(0,0)|c] = \frac{p_a + p_c}{p_c}[\mathrm{E}(Y_0|Z = 0, D = 1) - \mathrm{E}(Y_0|Z = 1, D = 1)],$$

$$\mathrm{E}[Y_0(0,0)|n] - \mathrm{E}[Y_0(0,0)|c] = \frac{p_n + p_c}{p_c}[\mathrm{E}(Y_0|Z = 1, D = 0) - \mathrm{E}(Y_0|Z = 0, D = 0)],$$

see equations (A5) and (A16) in the online appendix. However, to identify direct and indirect effects for any of these groups, we need to impose some further assumptions.

In contrast to the previous literature which mainly relied on sequential conditional independence or (in considerably fewer cases) on instruments, we subsequently base identification on common trend assumptions, as they are also used for the evaluation of total treatment effects based on difference-in-differences (DiD) across treatment groups (for a survey see Lechner, 2011). In contrast to the standard framework that aims at resolving treatment endogeneity, we impose common trend assumptions across strata to tackle endogeneity due to conditioning on the potential mediator states (through the definition of the strata), while the treatment is random by Assumption 1. This allows for differences in the effects of unobserved confounders on specific potential outcomes across strata, as long as these differences are time constant.

Assumption 4: Common trends for compliers and never takers under $z = 0$ and $d = 0$

$$E[Y_1(0,0)|n] - E[Y_0(0,0)|n] = E[Y_1(0,0)|c] - E[Y_0(0,0)|c]$$

Assumption 4 states that the difference in mean potential outcomes under $z = 0$ and $d = 0$ over time is identical for never takers and compliers or equivalently (by rearranging terms), that the difference in mean potential outcomes under $z = 0$ and $d = 0$ across compliers and never takers is constant over time. Similar to the common trend assumption in the standard DiD framework, Assumption 4 cannot directly be tested but can be scrutinized by placebo tests based on comparing the development of outcomes across groups with $Z = 1, D = 0$ (never takers) and $Z = 0, D = 0$ (never takers and compliers) in pre-treatment periods. Under Assumptions 1 to 4, the average direct effect on the never takers is identified based on four conditional means, as outlined in Theorem 1.[5] It also follows that our assumptions allow testing one implication of the instrumental variable exclusion restriction: If Z is a valid instrument for D and the parallel trend assumption holds, then θ_1^n must be equal to zero.

Theorem 1: Direct effect on the never takers

Under Assumptions 1 to 4, the average direct effect on the never takers is identified by a DiD approach among those with $D = 0$:

$$\begin{aligned}\theta_1^n = E[Y_1|Z = 1, D = 0] - E[Y_0|Z = 1, D = 0] \\ - \{E[Y_1|Z = 0, D = 0] - E[Y_0|Z = 0, D = 0]\}.\end{aligned}$$

Proof: See online appendix.

The remaining identification results presented in this section are based on stronger assumptions than those underlying Theorem 1. This implies that several restrictions cannot be scrutinized by any placebo tests as they are commonly used in the standard DiD framework. The next assumption

[5] From (A5) and (A6) in the online appendix follows that the sensitivity of θ_1^n to violations of Assumption 4 can be investigated by subtracting $\frac{p_c}{p_n+p_c} diff$ from the right hand side of the equation in Theorem 1, where $diff$ is the supposed difference in the trends of the potential outcomes between never takers and compliers.

imposes common trends in the potential outcomes of the always takers and compliers under $z = 1$ and $d = 1$.

Assumption 5: Common trends for compliers and always takers under $z = 1$ and $d = 1$

$$E[Y_1(1,1)|a] - E[Y_0(1,1)|a] = E[Y_1(1,1)|c] - E[Y_0(1,1)|c].$$

Assumption 5 appears somewhat harder to grasp than the restriction on potential outcomes under $z = 0$ and d=*0* imposed by Assumption 4.

Together with Assumption 3, which implies that $E[Y_0(1,1)|a] = E[Y_0(0,0)|a]$ and $E[Y_0(1,1)|c] = E[Y_0(0,0)|c]$, Assumption 5 either requires (i) that $E[Y_1(0,0)|a] - E[Y_0(0,0)|a] = E[Y_1(0,0)|c] - E[Y_0(0,0)|c]$ (common trend in mean potential outcomes under $z = 0$ and $d = 0$) and $E[Y_1(1,1)|a] - E[Y_1(0,0)|a] = E[Y_1(1,1)|c] - E[Y_1(0,0)|c]$ such that the mean effects of Z and D are homogeneous across strata, or (ii) that $[Y_1(0,0)|a] - E[Y_0(0,0)|a] \neq E[Y_1(0,0)|c] - E[Y_0(0,0)|c]$ and $E[Y_1(1,1)|a] - E[Y_1(0,0)|a] \neq E[Y_1(1,1)|c] - E[Y_1(0,0)|c]$ in a very specific way that satisfies Assumption 5. Case (ii) appears rather coincidental and hard to justify empirically, while case (i) has intuitive implications that may partially be checked in the data. Namely, the common trend assumption for always takers and compliers might be verified by placebo tests based on comparing the development of outcomes across groups with $Z = 0, D = 1$ (always takers) and $Z = 1, D = 1$ (always takers and compliers) in pre-treatment periods. The homogeneous effect assumption, on the other hand, is not testable. Under Assumptions 1, 2, 3, and 5, the direct effect on the always takers is identified. This yields another testable implication of the exclusion restriction for the standard LATE, namely that $\theta_1^a = 0$.

Theorem 2: Direct effect on the always takers

Under Assumptions 1, 2, 3, and 5, the average direct effect on the always takers is identified by a DiD approach among those with $D = 1$:

$$\theta_1^a = E[Y_1|Z = 1, D = 1] - E[Y_0|Z = 1, D = 1]$$
$$- \{E[Y_1|Z = 0, D = 1] - E[Y_0|Z = 0, D = 1]\}.$$

Proof: See online appendix.

Imposing all of Assumptions 1-5 identifies the average treatment effects on the compliers.

Theorem 3: Average treatment effect on the compliers

Under Assumptions 1 to 5,

$$\Delta_1^c = E[Y_1|Z = 1, D = 1] - E[Y_1|Z = 0, D = 0]$$
$$- \frac{p_{1|0}}{p_{1|1} - p_{1|0}} \{E[Y_0|Z = 0, D = 1] - E[Y_0|Z = 1, D = 1]\}$$
$$+ \frac{p_{0|1}}{p_{0|0} - p_{0|1}} \{E[Y_0|Z = 1, D = 0] - E[Y_0|Z = 0, D = 0]\}.$$

Proof: See online appendix.

In many empirical applications, assumption 5 is rather strong and unlikely to hold. In our empirical application, for example, military service for compliers (i.e., being drafted) and always takers (i.e. voluntarily joining the army) came with different terms with respect to service length, training or place of service (Angrist, 1991). Homogenous mean effects of Z and D across strata are thus unlikely to hold. Alternatively to identification based on Assumption 5, one may rule out a direct effect on the always takers per assumption.

Assumption 6: Zero direct effect on always takers

$$\theta_1^a = E[Y_1(1,1)|a] - E[Y_1(0,1)|a] = 0$$

Assumption 6 is an exclusion restriction as standardly used in the instrumental variable literature, however, with the difference that it is only imposed w.r.t. the stratum of always takers. This assumption seems plausible if exposure to treatment does not impose a change in behaviour – not only with respect to d, which is true for always takers by definition, but also with respect to any other potential channel that is subsumed into the direct effect. The assumption is violated, for example, if exposure to treatment causes a stigma to individuals who are exposed to treatment. In our application this seems to be a minor concern: always takers being exposed to treatment are not forced to change behaviour – in fact, the lottery outcome was irrelevant if a person planed joining the army in any way. Moreover, treatment status (or in other words the exact date of birth) was confidential so that any stigma effects seem unlikely. Assumption 6 allows identifying the total effect on the compliers.

Theorem 4: Average treatment effect on the compliers

Under Assumptions 1, 2, 3, 4, and 6,

$$\Delta_1^c = \frac{E[Y_1 D|Z=1] - E[Y_1 D|Z=0]}{p_{1|1} - p_{1|0}} - E[Y_1|Z=0, D=0]$$
$$+ \frac{p_{0|1}}{p_{0|0} - p_{0|1}} \{E[Y_0|Z=1, D=0] - E[Y_0|Z=0, D=0]\}.$$

Proof: See online appendix.

Assumptions 7 and 8 are further common trend assumptions that allow disentangling the total effect on the compliers into direct and indirect effects when combined with previous assumptions.

Assumption 7: Common trends for compliers and never takers under $z = 1$ and $d = 0$

$$E[Y_1(1,0)|n] - E[Y_0(1,0)|n] = E[Y_1(1,0)|c] - E[Y_0(1,0)|c]$$

Assumption 7 imposes a common trend restriction w.r.t. the potential outcomes of never takers and compliers under $z = 1$ and $d = 0$. Together with Assumptions 3 and 4, this implies that Z has a homogeneous direct effect across compliers and never takers for $d = 0$. To see this, first note that under Assumption 3, the expression in Assumption 7 becomes $E[Y_1(1,0)|n] - E[Y_0(0,0)|n] = E[Y_1(1,0)|c] - E[Y_0(0,0)|c]$. Subtracting from the right and left hand side of the latter expression the right and left hand side of Assumption 4, respectively, yields $E[Y_1(1,0)|n] - E[Y_1(0,0)|n] = E[Y_1(1,0)|c] - E[Y_1(0,0)|c]$. Assumption 7 is required for the identification of the direct effect under non-treatment and the indirect effect under treatment among compliers. For the latter effect, we derive the results by either imposing Assumption 5 (common trends for compliers and always takers under $z = 1$ and $d = 1$) or Assumption 6 (no direct effect on always takers). Assumption 7 can only be tested partially since only common trends prior to the treatment can be tested. It is not possible, however, to test for homogeneous direct treatment effects.

Theorem 5: Direct effect under $z = 0$ and indirect effect under $z = 1$ on compliers

i) Under Assumptions 1, 2, 3, 4, and 7,

$$\theta_1^c(0) = E[Y_1|Z = 1, D = 0] - E[Y_0|Z = 1, D = 0]$$
$$-\{E[Y_1|Z = 0, D = 0] - E[Y_0|Z = 0, D = 0]\}.$$

ii) Under Assumptions 1, 2, 3, 5, and 7,

$$\delta_1^c(1) = E[Y_1|Z=1,D=1]$$
$$-\frac{p_{1|0}}{p_{1|1}-p_{1|0}}\{E[Y_0|Z=0,D=1]-E[Y_0|Z=1,D=1]\}$$
$$-E[Y_1|Z=1,D=0]+E[Y_0|Z=1,D=0]$$
$$-\frac{E[Y_0(1-D)|Z=0]-E[Y_0(1-D)|Z=1]}{p_{0|0}-p_{0|1}}.$$

iii) Under Assumptions 1, 2, 3, 6, and 7,

$$\delta_1^c(1) = \frac{E[Y_1D|Z=1]-E[Y_1D|Z=0]}{p_{1|1}-p_{1|0}}$$
$$-E[Y_1|Z=1,D=0]+E[Y_0|Z=1,D=0]$$
$$-\frac{E[Y_0(1-D)|Z=0]-E[Y_0(1-D)|Z=1]}{p_{0|0}-p_{0|1}}.$$

Proofs: See online appendix.

Assumption 8: Common trends for compliers and always takers under $z=0$ and $d=1$

$$E[Y_1(0,1)|a]-E[Y_0(0,1)|a]=E[Y_1(0,1)|c]-E[Y_0(0,1)|c]$$

As an alternative to Assumption 7 we can also impose a common trend restriction w.r.t. potential outcomes of the always takers and compliers under $z=0$ and $d=1$. Similar to the discussion of Assumption 5, we note that when also invoking Assumption 3, Assumption 8 is satisfied if (i) $E[Y_1(0,0)|a]-E[Y_0(0,0)|a]=E[Y_1(0,0)|c]-E[Y_0(0,0)|c]$ (common trend in mean potential outcomes under $z=0$ and $d=0$) and $E[Y_1(0,1)|a]-E[Y_0(0,0)|a]=E[Y_1(0,1)|c]-E[Y_0(0,0)|c]$ (mean effect of D is homogeneous across strata),[6] or if (ii) $E[Y_1(0,0)|a]-E[Y_0(0,0)|a]\neq E[Y_1(0,0)|c]-E[Y_0(0,0)|c]$ and $E[Y_1(0,1)|a]-E[Y_0(0,0)|a]\neq E[Y_1(0,1)|c]-$

[6] As for Assumption 5, the common trend restriction is testable, while the homogeneous effect restriction is not.

$E[Y_0(0,0)|c]$ in a very specific way that satisfies Assumption 8. Assumption 8 therefore appears somewhat weaker than Assumption 5 when comparing case (i) of either assumption, as effect homogeneity is now only assumed w.r.t. *D* (rather than the joint effects of *D* and *Z*). However, Assumptions 5 and 8 are strictly speaking not nested, which becomes particularly obvious when comparing case (ii) of either assumption. Assumption 8 permits identifying the direct effect under treatment (when either imposing Assumption 5 or 6) and the indirect effect under non-treatment among compliers.

Theorem 6: Direct effect under $z = 1$ and indirect effect under $z = 0$ on compliers

i) Under Assumptions 1, 2, 3, 5, and 8,

$$\theta_1^c(1) = E[Y_1|Z=1, D=1]$$
$$-\frac{p_{1|0}}{p_{1|1} - p_{1|0}}\{E[Y_0|Z=0, D=1] - E[Y_0|Z=1, D=1]\}$$
$$-E[Y_1|Z=0, D=1] + E[Y_0|Z=0, D=1]$$
$$-\frac{E[Y_0(1-D)|Z=0] - E[Y_0(1-D)|Z=1]}{p_{0|0} - p_{0|1}}.$$

ii) Under Assumptions 1, 2, 3, 6, and 8,

$$\theta_1^c(1) = \frac{E[Y_1 D|Z=1] - E[Y_1 D|Z=0]}{p_{1|1} - p_{1|0}}$$
$$-E[Y_1|Z=0, D=1] + E[Y_0|Z=0, D=1]$$
$$-\frac{E[Y_0(1-D)|Z=0] - E[Y_0(1-D)|Z=1]}{p_{0|0} - p_{0|1}}.$$

iii) Under Assumptions 1, 2, 3, 4, and 8,

$$\delta_1^c(0) = E[Y_1|Z=0, D=1] - E[Y_0|Z=0, D=1]$$
$$-\{E[Y_1|Z=0, D=0] - E[Y_0|Z=0, D=0]\}.$$

Proofs: See online appendix.

We have demonstrated that direct and indirect effects can be identified for various subpopulations or principal strata under random treatment assignment and specific common trend assumptions that differ w.r.t. their strength. In particular, when several common trend assumptions need to be combined as it is the case for the compliers, identification only appears plausible if one can credibly assume homogeneity in average effects across strata. Whenever the principal strata-specific effects for all three strata (compliers, always takers, and never takers) are identified, so are the natural direct and indirect effects in the total population. This follows from an application of the law of total probability:

$$\theta_1(d) = p_c\theta_1^c(d) + p_a\theta_1^a(d) + p_n\theta_1^n(d) = [p_{1|1} - p_{1|0}]\theta_1^c(d) + p_{1|0}\theta_1^a(d) + p_{0|1}\theta_1^n(d),$$

$$\delta_1(d) = p_c\delta_1^c(d) + p_a0 + p_n0 = [p_{1|1} - p_{1|0}]\delta_1^c(d),$$

Note that under Assumption 6, $\theta_1^a(d) = 0$ such that the expression for $\theta_1(d)$ further simplifies.

3 Empirical application

During the Vietnam War the majority of American troops consisted of volunteers, while the rest were selected through a draft (Gimbel & Booth, 1996). Young men at age 18 had to register at local draft boards for classification. Initially, these boards determined medical fitness and also decided on the order in which registrants would be called. In an attempt to make the draft fair, a draft lottery was conducted in the years 1969 to 1972 to determine the order of call to military service for men born between 1944 and 1952. The lottery assigned a draft number to each birth date for men in certain age cohorts, where low draft numbers were called first up to a ceiling. In our application, respondents were exposed to the draft lottery taking place on July 1, 1970. It determined the order in which men born in 1951 were called to report for induction into the military in 1971. The ceiling of 125 was first announced in October 1971.

We analyse the impact of being assigned a low random draft lottery number on political preferences and attitudes, and to understand through which channels this effect materializes. This empirical application is motivated by the literature using the random draft lottery number as an instrument for military service (for example Angrist, 1990; Angrist, Chen, & Frandsen, 2010), while other authors argued that the possibility to receive a draft exemption induced individuals with a low draft number to enter college (Card & Lemieux, 2001; Deuchert & Huber, 2017). In our application, the indirect effect is the effect which goes through military service. We subsume all other effects into the direct effect.[7] These various effects are also interesting from a politico-economic perspective: In the political discussion and decision making process it seems useful to identify how different groups of the population potentially react to the treatment. On a more fundamental note, an impact of a change in public policy on individuals' political preferences contradicts the usual microeconomic assumption of stable preferences – we would have to reconsider standard economic models of politics.

Previous contributions studied the impact of the draft lottery on political preferences and attitudes towards the war. Bergan (2009) showed that a low draft lottery number increased the likelihood of people to favour an immediate withdrawal from Vietnam. Erikson and Stoker (2011) analysed the lottery's impact on young college bound males, which were especially vulnerable to the new draft policy. They found that the effect of the lottery number on political preferences and

[7] This can be for example the impact of induced college education, that may lead to more political participation (Dee, 2004; Milligan, Moretti, & Oreopoulos; Kam & Palmer, 2008; Milstein Sondheimer & Green, 2009) and affect political attitudes by increasing personal income (Morten, Tyran, & Wenström, 2011; Marshall, 2014), or the effect of leaving the country, even though this option was not used extensively. For a discussion on the estimated number of evaders leaving the country, see Baskir and Strauss (1978), Hagan (2001), or Jones (2005).

attitudes was strong. Young males with low draft numbers were more likely to favour the Democrats and had anti-war and liberal attitudes. The results, however, also showed that only about one third of respondents with low draft numbers actually served in the army.

These results illustrate important issues when analysing the effect of such a policy change: As a high proportion of respondents manged to avoid the draft, possibly due to a behavioural change, the ATE could be driven by different subpopulations, for example, by those who would only enlist when chosen by the lottery (compliers), or those who would not enlist whatever the lottery outcome (never takers). It is therefore important to distinguish between the effects of the policy intervention across these subgroups or strata.

3.1 Data

Our data comes from the "Young Men in High School and Beyond" (YESB) survey (Bachman, 1999), a five-wave longitudinal study among a national sample of male students who were in 10th grade in fall 1966. Information was collected in 1966 (wave 1), spring 1968 (at the end of eleventh grade, wave 2), spring 1969 (wave 3), June-July 1970 (wave 4), and spring 1974 (wave 5). We focus on the consequences of the draft lottery that took place on July 1, 1970.

The dataset is particularly suited for our research question for several reasons: (1) It contains a vast set of variables describing political preferences and attitudes before and after the lottery took place. (2) It is one of the very rare publicly available datasets that provides the exact birth date, which is necessary to link draft lottery numbers to individuals.[8] (3) Attrition is relatively low compared to many other longitudinal surveys – we observe almost 80% of the initial sample in wave 5. (4) Unlike many other surveys, the data also includes individuals serving in the military (if they can be located).

[8] Available from the Selective Service System: https://www.sss.gov/Portals/0/PDFs/1971.pdf

We use the subsample of respondents who were born in 1951 and who were not yet enlisted at the time of the data collection of wave 4 in 1970 (N = 849). We restrict the sample because the exact day of birth is only provided for respondents who participated in the fourth wave and did not serve in the military at the time of the interview. We do not use young males who were born in 1950 and before, because this cohort was exposed to the 1969 lottery and we cannot rule out that some respondents with low random draft numbers were already enlisted or drafted at the time of the interview. Since we select the base conditional on treatment, this could cause a selection bias (Deuchert & Huber, 2017). However, selection bias seems unlikely in our subsample no respondents were called for induction yet (inducement started in 1971) and the majority of interviews took place before the lottery so individuals were not aware of their random draft number.

Table 1: Descriptive statistics

Wave	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Mildly/strongly Republican	0.305	0.304	0.293	0.231	0.14
	(0.46)	(0.46)	(0.46)	(0.21)	(0.35)
Strongly Republican	0.107	0.078	0.081	0.044	0.025
	(0.31)	(0.27)	(0.27)	(0.21)	(0.16)
Mildly/strongly Democrat	0.396	0.377	0.392	0.337	0.296
	(0.49)	(0.48)	(0.49)	(0.47)	(0.46)
Strongly Democrat	0.148	0.098	0.154	0.098	0.087
	(0.36)	(0.30)	(0.36)	(0.30)	(0.28)

Notes: the columns report the means as well as the standard deviations (in parentheses).

Descriptive statistics for political preferences can be found in Table 1. They display some interesting patterns: Particularly in the last wave, the Republicans lost dramatically in electoral support, which most likely reflects the consequence of the Watergate scandal, with the Republican incumbent President, Richard Nixon, at centre stage.[9] Interestingly, the Democrats did not benefit from the scandal with higher rates of support. In the midst of the unfolding of the Watergate scandal Richard Nixon won his bid for re-election with a large margin against his Democratic rival, George McGovern. It was only after his re-election to a second term that President Nixon resigned in 1974 to prevent a likely impeachment.

Note that for all outcomes considered, there are no striking differences in pre-lottery outcomes between individuals with high and low RDN, indicating that selection bias is unlikely an issue in this application (see the results of the placebo estimates of the ATE in Table A1 in the online appendix).

3.2 Plausibility of the identifying assumptions

Our empirical application relies on a set of assumptions that are fairly standard in the empirical literature: Assumption 1 implies that there are no confounders jointly affecting the lottery outcome on the one hand and military service and/or the outcome variables on the other hand. This seems uncontroversial since the draft number was randomized and unlike the first lottery that had taken place in 1969, the randomization was well executed (Fienberg, 1971). Assumption 2 rules out the existence of defiers, which seems plausible in the context of the draft lottery. It appears difficult to argue why an individual should avoid the draft when being chosen by the lottery, but voluntarily join the army when not being chosen. Assumption 3 rules out anticipation effects of the treatment or the mediator w.r.t. to the outcome in the baseline period. Given the fact that the results of the lottery could not have been foreseen and that the large majority of interviews took place before the lottery, this assumption is also likely satisfied. Assumption 4 imposes common trends for compliers and never takers when receiving a high lottery number and not joining the army. This is a standard

[9] The Watergate Scandal refers to the political turmoil initiated by the break-in at the Democratic Parties headquarters, in which the incumbent Republican administration under President Richard Nixon was involved. The scandal ultimately led to the resignation of Richard Nixon in 1974.

restriction in the DiD literature, arguing that the mean outcomes of various groups develop in a comparable way if no one receives any treatment.

Assumption 1 and 3 are sufficient to estimate average treatment effects. The first column of Table A1 in the online appendix provides placebo estimates using wave 4 as placebo treatment period. The placebo effects are small and insignificant, demonstrating that the lottery was well executed and there is no selection bias. Assumptions 1 to 4 are sufficient to estimate the direct effect on the never takers. Our placebo estimations compare the development of outcomes across groups with $Z = 1, D = 0$ (never takers) and $Z = 0, D = 0$ in pre-treatment periods (second column of Table A1 in the online appendix). Again, placebo effects are small and insignificant for all outcomes and therefore support our strategy.[10]

Our theoretical discussion proposes two different possibilities to estimate the total effect for compliers. Assumption 5 implies that the joint average effect of the lottery and military service was comparable across individuals voluntarily joining the army (always takers) or being induced to join (compliers). This seems to be a very strong assumption given the fact that military terms were different for individuals who voluntarily joined the army and those who were drafted. We find Assumption 6 more credible (zero direct effect on always takers) since always takers were not forced to change their behaviour because they would have joined the army anyway. Given that a stigma effect is highly unlikely, the lottery outcome itself was thus irrelevant for always takers. Under Assumptions 1 to 4, and 6, we can estimate the total treatment effect on the compliers. We also conduct placebo estimations of the total treatment effect on compliers (Table A1 in the online appendix) that support our strategy.

[10] The online appendix also provides a graph displaying the evolution of average pre-treatment outcomes among compliers and never takers between waves 3 and 4, see Figure A.1.

Finally, there are two ways of decomposing the direct and indirect effects: Assumption 7 identifies the indirect effect of joining the army among compliers when having a low random number ($z = 1$) while Assumption 8 identifies the indirect effect of joining the army when compliers receive a high random number ($z = 0$). We find the latter effect rather hypothetical and of very little practical importance since compliers would never join the army if not induced to by the lottery. We therefore do not adopt the identification strategy related to Assumption 8. In contrast, Assumption 7 imposes a homogenous direct treatment effect for compliers and never takers in a hypothetical world where compliers would not comply with the treatment. We thus assume that compliers would adopt exactly the same draft avoiding strategy (for example going to college or leaving the country) as never-takers and assume that this draft avoiding strategy would have an identical impact on political preferences.

3.3 Average treatment effect

In the following we estimate the effect of a low draft lottery number on party preferences. In the first step we use the experimental estimator, i.e. mean differences in outcomes across treatment states, to evaluate the ATE. Table 2 presents the results, where the binary treatment is equal to one if the random draft number was below the ceiling. Individuals with lottery numbers below the cut-off are about 5% more likely to report to mildly or strongly favour Republicans. These are, however, likely to be swing voters, since we find no effect on strong preferences for Republicans. Moreover, we do not find any effect on preferences for the Democratic Party. A low lottery number has also no impact on higher scepticism towards the government or on policy contents (such as the Vietnam War or the Civil Rights movement, see Table A2 of the online appendix).

Table 2: Average treatment effects

	ATE
Mildly/strongly Republican	0.056**
	(0.027)
Strongly Republican	0.005
	(0.012)
Mildly/strongly Democrat	0.009
	(0.032)
Strongly Democrat	0.008
	(0.021)

Notes: Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results are in stark contrast to those of Bergan (2009) and Erikson and Stoker (2011) who generally found a quite substantial positive effect on preferences for Democrats. Erikson and Stoker (2011) found that young men with low lottery numbers were more likely to report to favour Democrats over Republicans. Specifically, they showed that individuals with low lottery numbers held more anti-war attitudes; voted more often for McGovern (Democrat) relative to Nixon (Republican); favoured Democrats over Republicans in a rating of attitudes towards Nixon vs. McGovern; favoured Democrats in partisan activities, in a composite issue attitude index, and in political ideology showing preferences for liberal relative to conservative positions. Bergan (2009) reported a significantly positive effect of the lottery on the probability of favouring an immediate withdrawal from Vietnam.

The differences of our results with respect to the study by Bergan (2009) and Erikson and Stoker (2011) may be explained by the sample selection process: Bergan (2009) focused on a small sample of university students in1972 and tested the impact of having a low lottery number while they were still in college. Once these students graduated they had no further possibility to receive a deferment. Erikson and Stoker (2011) focused on individuals with birth years around 1947 whose high school curriculum was college preparatory. It is thus very likely that many individuals in their dataset had entered college shortly after completing high school in 1965 and graduated in 1969 at the time of the first draft. They were thus at risk to be drafted for military without the possibility to receive a further deferment, as deferments for graduate studies were eliminated already in 1967. In both samples, respondents with low random draft numbers had a high draft risk without having the possibility to escape without leaving the country. In our sample, in contrast, individuals just completed high school and, at the time, could still receive a college deferment (which continued to be issued until 1971). Therefore, in the sample of Erikson and Stoker individuals were basically forced to be compliers (or forced to leave the country), while in our sample, individuals could choose to be a complier – at least to some extent. For this reason, effect heterogeneities across strata may be important. In the following we distinguish between different strata and estimate direct and indirect effects of the draft lottery.

3.4 Strata proportions and description

In the first stage, we estimate the impact of a RDN below the ceiling on veteran status (as reported in 1971) and describe the different strata w.r.t. their political preferences and attitudes – measured before the lottery took place.

As is shown in Table 3, the lottery shifted the likelihood of military service by more than 20 percentage points, which corresponds to the share of compliers. This seems relatively small at first glance but can be explained by the fact that a high share of our sample already held a college deferment before the lottery took place. About 5% of the population voluntarily joined the army even though they were not obliged to (always takers). Note that this does not correspond to the share of all individuals who voluntarily joined the army for two reasons: First, people who voluntarily enlisted before the lottery took place are not included in our sample since we cannot match the random draft number with the birth date. Second, a low draft number may have induced some men to enlist pre-emptively (Angrist, 1991). Because our mediator of interest is military service – no matter whether individuals joined voluntarily or were drafted – these pre-emptive enlistments are considered as compliers. The vast majority of the population are never takers (74%) who avoided the draft even with a RDN below the ceiling of 125 – either because they were ineligible or because they already had or applied for a deferment.

Table 3: First stage results

	coef.
RDN<126 (complier)	0.207***
	(0.026)
Constant (always taker)	0.05***
	(0.009)

Notes: Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In order to better understand the characteristics of the relevant groups in our population, Table A3 of the online appendix displays strata differences in pre-treatment characteristics that are estimated based on equations (A5) and (A16) in the online appendix. While the groups do not seem to differ in their knowledge about military life, compliers (C) had significantly lower academic skills measured in terms of an IQ-test, and were less likely to have college plans than never takers (NT). Consequently compliers were less likely to hold a student deferment shortly before or at the draft lottery, and were more likely available for the military than never takers. No statistically significant differences can be observed for always takers (AT). Even though the groups differ with respect to academic skills and college aspiration, Table A4 in the online appendix shows that the strata are very similar in terms of pre-treatment political preferences prior to the lottery.

3.5 Decomposition of the average treatment effect

In the following we decompose the ATEs displayed in Table 2 into strata-specific direct and indirect effects to understand which channels drive the overall findings. The results are displayed in Table 4. The reported standard errors are obtained based on 1999 bootstrap replications and take account of clustering on the individual level across time periods.

We use the results of Theorem 1 (Assumptions 1 to 4) to estimate the direct effects of the lottery on the never takers (second column of table 4). The direct effects on the preferences of never takers to at least mildly favour either the Republican or the Democratic Parties are with 5.5 and 4.9 percentage points, respectively, both positive and sizable, but only statistically significant for the Republicans. One possible interpretation for the significant direct effect for never takers with respect to mild preferences for Republicans is that Nixon already abolished the draft when the subjects were interviewed in the post-treatment period (1974, wave 5), so that never takers were finally safe from being inducted in the military. Hence, never takers were actually successful in avoiding the draft, which might have encouraged them to favour the government which let them from the hook. However, these effects were not very strong as no strong party preference shifts can be observed. We also find no significant effects on general attitudes towards government, or attitudes towards the Vietnam War, or the civil right movements (see Table A2 in the online appendix). This is suggesting that the draft itself, draft induced military service, or draft avoiding behaviour had little impact on political preferences. These results are in line with standard microeconomic theory in which preferences are fairly stable.[11]

Column three to five report the estimated total treatment effects on the compliers based on Theorem 4 (Assumptions 1 to 4, and 6), as well as the direct effects under non-treatment and indirect effects under treatment based on Theorem 5 (Assumptions 1 to 4, 6, and 7). Note that as a result of our identifying assumptions, the direct effect on the compliers and never takers are identical. The point estimates on the total effect for compliers point towards a higher support for Republicans and a lower support for Democrats, where the latter effect seems to be driven by a larger indirect effect of the lottery. However, neither the total effects nor the decomposed indirect effects reach conventional levels of statistical significance.

We also compare our result with the two stage least squares estimate for the LATE (using analytical standard errors). In the context of the Vietnam draft lottery, the LATE attempts to measure the complier effect of joining the army (for example Angrist, 1990; Angrist, Chen, & Frandsen, 2010), which corresponds to the indirect effect among compliers, as the first stage among compliers is one per definition of compliance. The LATE suggests a significant 27 percentage points increase in (at least) mild support for Republicans, while the estimate of the indirect effect is ten times lower and not significant at any conventional level of statistical significance. As shown in the last column of Table 4, the difference between the LATE and indirect effect estimates is significant at the 10% level. The results are therefore not robust across our method based on common trend and homogeneity assumptions and the instrumental variable approach used elsewhere in the literature.

[11] Note that stated party preferences could still change as a result of evolving party positions. Such changes in stated party preferences are then the result of an updating process based on the information on particular party positions and not a sign of endogenous preferences.

Table 4: Estimating direct effect and indirect effects

	Direct effect on NT	Total effect on C	Direct effect on C (Z=0)	Indirect effect on C (Z=1)	LATE	Test: LATE = indirect effect on C (Z=1)
Mildly/strongly Republican	0.055*	0.081	0.055*	0.027	0.268**	-0.241*
	(0.030)	(0.144)	(0.030)	(0.160)	(0.135)	(0.145)
Strongly Republican	-0.005	0.047	-0.005	0.052	0.026	0.026
	(0.018)	(0.073)	(0.018)	(0.084)	(0.060)	(0.083)
Mildly/strongly Democrat	0.049	-0.116	0.049	-0.164	0.042	-0.206
	(0.041)	(0.159)	(0.041)	(0.183)	(0.162)	(0.199)
Strongly Democrat	0.023	-0.040	0.023	-0.063	0.036	-0.099
	(0.027)	(0.100)	(0.027)	(0.115)	(0.102)	(0.123)

Notes: NT stands for "never takers", C stands for "compliers". Standard errors in parentheses (column 2 to 5; 7: based on 1999 bootstrap replications and take account of clustering on the individual level across time periods; column 6: analytical standard errors), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4 Conclusion

We propose a difference-in-differences approach to disentangle the total effect of a randomly assigned treatment within subpopulations (or strata) into a direct effect and an indirect effect operating through a binary intermediate variable (or mediator). The strata are defined upon how the mediator reacts to the treatment. We show under which assumptions the direct effects on the always and never takers (whose mediator is not affected by the treatment) as well as the direct and indirect effects on the compliers (whose mediator reacts to the treatment) are identified.

We apply our method to investigate the effects of the Vietnam draft lottery in the US on political preferences and attitudes towards the government or the Vietnam War. Our mediator of interest is military service during the Vietnam War. A subgroup of individuals (the compliers) was induced by the lottery to serve in the army, while others avoided the draft (the never takers) or would have served in any case (the always takers). In a first step, we estimate the average treatment effect (ATE) in the total population and find a 5.6 percentage points higher probability of at least mildly favouring the Republican Party. In a second step, we estimate the direct and indirect effects of the draft lottery within subgroups. We find a significant direct effect on never takers and compliers, which increased the probability by about 5.5 percentage points to at least mildly favour the Republicans. Indirect effects are insignificant and much smaller than the two stage least squares estimates of the LATE which uses the lottery as an instrument for military service.

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ONLINE APPENDIX

Direct and indirect effects based on difference-in-differences with an application to political preferences following the Vietnam draft lottery

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Abstract: This paper proposes a difference-in-differences approach for disentangling a total treatment effect on some outcome into a direct effect as well as an indirect effect operating through a binary intermediate variable – or mediator – within strata defined upon how the mediator reacts to the treatment. Imposing random treatment assignment along with specific common trend (and further) assumptions identifies the direct effects on the always and never takers, whose mediator is not affected by the treatment, as well as the direct and indirect effects on the compliers, whose mediator reacts to the treatment. We provide an empirical application based on the Vietnam draft lottery, where we analyse the impact of the random draft lottery number on political preferences. The results suggest that a high draft risk due to the lottery leads to a relative increase in mild preferences for the Republican Party, but has no effect on strong preferences for either party or on policy contents. Moreover, the increase in Republican support is mostly driven by the direct effect.

Keywords: treatment effects, causal mechanisms, direct and indirect effects, Vietnam War lottery, political preferences

JEL classification: C21, C22, D70, D72

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Proof of Theorem 1: Direct effect on never takers

We denote by $p_\tau = \Pr(\tau)$ the share of a particular type in the population and by $p_{d|z} = \Pr(D = d \mid Z = z)$ the conditional probability of a particular mediator state given the treatment, with d, z in *{1,0}*. By Assumption 1, the share of a type conditional on Z corresponds to p_τ (in the population), as Z is randomly assigned. Likewise, $E\left[Y_t(z,d) \mid \tau, Z=1\right] = E\left[Y_t(z,d) \mid \tau, Z=0\right] = E\left[Y_t(z,d) \mid \tau\right]$ due to the independence of Z and the potential outcomes as well as the types (which are a deterministic function of $D(z)$). It follows that conditioning on Z is not required on the right hand side of the following equation, which expresses the mean outcome given Z=0 and D=0 as weighted average of the mean potential outcomes of compliers and never takers, the two types satisfying $D(0)$=0 and thus making up the group with Z=0 and D=0:

$$E(Y_t \mid Z=0, D=0) = \frac{p_n}{p_n + p_c} E\left[Y_t(0,0) \mid n\right] + \frac{p_c}{p_n + p_c} E\left[Y_t(0,0) \mid c\right]. \tag{A1}$$

After some rearrangements we obtain

$$E\left[Y_t(0,0) \mid n\right] - E\left[Y_t(0,0) \mid c\right] = \frac{p_n + p_c}{p_c}\left\{E\left[Y_t(0,0) \mid n\right] - E(Y_t \mid Z=0, D=0)\right\}. \tag{A2}$$

Next, consider observations with Z=1 and D=0 who might consist of both never takers and defiers, as $D(1)$=0 for both types. However, by Assumption 2, defiers are ruled out, such that the mean outcome given Z=1 and D=0 is determined by never takers only:

$$E(Y_t \mid Z=1, D=0) = E\left[Y_t(1,0) \mid n\right]. \tag{A3}$$

Furthermore, by Assumption 3,

$$E\left[Y_0(0,0) \mid n\right] = E\left[Y_0(1,0) \mid n\right] = E(Y_0 \mid Z=1, D=0). \tag{A4}$$

It follows that when considering (A2) in period T=0, $E\left[Y_0(0,0)\mid n\right]$ on the right hand side of the equation may be replaced by $E(Y_0 \mid Z=1, D=0)$:

$$E\left[Y_0(0,0)\mid n\right]-E\left[Y_0(0,0)\mid c\right]=\frac{p_n+p_c}{p_c}\left\{E(Y_0 \mid Z=1,D=0)-E(Y_0 \mid Z=0,D=0)\right\}. \quad \text{(A5)}$$

Let us now consider (A1) in period $T=1$:

$$\begin{aligned}
&E(Y_1 \mid Z=0,D=0)=\frac{p_n}{p_n+p_c}E\left[Y_1(0,0)\mid n\right]+\frac{p_c}{p_n+p_c}E\left[Y_1(0,0)\mid c\right]\\
&\Leftrightarrow E(Y_1 \mid Z=0,D=0)=E\left[Y_1(0,0)\mid n\right]-\frac{p_c}{p_n+p_c}\left\{E\left[Y_1(0,0)\mid n\right]-E\left[Y_1(0,0)\mid c\right]\right\} \quad \text{(A6)}\\
&\Leftrightarrow E\left[Y_1(0,0)\mid n\right]=E(Y_1 \mid Z=0,D=0)+\frac{p_c}{p_n+p_c}\left\{E\left[Y_1(0,0)\mid n\right]-E\left[Y_1(0,0)\mid c\right]\right\}.
\end{aligned}$$

By Assumption 4, we may replace $E\left[Y_1(0,0)\mid n\right]-E\left[Y_1(0,0)\mid c\right]$ in (A6) by the right hand side of (A5), which gives

$$E\left[Y_1(0,0)\mid n\right]=E(Y_1 \mid Z=0,D=0)+E(Y_0 \mid Z=1,D=0)-E(Y_0 \mid Z=0,D=0). \quad \text{(A7)}$$

Finally, using (A3) in period T=1 and subtracting (A7) yields the identification result based on differences in differences:

$$\begin{aligned}
\theta_1^n &= E\left[Y_1(1,0)\mid n\right]-E\left[Y_1(0,0)\mid n\right]\\
&=E(Y_1 \mid Z=1,D=0)-\left[E(Y_1 \mid Z=0,D=0)+E(Y_0 \mid Z=1,D=0)-E(Y_0 \mid Z=0,D=0)\right] \quad \text{(A8)}\\
&=\left[E(Y_1 \mid Z=1,D=0)-E(Y_0 \mid Z=1,D=0)\right]-\left[E(Y_1 \mid Z=0,D=0)-E(Y_0 \mid Z=0,D=0)\right].
\end{aligned}$$

Testable implication of Assumptions 1 to 3:

We consider (A1) for period T=0 and replace $E\left[Y_0(0,0)\mid n\right]$ by $E(Y_0 \mid Z=1,D=0)$ as suggested in (A4):

$$E(Y_0 \mid Z=0, D=0) = \frac{p_n}{p_n + p_c} E(Y_0 \mid Z=1, D=0) + \frac{p_c}{p_n + p_c} E\left[Y_0(0,0) \mid c\right]. \tag{A9}$$

Under Assumptions 1 and 2, $p_n + p_c = p_{0|0}$, $p_n = p_{0|1}$ and $p_c = p_{0|0} - p_{0|1}$, which corresponds to the (first stage) effect of *Z* on *D*. Therefore, $E\left[Y_0(0,0) \mid c\right]$ is identified when plugging the latter probabilities into (A9):

$$\begin{aligned}
E(Y_0 \mid Z=0, D=0) &= \frac{p_{0|1}}{p_{0|0}} E(Y_0 \mid Z=1, D=0) + \frac{p_{0|0} - p_{0|1}}{p_{0|0}} E\left[Y_0(0,0) \mid c\right] \\
\Leftrightarrow E\left[Y_0(0,0) \mid c\right] &= \frac{p_{0|0}}{p_{0|0} - p_{0|1}} E(Y_0 \mid Z=0, D=0) - \frac{p_{0|1}}{p_{0|0} - p_{0|1}} E(Y_0 \mid Z=1, D=0) \qquad \text{(A10)} \\
&= \frac{E(Y_0(1-D) \mid Z=0) - E(Y_0(1-D) \mid Z=1)}{p_{0|0} - p_{0|1}}.
\end{aligned}$$

Similarly to (A1) for the never takers and compliers, consider the mean outcome given *Z*=1 and *D*=1, which is made up by always takers and compliers (the types with *D*(1)=1)

$$E(Y_t \mid Z=1, D=1) = \frac{p_a}{p_a + p_c} E\left[Y_t(1,1) \mid a\right] + \frac{p_c}{p_a + p_c} E\left[Y_t(1,1) \mid c\right]. \tag{A11}$$

In analogy to (A10), one can show that under Assumptions 1 to 3,

$$E\left[Y_0(1,1) \mid c\right] = \frac{E(Y_0 D \mid Z=1) - E(Y_0 D \mid Z=0)}{p_{1|1} - p_{1|0}}. \tag{A12}$$

Under the validity of Assumptions 1 to 3, (A10) and (A12) must be identical. It is easy to show (based on counter-probabilities) that the denominator on the right hand side of (A12), $p_{1|1} - p_{1|0}$, is equal to that in the last line in (A10), $p_{0|0} - p_{0|1}$. It therefore also follows that the respective denominators must be equal under Assumptions 1 to 3, which implies:

$$
\begin{aligned}
&E(Y_0 D \mid Z=1) - E(Y_0 D \mid Z=0) = E(Y_0(1-D) \mid Z=0) - E(Y_0(1-D) \mid Z=1) \\
&\Leftrightarrow E(Y_0 D \mid Z=1) + E(Y_0(1-D) \mid Z=1) - E(Y_0 D \mid Z=0) - E(Y_0(1-D) \mid Z=0) = 0 \quad \text{(A13)} \\
&\Leftrightarrow E(Y_0 \mid Z=1) - E(Y_0 \mid Z=0) = 0
\end{aligned}
$$

Proof of Theorem 2: Identification of direct effect on always takers

From rearranging (A11) follows that

$$
E\left[Y_t(1,1) \mid a\right] - E\left[Y_t(1,1) \mid c\right] = \frac{p_a + p_c}{p_c}\left\{E\left[Y_t(1,1) \mid a\right] - E(Y_t \mid Z=1, D=1)\right\}. \quad \text{(A14)}
$$

By Assumptions 1 and 2,

$$
E\left[Y_0(0,1) \mid a\right] = E(Y_0 \mid Z=0, D=1)\,. \quad \text{(A15)}
$$

Now consider (A14) for period T=0, and note that by Assumption 3,

$E\left[Y_0(1,1) \mid a\right] = E\left[Y_0(0,0) \mid a\right] = E\left[Y_0(0,1) \mid a\right]$ (and $E\left[Y_0(1,1) \mid c\right] = E\left[Y_0(0,0) \mid c\right]$), such that we may plug the right hand side of (A15) into (A14) to obtain

$$
\begin{aligned}
&E\left[Y_0(0,0) \mid a\right] - E\left[Y_0(0,0) \mid c\right] = E\left[Y_0(0,1) \mid a\right] - E\left[Y_0(1,1) \mid c\right] \\
&= \frac{p_a + p_c}{p_c}\left\{E(Y_0 \mid Z=0, D=1) - E(Y_0 \mid Z=1, D=1)\right\}.
\end{aligned} \quad \text{(A16)}
$$

Considering (A11) for period T=1 and performing some rearrangements yields

$$
E\left[Y_1(1,1) \mid a\right] = E(Y_1 \mid Z=1, D=1) + \frac{p_c}{p_a + p_c}\left\{E\left[Y_1(1,1) \mid a\right] - E\left[Y_1(1,1) \mid c\right]\right\}. \quad \text{(A17)}
$$

By Assumption 5, $E\left[Y_1(1,1) \mid a\right] - E\left[Y_1(1,1) \mid c\right]$ in (A17) may be replaced by the right hand side of (A16) which gives

$$
E\left[Y_1(1,1) \mid a\right] = E(Y_1 \mid Z=1, D=1) + E(Y_0 \mid Z=0, D=1) - E(Y_0 \mid Z=1, D=1) \quad \text{(A18)}
$$

Finally, acknowledging that $E[Y_1(0,1)|a] = E(Y_1|Z=0,D=1)$ by Assumptions 1 and 2 and subtracting (A18) yields the identification result based on differences in differences:

$$\begin{aligned}
\theta_1^a &= E[Y_1(1,1)|a] - E[Y_1(0,1)|a] \\
&= E(Y_1|Z=1,D=1) + E(Y_0|Z=0,D=1) - E(Y_0|Z=1,D=1) - E(Y_1|Z=0,D=1) \qquad \text{(A19)} \\
&= [E(Y_1|Z=1,D=1) - E(Y_0|Z=1,D=1)] - [E(Y_1|Z=0,D=1) - E(Y_0|Z=0,D=1)].
\end{aligned}$$

Proof of Theorem 3: Identification of ATE on compliers under Assumptions 1 to 5

Using Assumptions 1 to 4, we plug in the expression on the right hand side of (A7), which identifies $E[Y_1(0,0)|n]$, into (A1) for period T=1, which allows identifying $E[Y_1(0,0)|c]$ (when also using $p_n + p_c = \Pr(D=0|Z=0)$ and $p_n = \Pr(D=0|Z=1)$):

$$\begin{aligned}
E(Y_1|Z=0,D=0) &= \frac{p_{0|1}}{p_{0|0}}[E(Y_1|Z=0,D=0) + E(Y_0|Z=1,D=0) - E(Y_0|Z=0,D=0)] \\
&\quad + \frac{p_{0|0} - p_{0|1}}{p_{0|0}} E[Y_1(0,0)|c] \\
\Leftrightarrow E[Y_1(0,0)|c] &= E(Y_1|Z=0,D=0) - \frac{p_{0|1}}{p_{0|0} - p_{0|1}}[E(Y_0|Z=1,D=0) - E(Y_0|Z=0,D=0)].
\end{aligned}$$

(A19)

Using Assumptions 1, 2, 3, and 5, we plug in the expression on the right hand side of (A18), which identifies $E[Y_1(1,1)|a]$, into (A11) for period T=1, which allows identifying $E[Y_1(1,1)|c]$ (when also using $p_a + p_c = \Pr(D=1|Z=1)$ and $p_a = \Pr(D=1|Z=0)$):

$$E(Y_1 \mid Z=1, D=1) = \frac{p_{1|0}}{p_{1|1}}\left[E(Y_1 \mid Z=1, D=1) + E(Y_0 \mid Z=0, D=1) - E(Y_0 \mid Z=1, D=1)\right]$$
$$+\frac{p_{1|1}-p_{1|0}}{p_{1|1}} E\left[Y_1(1,1) \mid c\right]$$
$$\Leftrightarrow E\left[Y_1(1,1) \mid c\right] = E(Y_1 \mid Z=1, D=1) - \frac{p_{1|0}}{p_{1|1}-p_{1|0}}\left[E(Y_0 \mid Z=0, D=1) - E(Y_0 \mid Z=1, D=1)\right]. \tag{A20}$$

Subtracting (A19) from (A20) yields Δ_1^c.

Proof of Theorem 4: Identification of ATE on compliers under Assumptions 1, 2, 3, 4, and 6

Note that the identification result for $E\left[Y_1(0,0) \mid c\right]$ given in (A19) based on Assumptions 1 to 4 remains unchanged. Concerning $E\left[Y_1(1,1) \mid c\right]$, reconsider (A11) for period *T*=1 and note that under Assumptions 1, 2, and 6, $E\left[Y_1(1,1) \mid a\right] = E\left[Y_1(0,1) \mid a\right] = E(Y \mid Z=0, D=1)$, which suffices for identification:

$$E(Y_1 \mid Z=1, D=1) = \frac{p_{1|0}}{p_{1|1}} E(Y \mid Z=0, D=1) + \frac{p_{1|1}-p_{1|0}}{p_{1|1}} E\left[Y_1(1,1) \mid c\right]$$
$$\Leftrightarrow E\left[Y_1(1,1) \mid c\right] = \frac{E(Y_1 D \mid Z=1) - E(Y_1 D \mid Z=0)}{p_{1|1}-p_{1|0}}. \tag{A21}$$

Subtracting (A19) from (A21) yields Δ_1^c.

Proof of Theorem 5: Direct effects under *z* = 0 and indirect effect under *z* = 1 on compliers

Identification of $\theta_1^c(0) = E\left[Y_1(1,0) \mid c\right] - E\left[Y_1(0,0) \mid c\right]$ under Assumptions 1, 2, 3, 4, and 7 and $\delta_1^c(1) = E\left[Y_1(1,1) \mid c\right] - E\left[Y_1(1,0) \mid c\right]$ under Assumptions 1 ,2, 3, 5, and 7 or Assumptions 1, 2, 3, 6, and 7, respectively:

Note that similarly as in (A4) for the never takes, under Assumptions 1 to 3 it holds for the compliers that

$$E\left[Y_0(1,0)\,|\,c\right]=E\left[Y_0(0,0)\,|\,c\right]=\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}},$$

where the second equality follows from (A10). Considering Assumption 7, it therefore follows that

$$\begin{aligned}
&E\left[Y_1(1,0)\,|\,n\right]-E\left[Y_0(1,0)\,|\,n\right]=E\left[Y_1(1,0)\,|\,c\right]-E\left[Y_0(1,0)\,|\,c\right]=\\
&E(Y_1\,|\,Z=1,D=0)-E(Y_0\,|\,Z=1,D=0)=E\left[Y_1(1,0)\,|\,c\right]-\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}}\\
&\Leftrightarrow E\left[Y_1(1,0)\,|\,c\right]=E(Y_1\,|\,Z=1,D=0)-E(Y_0\,|\,Z=1,D=0)+\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}},
\end{aligned} \tag{A22}$$

where we also made use of $E\left[Y_t(1,0)\,|\,n\right]=E(Y_t\,|\,Z=1,D=0)$. It follows that $\theta_1^c(0)$ is identified as the difference of (A22) and (A19) under Assumptions 1, 2, 3, 4 and 7, which simplifies to the expression in Theorem 5 i). Furthermore, $\delta_1^c(1)$ is identified as the difference of (A20) and (A22) under Assumptions 1, 2, 3, 5, and 7. Finally, $\delta_1^c(1)$ is identified as the difference of (A21) and (A22) under Assumptions 1, 2, 3, 6, and 7.

Proof of Theorem 6: Direct effects under z = 1 and indirect effect under z = 0 on compliers

Identification of $\delta_1^c(0)=E\left[Y_1(0,1)\,|\,c\right]-E\left[Y_1(0,0)\,|\,c\right]$ under Assumptions 1, 2, 3, 4, and 8 and $\theta_1^c(1)=E\left[Y_1(1,1)\,|\,c\right]-E\left[Y_1(0,1)\,|\,c\right]$ under Assumptions 1, 2, 3, 5, and 8 or Assumptions 1, 2, 3, 6, and 8, respectively:

Under Assumptions 1 to 3 it holds for the compliers that

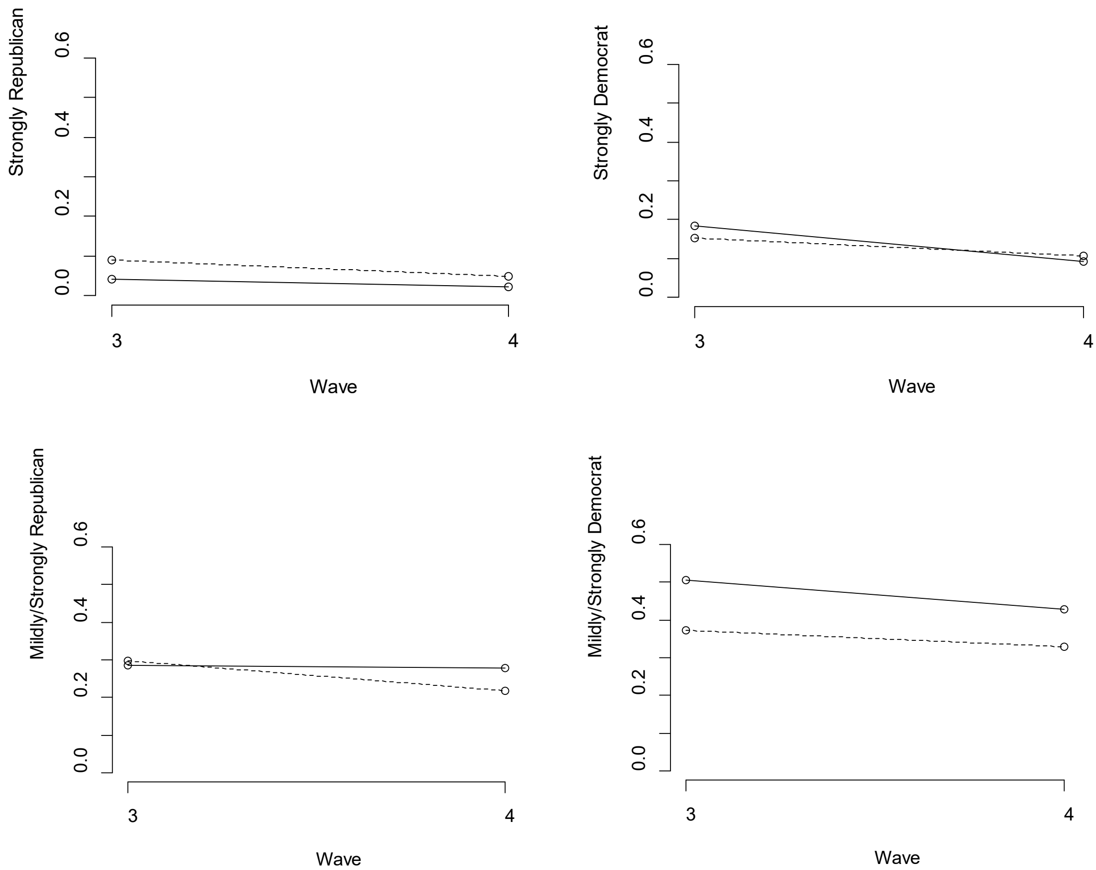
$$E\left[Y_0(0,1)\,|\,c\right]=E\left[Y_0(0,0)\,|\,c\right]=\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}},$$

where the second equality follows from (A10). Considering Assumption 8, it therefore follows that

$$E\left[Y_1(0,1)\,|\,a\right]-E\left[Y_0(0,1)\,|\,a\right]=E\left[Y_1(0,1)\,|\,c\right]-E\left[Y_0(0,1)\,|\,c\right]=$$
$$E(Y_1\,|\,Z=0,D=1)-E(Y_0\,|\,Z=0,D=1)=E\left[Y_1(0,1)\,|\,c\right]-\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}}$$
$$\Leftrightarrow E\left[Y_1(0,1)\,|\,c\right]=E(Y_1\,|\,Z=0,D=1)-E(Y_0\,|\,Z=0,D=1)+\frac{E(Y_0(1-D)\,|\,Z=0)-E(Y_0(1-D)\,|\,Z=1)}{p_{0|0}-p_{0|1}},\tag{A23}$$

where we also made use of $E\left[Y_t(0,1)\,|\,a\right]=E(Y_t\,|\,Z=0,D=1)$. It follows that $\delta_1^c(0)$ is identified as the difference of (A23) and (A19) under Assumptions 1, 2, 3, 4, and 8, which simplifies to the expression in Theorem 6 iii). Furthermore, $\theta_1^c(1)$ is identified as the difference of (A20) and (A23) under Assumptions 1, 2, 3, 5, and 8. Finally, $\theta_1^c(1)$ is identified as the difference of (A21) and (A23) under Assumptions 1, 2, 3, 6, and 8.

Figure A1: Pre-treatment outcomes among compliers and never takers (waves 3 and 4)



Notes: The solid lines (dashed lines) correspond to the respective evolutions of average pre-treatment outcomes among compliers (never takers).

Table A1: Placebo results (wave 4= placebo treatment period, wave 3= pre-treatment period)

	ATE	Direct effect never taker	Total effect complier	Indirect effect complier (Z=1)
Testing	Ass. 1 + 3	Ass. 1 to 4	Ass. 1 to 4 + 6	Ass. 1 to 4 + 7
Strongly Republican	0.01	-0.005	0.066	0.072
	(0.016)	(0.022)	(0.098)	(0.114)
Strongly Democrat	-0.005	0.01	-0.068	-0.078
	(0.022)	(0.028)	(0.122)	(0.14)
Mildly/Strongly Republican	0.009	-0.015	0.087	0.102
	(0.031)	(0.033)	(0.165)	(0.182)
Mildly/Strongly Democrat	-0.018	0.01	-0.142	-0.152
	(0.035)	(0.04)	(0.173)	(0.193)

Notes: Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Effects on additional outcomes

	ATE	DE never taker	TE complier	DE complier (Z=0)	IE complier (Z=1)	LATE	Test LATE = IE complier
Skeptical attitudes towards the government (max 15)	0.044	-0.004	0.191	-0.004	0.195	0.199	-0.005
	(0.126)	(0.156)	(0.615)	(0.156)	(0.699)	(0.599)	(0.709)
Skeptical attitudes towards Vietnam War (max 24)	-0.069	0.244	-1.4	0.244	-1.644	-0.314	-1.33
	(0.255)	(0.273)	(1.248)	(0.273)	(1.386)	(1.195)	(1.278)
Preferences for civil rights interventions (max 12)	-0.112	-0.103	-0.152	-0.103	-0.049	-0.514	0.465
	(0.125)	(0.171)	(0.57)	(0.171)	(0.667)	(0.587)	(0.774)

Note: DE stands for direct effect, IE for indirect effect, and TE for total effect. Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The indices are constructed as follows:

Skeptical attitudes towards the government (sums of three items, max 15): “Do you think the government wastes much of the money we pay in taxes?” 1 (no) to 5 (nearly all), “How much of the time do you think you can trust the government in Washington to do what is right?” 1 (almost always) to 5 (never), “Do you feel that the people running the government are smart people who usually know what they are doing?” 1 (always know what doing) to 5 (never know what doing).

Skeptical attitudes toward Vietnam War (sum of six item, max 24): “Fighting the war in Vietnam…”: “was damaging to our national honour or pride” 1 (strongly disagree) to 4 (strongly agree), “was really not in the national interest” 1 (strongly disagree) to 4 (strongly agree), “was important to fight the spread of Communism” 1 (strongly agree) to 4 (strongly disagree), “brought us closer to world war” 1 (strongly disagree) to 4 (strongly agree), “was important to protect friendly countries” 1 (strongly agree) to 4 (strongly disagree), “was important to show other nations that we keep our promises” 1 (strongly agree) to 4 (strongly disagree).

Preferences for civil rights interventions (sum of three items, max 12): “The government in Washington should see to it that white and black children are allowed to go to the same schools if they want to” 1 (disagree) to 4 (agree) , “The government in Washington should see to it that people are treated fairly and equally in jobs, no matter what their race may be” 1 (disagree) to 4 (agree), “It is not the government’s business to pass laws about equal treatment for all races” 1 (agree) to 4 (disagree).

Table A3: Pre-treatment differences in background variables

	C – AT (T=0)	C – NT (T=0)
Wave 3: Military knowledge test (0-40)	0.824	0.994
	(1.269)	(1.511)
Wave 1: IQ Test (0-150)	-5.321	-8.359**
	(3.452)	(3.977)
Wave 1: Self perceived intelligence (1: top 10% to 6: bottom 10%)	0.245	0.493
	(0.203)	(0.315)
Wave 1: has college plans	-0.028	-0.347**
	(0.133)	(0.16)
Military classification (Wave 4)		
Student deferment	-0.071	-0.401**
	(0.142)	(0.181)
Available for military	0.192	0.444***
	(0.136)	(0.152)
Not classified	-0.046	0.021
	(0.072)	(0.066)
Other	-0.075	-0.064
	(0.105)	(0.122)

Notes: Pooled data from all pre-treatment waves. Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Military knowledge test: Number of correct answers to 40 different questions.

IQ-Test: Quick Test of intelligence (for discussion of this test, see Bachman et al.. (1967), *Youth in transition: Volume I*, Ann Arbor, Michigan: Survey Research Center, Institute for Social Research, 1967, p. 63)

Table A4: Pre-treatment differences in outcome variables

	C – AT (T=0)	C – NT (T=0)
Strongly Republican	0.016	-0.048
	(0.038)	(0.051)
Strongly Democrat	-0.004	0.048
	(0.041)	(0.059)
Mildly/Strongly Republican	0.06	0.101
	(0.064)	(0.083)
Mildly/Strongly Democrat	0.006	0.107
	(0.067)	(0.09)
Skeptical attitudes towards the government (max 15)	-0.242	-0.241
	(0.304)	(0.329)
Skeptical attitudes towards Vietnam War (max 24)	0.275	-0.173
	(0.647)	(0.938)
Preferences for civil rights interventions (max 12)	-0.309	-0.732
	(0.312)	(0.459)

Notes: Pooled data from all pre-treatment waves. Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Working Paper 473 - first version: july 2016, this version: July 2017

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Abstract

This paper proposes a difference-in-differences approach for disentangling a total treatment effect on some outcome into a direct effect as well as an indirect effect operating through a binary intermediate variable – or mediator – within strata defined upon how the mediator reacts to the treatment. Imposing random treatment assignment along with specific common trend (and further) assumptions identifies the direct effects on the always and never takers, whose mediator is not affected by the treatment, as well as the direct and indirect effects on the compliers, whose mediator reacts to the treatment. We provide an empirical application based on the Vietnam draft lottery, where we analyse the impact of the random draft lottery number on political preferences. The results suggest that a high draft risk due to the lottery leads to a relative increase in mild preferences for the Republican Party, but has no effect on strong preferences for either party or on policy contents. Moreover, the increase in Republican support is mostly driven by the direct effect.

Citation proposal

Eva Deuchert, Martin Huber, Mark Schelker. 2017. «Direct and indirect effects based on difference-in-differences with an application to political preferences following the Vietnam draft lottery (Version July 2017)». Working Papers SES 473, Faculty of Economics and Social Sciences, University of Fribourg (Switzerland)

Jel Classification

C21, C22, D70, D72

Keywords

Treatment effects, causal mechanisms, direct and indirect effects, Vietnam War lottery, political preferences, difference-in-differences

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