EXPERIMENTS AND REGRESSION DISCONTINUITY WITH DURATION OUTCOMES

Gerard J. van den Berg (Amsterdam, IFAU-Uppsala, IZA)

Antoine Bozio (UCL)

Monica Costa Dias (Porto, IFS)

- Experiments are often seen as the “gold standard” for policy evaluation.
- But sometimes problems with implementation of randomization, and with interpretation of the results.
- Today: what can we learn from experiments if the outcome is a duration variable and we are interested in causal effects on the hazard rate.
- Analyze this by considering a situation with 2 policy regimes.
  In an ideal setting (same populations, same context, no non-compliance etc.): same as randomized experiment.

AIM:

evaluate the effect of a new policy or treatment regime, where the individual outcome of interest is a duration variable.

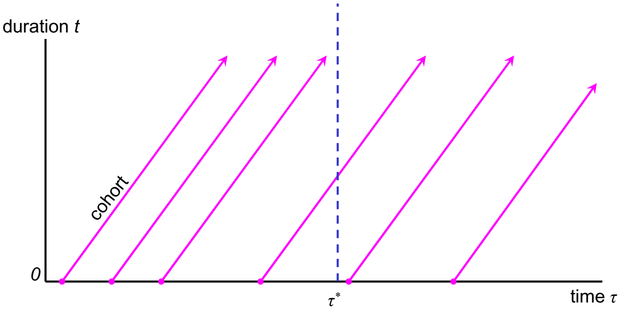
e.g. introduction of a new active labor market policy for the unemployed or new affirmative-action program for the disadvantaged,
or a new medication, or arrival of new information.

FOCUS: SETTING:

- heterogeneous population.
- the data cover two adjacent calendar time intervals with their own mandatory "policy" regime.

FOCUS: SETTING:

- heterogeneous population.
- the data cover two adjacent calendar time intervals with their own mandatory "policy" regime.



Lexis Diagram with a policy change at τ^*

FOCUS: INFERENCE:

- average treatment effects on individual outcomes : conditional on survival until a duration t, notably individual hazard rates at durations t.
- non-parametric analysis.

ISSUES:

- survivors at t are selective subset of population, and the degree of selection may depend on treatment status.
- timing of events $\sim$ accumulation of information $\sim$ identification strategy

Outline:

1. Define meaningful average treatment effects.
2. Non-parametric identification; examine:
   - spells that are either before or after the policy change (like in an experiment),
   - spells crossing the moment of the policy change.
3. Empirical application: job search assistance program for unemployed individuals.

GENERAL NOTATION

s	values representing mutually exclusive treatment statuses; e.g. treated/control, or the duration at which the treatment starts.
$T(s)$	random variable: the potential outcome for a given s.
S	random variable: the actual treatment status.
T	random variable: the actual outcome: $T := T(S)$.

We are interested in properties of the distributions of $T(s)$, in particular, in the effect of s on

$\theta_{T(s)}(t) =$ the hazard rate of $T(s)$ at t. Note: hazard rate: condition on $T(s) \geq t$.

More in general: interest in effect of s on conditional distributions of $T(s) \mid T(s) \geq t$, e.g. $\Pr(T(s) \geq t + 3 \mid T(s) \geq t)$.

We want to summarize the effect of s on $\theta_{T(s)}(t)$ by way of averages (to be defined), over individuals, of the individual treatment effect on the hazard rate at t,

$$\frac{\theta_{T(s')}(t)}{\theta_{T(s)}(t)} \quad \text{or e.g.} \quad \theta_{T(s')}(t) - \theta_{T(s)}(t)$$

We want to summarize the effect of s on $\theta_{T(s)}(t)$ by way of averages (to be defined), over individuals, of the individual treatment effect on the hazard rate at t,

$$\frac{\theta_{T(s')}(t)}{\theta_{T(s)}(t)} \quad \text{or e.g.} \quad \theta_{T(s')}(t) - \theta_{T(s)}(t)$$

Ex ante individual heterogeneity:

X observed individual characteristics
V unobserved individual characteristics

(for ease of exposition: X, V time-invariant).

We want to summarize the effect of s on $\theta_{T(s)}(t)$ by way of averages (to be defined), over individuals, of the individual treatment effect on the hazard rate at t,

$$\frac{\theta_{T(s')}(t)}{\theta_{T(s)}(t)} \quad \text{or e.g.} \quad \theta_{T(s')}(t) - \theta_{T(s)}(t)$$

Ex ante individual heterogeneity:

X	observed individual characteristics
V	unobserved individual characteristics

(for ease of exposition: X, V time-invariant).

Assumption 1. Assignment:

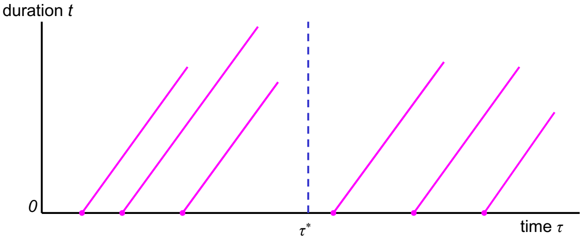
- $S \perp\!\!\!\perp \{T(s)\} \mid X, V$.
- $S \perp\!\!\!\perp V \mid X$.

Consider the introduction of a policy regime at calender time τ^*.

The policy consists of a compulsory and immediate exposure ("treatment") for everyone in the state of interest (like an experiment with treated and controls).

Suppose we follow the common approach: compare two samples:

- spells starting after the discontinuity,
- spells starting before the discontinuity
  (if ongoing at the discontinuity: then censored or discarded).



"before" sample and "after" sample

...in our notation: one sample with $S = 0$ (“after”) and one with $S = \infty$ (“before”).

Average treatment effect on the individual hazard rate:

some average of e.g.

$$\theta_{T(0)}(t|X,V) - \theta_{T(\infty)}(t|X,V)$$

or some average of e.g. the ratio.

average over which population?

recall: hazard rates at t are conditional on survival until t.

Problem: in general at any $t > 0$:

$$V \not\perp\!\!\!\perp S \mid X, T \geq t$$

so the composition of survivors at t may depend on whether $S = 0$ or $S = \infty$.

- so we have to be more specific on the definition of the average effect,
- we cannot just plug in the nonparametric estimators of $\theta_T(t|X, S = 0)$ and $\theta_T(t|X, S = \infty)$.

Meaningful average treatment effects on individual hazard rates? E.g for the ratio:

1. ATE$(t|X) := \mathbb{E}_V \left[\dfrac{\theta_{T(0)}(t|X,V)}{\theta_{T(\infty)}(t|X,V)} \;\middle|\; X \right]$

   aggregate over $V|X$ in population.

2. alternative concepts: e.g.

   Average Treatment Effect on the Treated Survivors at t:

$$\text{ATTS}(t|X) := \mathbb{E}_V \left[\frac{\theta_{T(0)}(t|X,V)}{\theta_{T(\infty)}(t|X,V)} \;\middle|\; X,\ T(0) \geq t \right]$$

Observable hazard rates are not useful (Meyer, 1996, back to Vaupel et al., 1979):

$$\frac{\theta_T(t|X,S=0)}{\theta_T(t|X,S=\infty)} = \frac{\mathbb{E}_V(\theta_T(t|X,S=0,V) \mid X,T\geq t,S=0)}{\mathbb{E}_V(\theta_T(t|X,S=\infty,V) \mid X,T\geq t,S=\infty)}$$

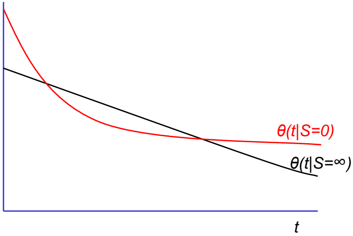
$\neq$ ATTS$(t|X)$ or ATE$(t|X)$.

Observable hazard rates are not useful (Meyer, 1996, back to Vaupel et al., 1979):

$$\frac{\theta_T(t|X,S=0)}{\theta_T(t|X,S=\infty)} = \frac{\mathbb{E}_V(\theta_T(t|X,S=0,V) \mid X, T \geq t, S=0)}{\mathbb{E}_V(\theta_T(t|X,S=\infty,V) \mid X, T \geq t, S=\infty)}$$

$\neq$ ATTS$(t|X)$ or ATE$(t|X)$.

Even possible: $\frac{\theta_T(t|X,S=0)}{\theta_T(t|X,S=\infty)} < 1$ for some t when at the same time
$\forall t, X, V \quad \frac{\theta_{T(0)}(t|X,V)}{\theta_{T(\infty)}(t|X,V)} > 1.$



So: here, and in an ideal randomized experiment:

observed hazard rates among treated and controls are not informative on any meaningful average treatment effect,

and can even give the impression that the effect has a different sign than in reality.

With the data at hand: only solution: impose model structure.
e.g. MPH:

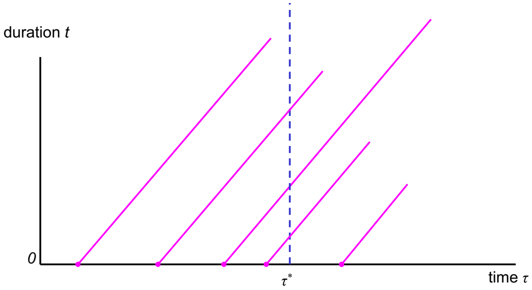
$$\theta_{T(s)}(t|X,V) \;=\; \lambda(t) \;\cdot\; \exp(\gamma s + \beta X) \;\cdot\; V$$

but this amounts to effect homogeneity.

SPELLS INTERRUPTED BY POLICY CHANGE

Setting:

- Again: new policy regime at calender time τ^*:
  immediate and compulsory treatment for everyone.
- Ongoing spells are also affected.
- Observe cohorts flowing in at calender times $\tau < \tau^*$.



include spells interrupted by the policy change

Assumption 2. No Anticipation:

Current hazard does not depend on future policy:

For all $s \in [0, \infty)$ and all $t \leq s$ and all X, V, $\Theta_{T(s)}(t|X, V) = \Theta_{T(\infty)}(t|X, V)$.

Note: Assumption 1 (“Assignment”) implies that the distribution of $V|X$ in the inflow does not vary over time before τ^*.

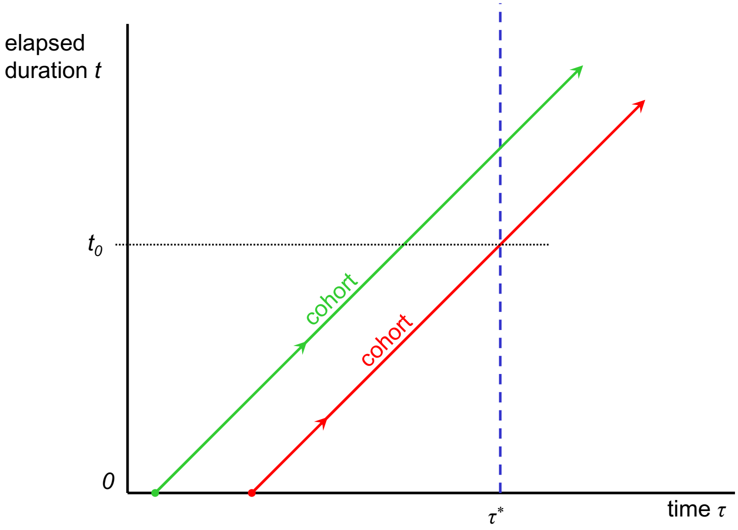
Meaningful average treatment effects on individual hazard rate: like before:

$$\text{ATE}(s', s, t|X) := \mathbb{E}_V \left[\frac{\theta_{T(s')}(t|X,V)}{\theta_{T(s)}(t|X,V)} \;\middle|\; X \right]$$

$$\text{ATTS}(s', s, t|X) := \mathbb{E}_V \left[\frac{\theta_{T(s')}(t|X,V)}{\theta_{T(s)}(t|X,V)} \;\middle|\; X \,,\; T(s') \geq t \right]$$

with $s' \leq t < s$.

We now show: such things are identified from pre-policy and interrupted spells.


elapsed
duration t
t0
0
cohort
cohort
τ*
time τ

Intuition:

Consider two cohorts at duration t_0. The youngest cohort entered at $\tau^* - t_0$ and the other at an earlier date $\tau^* - t_1$.

- At the duration t_0, the youngest cohort is for the first time exposed to the policy, but the earlier cohort not yet.
- Initially, the cohorts are the same (A1: CIA), and individuals do not foresee the moment of exposure to the policy (A2: no anticipation)
  $\Rightarrow$ the cohorts' dynamic evolution is the same on $(0, t_0)$.
- So the cohorts' composition at t_0 in terms of unobservables $V|X$ is the same.
- So a difference between observable outcomes conditional on $T \geq t_0$ can be fully attributed to the causal policy effect.

E.g. consider the difference of the hazard rates at t_0:

$$\theta_T(t_0|X, S = t_0) - \theta_T(t_0|X, S = t_1)$$
$$= \text{ATTS}(t_0, t_1, t_0|X)$$

<u>MAIN RESULT:</u>
Under Assumptions 1,2 (CIA & no-anticipation), data from the pre-policy-inflow cohorts non-parametrically identify average treatment effects at given t and X.

This is a new result. It gives rise to a range of additional results (more cohorts... other outcome measures... aggregating over X).

We do not use or need a model with full identification.

The observed hazards $\theta_T(t_0|X, S = t_i)$ are by definition identified, and can always be estimated in some way.

The question is what we can learn from their differences.
We saw that $\widehat{\theta}_T(t_0|X, S = 0) - \widehat{\theta}_T(t_0|X, S = \infty)$ is meaningless, unless there is no unobserved heterogeneity.

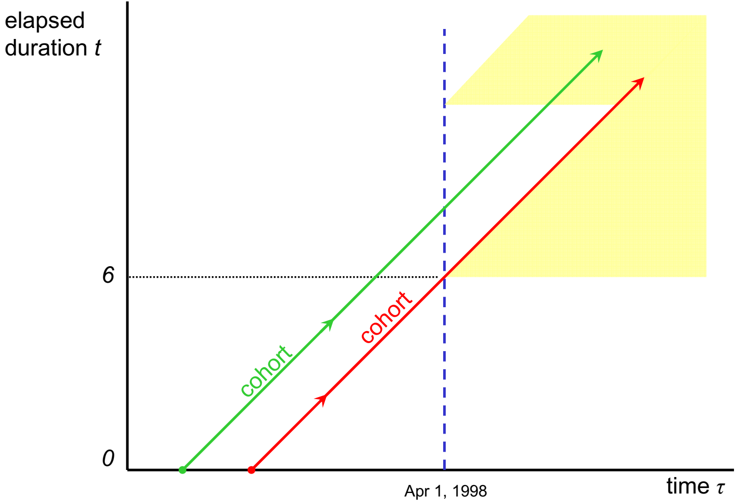
But $\widehat{\theta}_T(t_0|X, S = t_0) - \widehat{\theta}_T(t_0|X, S = t_1)$ corresponds to a meaningful average causal effect. Goes deeper than saying that interrupted spells provide variation in S:

We can identify meaningful average causal effects in the presence of unobserved heterogeneity and heterogenous treatment effects, without (MPH) model.

APPLICATION: THE NEW DEAL FOR YOUNG PEOPLE:

- Job search assistance program.
- Target population: unemployment benefits recipients in the UK, aged 18-24, elapsed unemployment duration exactly 6 months.
- Introduced on $\tau^* =$ April 1, 1998.
- Compulsory and permanent treatment.
- Huge: until now: $>$1.5 million participants, $>$5 billion euro.
- Data: JUVOS: 5% of all unemployment spells.

Introduction of NDYP

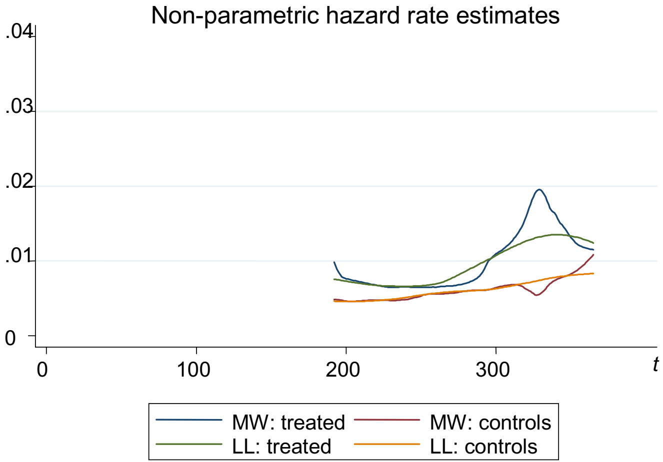


We estimate ATTS for the exit rate to work at $t_0 = 6$,
by way of estimating $\theta_T(6|\text{inflow at } \tau^* - 6)$
and $\theta_T(6|\text{inflow at } \tau^* - 7)$.
Total $n =$1151.

RESULTS:

Boundary kernel hazard estimation (optimal local bandwidths) or local linear hazard estimation, for each hazard rate: $\Rightarrow$
95% C.I. for difference: $(0.0011, 0.0087)$, and for ratio: $(1.04, 2.97)$.

So among those who enter the regime at or after 6m, the program has a significantly positive effect on the exit rate at 6m.


Non-parametric hazard rate estimates
.04
.03
.02
.01
0
0
100
200
300
t
MW: treated
MW: controls
LL: treated
LL: controls

Concerning those with t <6 at τ^*:

- In Apr98: individuals at $t < 6$: become aware of treatment at $t = 6$.
  $\longrightarrow$ possibly adjust behavior before $t = 6$.

  This is also an immediate, compulsory treatment at τ^*.
  We estimate ATTS at durations < 6.

- Results: average hazard at $t < 6$ in Apr98 is significantly lower than at the same t in earlier cohorts.
  $\Rightarrow$ individuals wait for the job search assistance.

  $\Rightarrow$ composition of those who flow into JSA at $t = 6$ probably “better” than of those with $t = 6$ before NDYP.

SUMMARY AND CONCLUSIONS:

- Spells that are *ongoing* at the implementation of a policy regime can be used to estimate meaningful average treatment effects on hazard rates,
  in the presence of unobserved heterogeneity,
  without the need to rely on MPH models.
- Using only spells from before or after the discontinuity does not allow for this.
- Application to NDYP data suggests a positive average effect of treatment,
  but adverse behavioral response before treatment at 6m.
- Implications for experimental design: randomize in ongoing spells.