Journal of Business & Economic Statistics

ISSN: 0735-0015 (Print) 1537-2707 (Online) Journal homepage: https://www.tandfonline.com/loi/ubes20

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To cite this article: Eva Deuchert, Martin Huber & Mark Schelker (2019) Direct and Indirect Effects Based on Difference-in-Differences With an Application to Political Preferences Following the Vietnam Draft Lottery, Journal of Business & Economic Statistics, 37:4, 710-720, DOI: 10.1080/07350015.2017.1419139

To link to this article: https://doi.org/10.1080/07350015.2017.1419139

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Direct and Indirect Effects Based on Difference-in-Differences With an Application to Political Preferences Following the Vietnam Draft Lottery

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We propose a difference-in-differences approach for disentangling a total treatment effect within specific subpopulations into a direct effect and an indirect effect operating through a binary mediating variable. Random treatment assignment along with specific common trend and effect homogeneity assumptions identify 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. In our empirical application, we analyze the impact of the Vietnam draft lottery on political preferences. The results suggest that a high draft risk due to the draft lottery outcome leads to an increase in mild preferences for the Republican Party, but has no effect on strong preferences for either party or on specific political attitudes. The increase in Republican support is mostly driven by the direct effect not operating through the mediator that is military service.

KEY WORDS: Causal mechanisms; Direct and indirect effects; Political preferences; Treatment effects; Vietnam War draft lottery.

1. INTRODUCTION

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

The main contribution of this article is a difference-in-differences (DiD) approach under random treatment assignment that separates direct and indirect effects. We do so within subpopulations which are defined based on how/whether a binary mediator reacts to treatment assignment. Our approach identifies the 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 allow assessing 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. For identification, random treatment assignment, monotonicity of the mediator in the treatment, common trend restrictions, and effect homogeneity assumptions are imposed. While random treatment assignment alone only permits evaluating the total treatment effect, our DiD method in addition tackles mediator endogeneity within subpopulations. To this end, we assume that the mean potential outcomes for specific treatment–mediator combinations evolve in the same way over time across specific subpopulations (rather than across treatment groups as in standard DiD).

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" was for instance imposed by 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 article is to the best of our knowledge the first one to offer an alternative to sequential ignorability and instrumental variable assumptions and to propose identification 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

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Journal of Business & Economic Statistics
October 2019, Vol. 37, No. 4
DOI: 10.1080/07350015.2017.1419139

(or principal strata, see Frangakis and Rubin 2002), which are 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 for focusing 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 and Lemieux 2001; Kuziemko 2010; Deuchert and 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 support for Democrats and strengthened liberal attitudes. 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 significant 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. However, we find no effects on Vietnam War attitudes, general attitudes toward the government, or attitudes toward the Civil Rights Movement. Given that we find no effects on the probability to *strongly* prefer a party, the results are in line with a traditional swing voter interpretation, in which citizens without a stable party attachment in the center of the policy spectrum update their *stated* party preferences. Provided that we do not find evidence of changes in other measures of policy preferences and attitudes, underlying personal preferences seem to be unaffected. Therefore, changes in *stated* party preferences are more likely to result from perceived changes of policy platforms offered by political parties rather than changes of underlying preferences. Hence, in contrast to Erikson and Stoker (2011) and Bergan (2009), our results are in line with traditional microeconomic assumptions of stable preferences.

Our approach is also relevant from a practical perspective: in the political process politicians formulate policy objectives (e.g., to increase fairness of the military draft) and, based on such objectives, define policy interventions (e.g., draft lottery). In that sense, the indirect effect (e.g., increasing fairness in actual military service) is often the politically intended main effect of a policy. However, in anticipation of individuals who do not freely comply with policies (never takers), policymakers also define complementary policies (e.g., limiting educational deferment options or outright sanctions) in order to increase compliance. Therefore, it is not only important to understand the politically intended main effect of a policy (the "indirect effect on compliers"), but also the side effects, such as the "direct effects on compliers" as well as the effects on never takers (e.g., on those who are potentially sanctioned) and always takers (who remain unaffected by the policy intervention).

The commonly considered local average treatment effect (LATE) on compliers is equivalent to our indirect effect on compliers. The LATE is conventionally estimated by instrumental variable methods (e.g., Angrist 1990; Angrist, Chen and Frandsen 2010). The indirect effect estimated by our method is 10 times lower and statistically significantly different from the LATE. This suggests 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 article is organized as follows. Section 2 outlines the econometric framework, that is, 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 is 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 $\Delta_1 = E[Y_1(1, D(1)) - Y_1(0, D(0))]$. That is, the ATE corresponds to the cumulative effect of Z on the outcome that either affects the latter directly (i.e., any effect not running through 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 $\theta_1(z) = E[Y_1(1, D(z)) - Y_1(0, D(z))]$ and $\delta_1(z) = E[Y_1(z, D(1)) - Y_1(z, D(0))]$, by adding and subtracting $Y_1(1, D(0))$ or $Y_1(0, D(1))$:

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

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 by 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 ($d : D_i(1) = 0, D_i(0) = 1$) 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 $\Delta_1^\tau = E[Y_1(1, D(1)) - Y_1(0, D(0))|\tau]$ 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 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

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

Assumption 2 is standard in the literature on local average treatment effects (see Imbens and Angrist 1994; Angrist, Imbens and Rubin 1996) and rules out the existence of defiers. Defiance seems to be a common behavior 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

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

Assumption 3 rules out anticipation effects of the treatment or the mediator w.r.t. 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 1–3 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

[1] Robins and Greenland (1992) and Robins (2003) referred 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).

[3] Vander Weele (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.

point to a violation of our identifying assumptions. Furthermore, Assumption 1 allows identifying the average treatment effect in the total population:

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

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

$$\begin{aligned}
&\mathrm{E}\left[Y_0\left(0,0\right)|a\right] - \mathrm{E}\left[Y_0\left(0,0\right)|c\right]\\
&\quad = \frac{p_a + p_c}{p_c}\left[\mathrm{E}(Y_0\,|Z=0, D=1) - \mathrm{E}(Y_0\,|Z=1, D=1)\right],\\
&\quad \mathrm{E}\left[Y_0\left(0,0\right)|n\right] - \mathrm{E}\left[Y_0\left(0,0\right)|c\right]\\
&\quad = \frac{p_n + p_c}{p_c}\left[\mathrm{E}(Y_0\,|Z=1, D=0) - \mathrm{E}(Y_0\,|Z=0, D=0)\right],
\end{aligned}$$

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 invariant.

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

$$\begin{aligned}
&E\left[Y_1\left(0,0\right)|\,n\right] - E\left[Y_0\left(0,0\right)|\,n\right]\\
&\quad = E\left[Y_1\left(0,0\right)|\,c\right] - E\left[Y_0\left(0,0\right)|\,c\right].
\end{aligned}$$

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

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

Proof. Proofs of Theorem 1–6 are relegated to the 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 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 $z=0$ and $d=0$ and homogeneous mean effects of Z and D jointly across compliers and always takers

$$\begin{aligned}
&E\left[Y_1\left(0,0\right)|a\right] - E\left[Y_0\left(0,0\right)|a\right]\\
&\quad = E\left[Y_1\left(0,0\right)|c\right] - E\left[Y_0\left(0,0\right)|c\right],\\
&E\left[Y_1\left(1,1\right)|a\right] - E\left[Y_1\left(0,0\right)|a\right]\\
&\quad = E\left[Y_1\left(1,1\right)|c\right] E\left[Y_1\left(0,0\right)|c\right]
\end{aligned}$$

The first part of Assumption 5 is a common trend assumption on mean potential outcomes under $z=0$ and $d=0$ similar to Assumption 4, however imposed w.r.t. compliers and always takers (rather than never takers). The second part requires the joint mean effects of Z and D to be homogeneous across compliers and always takers. Assumption 5 is therefore stronger than Assumption 4, which does not require effect homogeneity across strata. We note that Assumption 5 implies $E[Y_1(1,1)|a] - E[Y_0(1,1)|a] = E[Y_1(1,1)|c] - E[Y_0(1,1)|c]$, namely a common trend in mean potential outcomes of always takers and compliers under $z=1$ and $d=1$. The first part of Assumption 5, that is, the common trend restriction under $z=0$ and $d=0$ 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 second part with 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

[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.

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 $D = 1$:

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

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

Theorem 3. Average treatment effect on the compliers

Under Assumptions 1–5,

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

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

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

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 behavior—not only with respect to d, which is true for always takers by definition, but also with respect to any other mechanism that could affect the outcomes and is subsumed into the direct effect. In our application, this appears likely as always takers being exposed to treatment were not forced to change their plans in an important way (in contrast to never takers deliberately entering college to avoid the draft): in fact, the lottery outcome was irrelevant if a person intended joining the army in any way. 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,

$$\Delta_1^c = \frac{E\left[Y_1D|Z=1\right] - E\left[Y_1D|Z=0\right]}{p_{1|1} - p_{1|0}} - E\left[Y_1|Z=0, D=0\right] + \frac{p_{0|1}}{p_{0|0} - p_{0|1}}\left\{E\left[Y_0|Z=1, D=0\right] - E\left[Y_0|Z=0, D=0\right]\right\}.$$

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 $d = 0$

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

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 (implying common trends for compliers and always takers under $z = 1$ and $d = 1$) or Assumption 6 (no direct effect on always takers).

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

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

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

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

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

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

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

Assumption 8. Common trends for compliers and always takers under $z = 0$ and $d = 0$ and homogeneous mean effect of D across compliers and always takers

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

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

The first part of Assumption 8 is the same as the first part of Assumption 5. The second part requires the mean effect of D to be homogeneous across always takers and compliers (rather than the joint mean effects of D and Z as in Assumption 5).[6] As an alternative to Assumption 7, this implies a common trend restriction w.r.t. potential outcomes of the always takers and compliers under $z = 0$ and $d = 1$: $E[Y_1(0, 1)|a] - E[Y_0(0, 1)|a] = E[Y_1(0, 1)|c] - E[Y_0(0, 1)|c]$. Assumption 8 permits identifying the direct effect under treatment (when either imposing Assumption 5 or 6) and the indirect effect under nontreatment 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,

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

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

$$\begin{aligned}\theta_1^c(1) &= \frac{E[Y_1D|Z=1] - E[Y_1D|Z=0]}{p_{1|1} - p_{1|0}} \\ &\quad - E[Y_1|Z=0, D=1] + E[Y_0|Z=0, D=1] \\ &\quad - \frac{E[Y_0(1-D)|Z=0] - E[Y_0(1-D)|Z=1]}{p_{0|0} - p_{0|1}}.\end{aligned}$$

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

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

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 and effect homogeneity 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:

$$\begin{aligned}\theta_1(d) &= p_c\theta_1^c(d) + p_a\theta_1^a + p_n\theta_1^n \\ &= \left[p_{1|1} - p_{1|0}\right]\theta_1^c(d) + p_{1|0}\theta_1^a + p_{0|1}\theta_1^n, \\ \delta_1(d) &= p_c\delta_1^c(d) + p_a 0 + p_n 0 = \left[p_{1|1} - p_{1|0}\right]\delta_1^c(d),\end{aligned}$$

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

[6]As for Assumption 5, the common trend restriction in the first part can be verified by placebo tests, while the homogeneous effect restriction is not testable.

3. EMPIRICAL APPLICATION

During the Vietnam War the majority of American troops consisted of volunteers, while the rest were selected through a draft (Gimbel and 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 1969–1972 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 analyze 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 (e.g., Angrist 1990; Angrist, Chen and 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 and Lemieux 2001; Deuchert and 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 is useful to identify how different groups of the population are affected.

Previous contributions studied the impact of the draft lottery on political preferences and attitudes toward the war. Bergan (2009) showed that a low draft lottery number increased the likelihood of people to favor an immediate withdrawal from Vietnam. Erikson and Stoker (2011) analyzed the lottery's impact on young college bound males, who were especially vulnerable to the new draft policy. They found that the effect of the lottery number on political preferences and attitudes was strong. Young males with low draft numbers were more likely to favor 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 analyzing the effect of such a policy change: As a high proportion of respondents managed to avoid the draft, possibly due to a behavioral 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

[7]This can be for example the impact of induced college education, that may lead to more political participation (Dee 2004; Milligan, Moretti, and Oreopoulos 2004; Kam and Palmer 2008; Milstein Sondheimer and Green, 2009) and affect political attitudes by increasing personal income (Morton, Tyran, and Wengström, 2011; Marshall, 2016), 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).

distinguish between the effects of the policy intervention across these subgroups or strata.

3.1 Data

Our data come 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 include individuals serving in the military (if they can be located).

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 and 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.

We measure political preferences with various questions. Specifically, we present summary statistics on the answers to the question "*How would you describe your political preference?*" in Table 1. Further empirical results based on measures of general government attitudes, Vietnam War attitudes, and preferences for civil rights interventions are presented in the online appendix. The descriptive statistics on party preferences in Table 1 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 center stage.[9] Interestingly, the Democrats did not benefit from the scandal with higher rates of support.

Table 1. Descriptive statistics

Wave	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Mildly/strongly	0.305	0.304	0.293	0.231	0.140
Republican	(0.46)	(0.46)	(0.46)	(0.21)	(0.35)
Strongly	0.107	0.078	0.081	0.044	0.025
Republican	(0.31)	(0.27)	(0.27)	(0.21)	(0.16)
Mildly/strongly	0.396	0.377	0.392	0.337	0.296
Democrat	(0.49)	(0.48)	(0.49)	(0.47)	(0.46)
Strongly	0.148	0.098	0.154	0.098	0.087
Democrat	(0.36)	(0.30)	(0.36)	(0.30)	(0.28)

Notes: The columns report the means as well as the standard deviations (in parentheses). The measure of party preferences is based on question B32 of the "Young Men in High School and Beyond" survey by Bachman (1999). The question reads: "*How would you describe your political preference?*" Possible answers are: (1) Strongly Republican, (2) Mildly Republican, (3) Mildly Democrat, (4) Strongly Democrat, (5) American Independent Party, (6) No preference, independent, (7) Other (please specify), (8) Haven't thought about it; don't know. Category 5 to 8 are omitted.

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.

For all outcomes considered, there are no striking differences in pre-lottery outcomes between individuals with high and low draft lottery numbers (see the results of the placebo estimates of the ATE in Table A1 in the online appendix). The same holds for differences in pre-lottery background characteristics as IQ and military classification (see the last column of Table A3 in the online appendix). This indicates that selection bias is unlikely an issue in this application.

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

Assumptions 1 and 3 are sufficient to estimate average treatment effects. The first column of Table A1 in the online

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

[9] The Watergate Scandal refers to the political turmoil initiated by the break-in at the Democratic Party 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.

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 1–4 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, namely waves 3 and 4 (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 on 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): always takers were not forced to change their behavior because they would have joined the army anyway. The lottery outcome was thus irrelevant for always takers. Under Assumptions 1–4, 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) based on waves 3 and 4 that support our strategy. Note that these estimations test for the validity of Assumptions 1–4, as Assumption 6 cannot be tested in pre-treatment periods (unless Assumption 3 fails for the always takers).

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 lottery number ($z = 1$), while Assumption 8 identifies the indirect effect of joining the army when compliers receive a high lottery 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, in which 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, that is, 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 5.6% more likely to report to mildly or strongly favor 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 significant 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 contrast to the interpretations of the results proposed by Bergan (2009) and Erikson and Stoker (2011). They generally interpret their findings to show a substantial positive effect on preferences for Democrats or liberal policy positions. Erikson and Stoker (2011) found that young men with low lottery numbers held more anti-war attitudes; voted more often for McGovern (Democrat) relative to Nixon (Republican); favored Democrats over Republicans in a rating of attitudes towards Nixon vs. McGovern; and favored 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 favoring an immediate withdrawal from Vietnam.

The differences of our results with respect to the studies 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 in 1972 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 and lacked 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

[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 A1.

Table 3. First-stage results

	Regression coef. of D on Z
Random draft number < 126 (compliers)	0.207***
	(0.026)
Constant (always takers)	0.050***
	(0.009)

Notes: Dependent variable: military service (mediator). Standard errors in parentheses are based on 1999 bootstrap replications and take account of clustering on the individual level across time periods, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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. Moreover, it has to be mentioned that the political preference measures in Bergan (2009) and Erikson and Stoker (2011) are not identical to ours. Erikson and Stoker (2011) mostly used composite index measures of preferences, while we use more direct measures of preferences.

3.4 Strata Proportions and Description

In the first stage, we estimate the impact of a random draft number below the ceiling (RDN < 126) 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.

Table 3 reports the coefficients of a regression of the mediator (D) on the treatment (Z). We observe that 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 random draft number below the ceiling of 125—either because they were ineligible or because they already had or applied for a deferment.

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 background variables 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 1–4) to estimate the direct effects of the lottery on the never takers (first results column of Table 4). The direct effects on the preferences of never takers to at least mildly favor 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 of the significant direct effect on 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 into the military. Hence, never takers were actually successful in avoiding the draft, which might have encouraged them to favor 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 toward the government, or attitudes towards the Vietnam War, or the Civil Rights Movement (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]

Columns two to four report the estimated total treatment effects on the compliers based on Theorem 4 (Assumptions 1–4, and 6), as well as the direct effects under nontreatment and indirect effects under treatment based on Theorem 5 (Assumptions 1–4, 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

[11]Note that *stated* party preferences can change as a result of evolving party positions, while underlying preferences remain stable. 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. This corresponds to the standard microeconomic approach to analyzing human decision-making.

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 (columns 2–5; 7: based on 1999 bootstrap replications and take account of clustering on the individual level across time periods; column 6: analytical standard errors), $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

effects nor the decomposed indirect effects reach conventional levels of statistical significance.

We also compare our results 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 (e.g., Angrist 1990; Angrist, Chen, and 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.

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 U.S. on political preferences and attitudes toward 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 military, 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 favoring 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 favor 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. As only mild party preferences (and no other measures of political attitudes) are affected, we propose an interpretation in which swing voters adjust their reported party preferences, presumably by updating their beliefs about party platforms. Our findings seem to be in line with basic microconomic assumptions of stable preferences.

ACKNOWLEDGMENTS

We have greatly benefitted from comments by seminar participants of the Hong Kong University of Science and Technology, the Academia Sinica in Taipei, the University of St. Gallen, the European University Institute in Florence, the Halle Institute of Economic Research, the editors, and two anonymous referees. The usual disclaimer applies.

SUPPLEMENTARY MATERIALS

The supplementary materials contain an online appendix and replication files. The online appendix provides proofs of all theorems, and presents complementary estimation results using alternative measures of political preferences, placebo results and pre-treatment characteristics of background variables.

[Received July 2016. Revised December 2017.]

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