

# **Evidence of randomisation bias in a large-scale social experiment: The case of ERA**

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TSE

# Contribution

- RCTs hailed as gold standard in program evaluation
- Identifying “no randomisation bias” assumption
- Develop framework to think about randomisation bias
- First empirical evidence on randomisation bias in an actual social experiment (ERA)
  - Empirically test extent to which randomisation *per se* has affected participation
  - Non-experimental methods to assess extent to which experimental impacts are representative of impacts that would have been experienced by the population who would have been exposed to ERA in routine mode
  - Extend estimators to deal with
    - non-linear case of binary outcomes
    - selective survey non-response based on observed characteristics
  - Partial identification

## Randomisation bias

- Program can run in routine mode ( $RCT=0$ ) or along with a randomised trial ( $RCT=1$ )
- $D(RCT)$ ,  $Y_1(RCT)$  and  $Y_0(RCT)$

**Randomisation bias** (Heckman, 1992) if

$$ATT(1) \equiv E(Y_{1i}(1) - Y_{0i}(1) \mid D_i(1)=1) \neq E(Y_{1i}(0) - Y_{0i}(0) \mid D_i(0)=1) \equiv ATT(0)$$

Assume  $Y_{ki}(1) = Y_{ki}(0) = Y_{ki}$  for  $k=0,1$  and for all  $i$ :

$$ATT(1) \equiv E(Y_{1i} - Y_{0i} \mid D_i(1)=1) \neq E(Y_{1i} - Y_{0i} \mid D_i(0)=1) \equiv ATT(0)$$

- Compliers  $D_i(1) < D_i(0)$
- Defiers  $D_i(1) > D_i(0)$
- Always-takers  $D_i(1) = D_i(0) = 1$
- Never-takers  $D_i(1) = D_i(0) = 0$

When all that is available is an RCT:

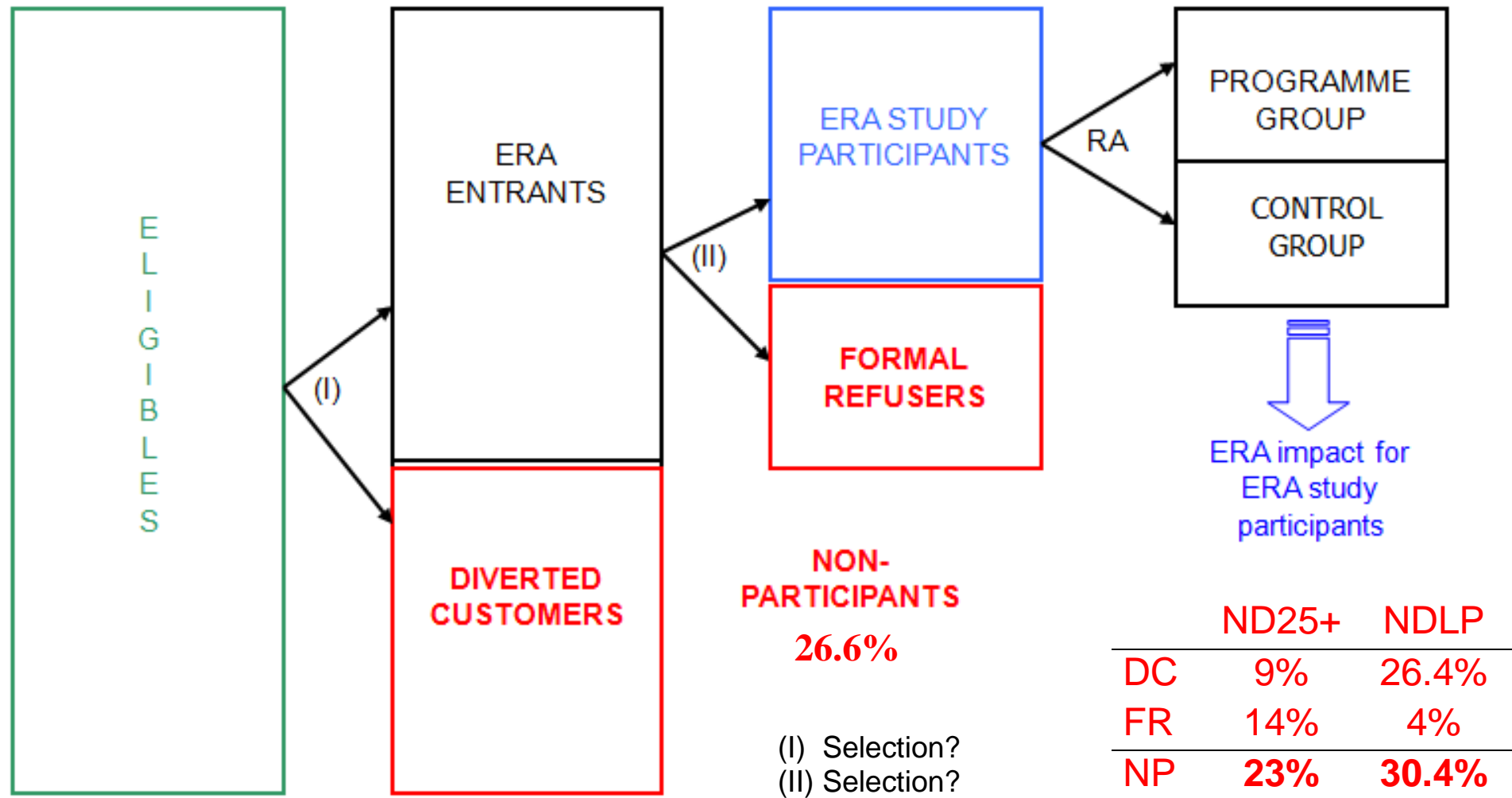
- $D(0)=1$  – group of treated under normal operation (always-takers & compliers) and
  - $ATT(0)$  – treatment effect for this group
- are in general unobserved.

# The Employment Retention and Advancement (ERA) Study

- ERA treatment = offer of a package of time-limited in-work support
  - advisory services
  - financial incentives
- Eligibles:
  - LT unemployed mandated for ND25+
  - unemployed LPs volunteering for NDLP
- Tested as large-scale ( $N=16,000$ ), multi-site (6 districts) social experiment
- Randomisation of eligibility design
- Parameter of interest:  
average treatment effect (*ATE*) of offering eligibility to ERA services and incentives (against standard New Deal) – an *ITE*

However ...

# Non-participation in the ERA study



## Randomisation bias in the ERA study

1. ERA: bestowing of an eligibility
  2. Parameter of interest: average impact of *offering* this eligibility
  3. In routine mode, eligibility would have covered a well-defined (and observed) population  
→  $D(0)=1$  group that would have been exposed to ERA under normal operation is *observed*
- No never-takers
  - No defiers
  - $D(0)=1$  group of New Deal entrants (observed) =  
ERA study participants (the always-takers) +  
formal refusers & diverted customers (the compliers, i.e. those eligibles not included in study)

Causal effect of randomisation on program participation choices is identified:

$$E(D(1) - D(0)) = -\pi \quad \pi \equiv \text{incidence of (DC+FR)}$$

But: experiment does not allow for identification of the *ATE*, i.e. the average effect that ERA would have had on the full eligible population

## Aside: Randomisation bias in the clinical literature

- RCTs: recognised gold standard underpinning evidence-based medicine

BUT

- Deliberate attempts to subvert randomisation (“Randomized controlled trials appear to annoy human nature – if properly conducted, indeed they should”, Schulz, 1995)
- **Randomisation can affect potential outcomes**
  1. Preference effects
    - Therapeutic effect of choice and sense of control can affect outcomes
    - Differential compliance by patients (care by physicians) assigned to their preferred or non-preferred treatment, esp. “resentful demoralisation”, can bias impact estimates
    - Blinding
    - Zelen’s (1979) “randomised consent” design and other designs incorporating preference arms
  2. Informed consent effects
    - Affect therapeutic response to treatment – Dahan *et al.* (1986) and Bergmann *et al.* (1994)

- **Randomisation can lead to biased sampling of the population**

- Low accrual to clinical trials
- Published trials mostly do not report (nor justify) exclusion criteria, nor characteristics of eligible non-participants
- Chain to be included in RCT
  1. Centre selection (“site effects”) – not exclusive to RCT
  2. Researchers: blanket exclusion criteria and scientific/administrative reasons arising from randomisation
  3. Clinicians – personal discomfort with randomisation (Ellis, 2000)
  4. Patients
    - Preferences
    - Informed refusal due to dislike of randomisation and loss of control; fear of being a placebo responder
- Britton *et al.* (1998) review:

“Those who participate in RCTs are often a highly selected group quite unrepresentative of the population to whom the results will be applied”

“Authors often ignored or discounted clear, statistical evidence that participation bias may have occurred – presumably because they felt it would undermine their findings.”

“Very limited evidence available on which to make judgements on this issue.”



## **How does the clinical literature deal with “participation bias”**

- Speculate about the potential generalisability of their intervention to other sites
  - Frameworks for the evaluation of external validity, e.g. integral “process evaluations” and checklists (e.g. Bornhöft *et al.*, 2006)
  - Qualitative research (interviews, questionnaires) to explore preference formation and decision-making in clinical RCTs; probing attitudes towards likely participation in *hypothetical* RCT.
  - Study closest to mine: Bartlett *et al.* (2005)
    - Investigate exclusion from statins trials of women, older people and ethnic minorities  
Compare those included, those using the drugs in the ‘real world’ and those with an evident need of the treatment in the ‘real world’
    - Estimate level of relative effectiveness in these groups and see whether they differ greatly from those in the well-represented groups
- Evidence-synthesis techniques:
- Fixed-effects meta-analysis – confirming statins effectiveness for women
  - Random-effects meta-regression of proportion of women in each trial against the trial effect size – do not even present results; advocate individual patient data meta-analysis
- Data too sparse to investigate ethnic groups or adverse events in any socio-demographic group

## Sample and data

	ND25			NDLP		
Eligibles	7,796	100.0%		7,261	100.0%	
– Study non-participants	1,790	23.0%		2,209	30.4%	
– Study participants	6,006	77.0%	100.0%	5,052	69.6%	100.0%
– with survey outcome	1,840		30.6%	1,745		34.5%
– without survey outcome	4,166		69.4%	3,307		65.5%

### Outcomes

- 12-month follow-up
- employment (employed at month 12 and days employed) – admin data
- annual earnings – survey data

## Control variables

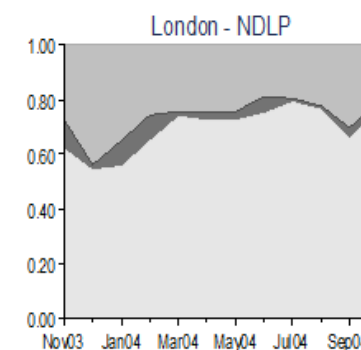
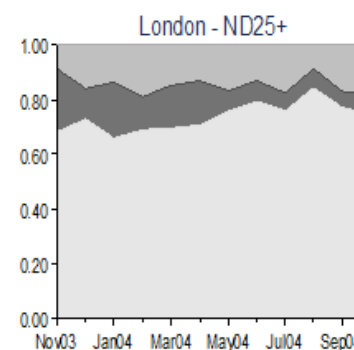
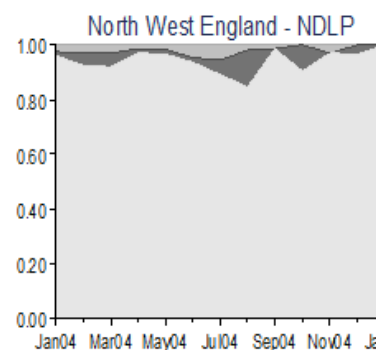
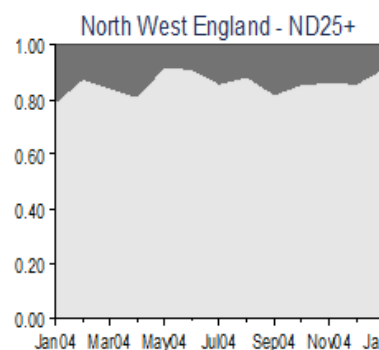
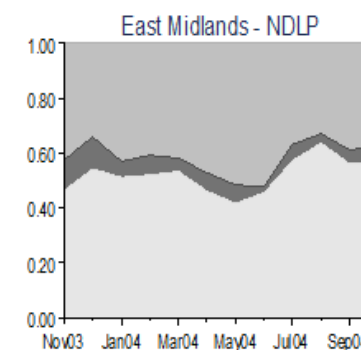
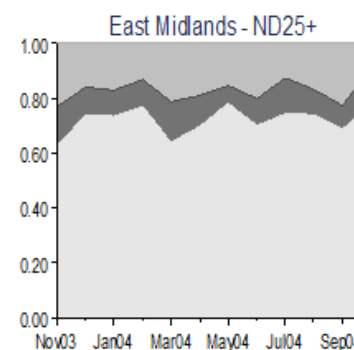
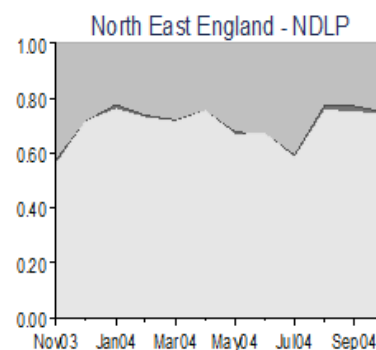
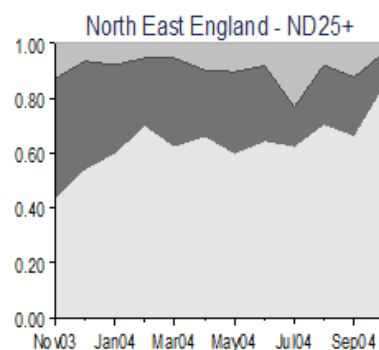
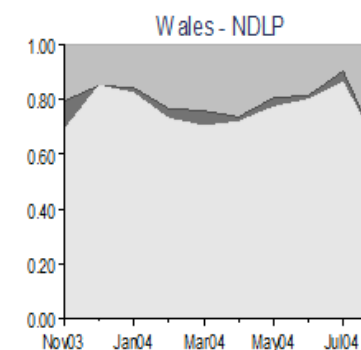
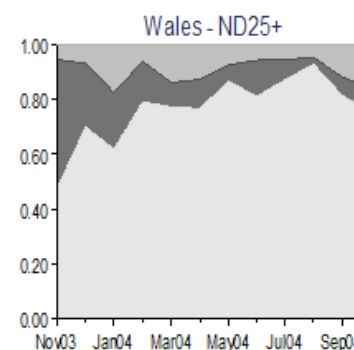
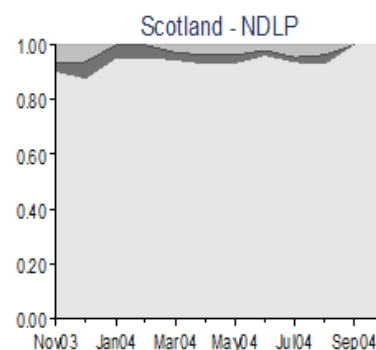
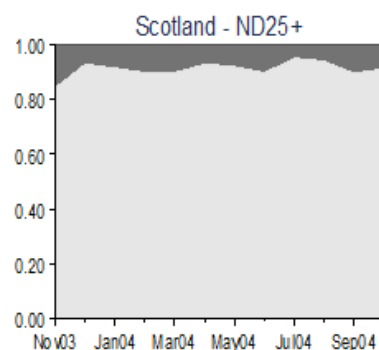
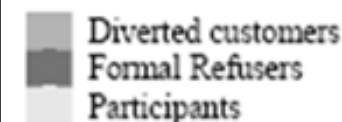
ERA district	
Inflow month	District-specific month from random assignment start when the individual started the ND25 Gateway or volunteered for NDLP
Local conditions	Total New Deal caseload at office, share of lone parents in New Deal caseload at office, quintiles of the index of multiple deprivation, local unemployment rate
Demographics	Gender, age, ethnic minority, disability, partner (ND25+), number of children (NDLP), age of youngest child (NDLP)
Current spell	Not on benefits at inflow (NDLP), employed at inflow (indicator of very recent/current employment), time to show up (defined as the time between becoming mandatory for ND25+ and starting the Gateway or between being told about NDLP and volunteering for it), early entrant into ND25+ programme (Spent <540 days on JSA before entering ND25+)
Labour market history (3 years pre-inflow)	<p>Past participation in basic skills, past participation in voluntary programmes (number of previous spells on: NDLP, New Deal for Musicians, New Deal Innovation Fund, New Deal Disabled People, WBLA or Outreach), past participation in ND25+</p> <p>Active benefit history, inactive benefit history, employment history:</p> <ol style="list-style-type: none"> <li>(1) parsimonious summary</li> <li>(2) monthly employment dummies</li> <li>(3) dummies for sequences of employment/benefits/neither states</li> <li>(4) dummies for ever employed in 12m window at any time in the past</li> </ol>

# Impact of RA on program participation

Causal effect of randomisation on program participation choices  
 $E(D(1) - D(0))$

	<b>ND25</b>	<b>NDLP</b>
All	23.0	30.4
Scotland	8.7	5.3
NE England	34.9	29.2
NW England	14.6	6.2
Wales	20.7	23.6
East Midlands	27.5	47.1
London	25.8	31.0

# Impact of RA on program participation



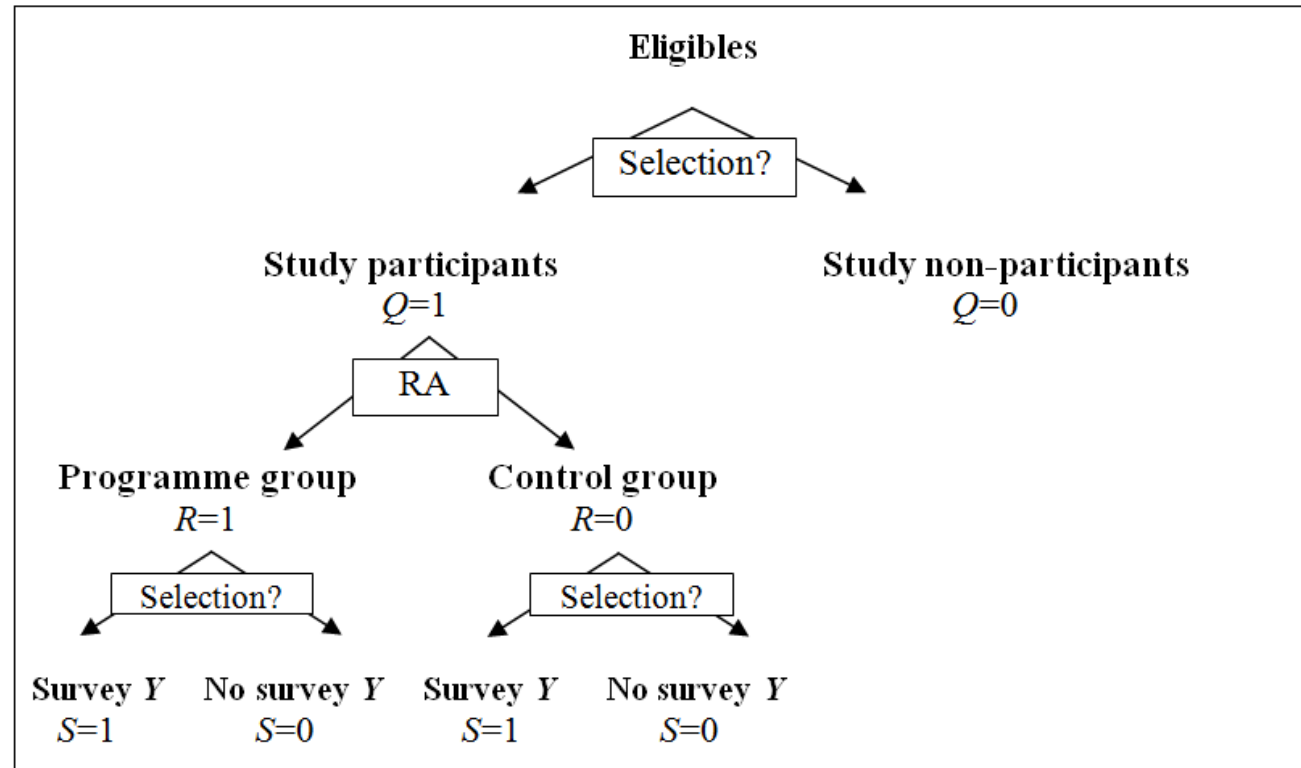
# Impact of RA on program participation

	ND25	NDLP
<b>Diverted customers</b> (% of eligibles)	(9.4%)	(26.4%)
<i>% explained Var(offer) accounted by</i>		
District	12.0	39.7
Office	38.2	3.2
Inflow month	0.0	0.9
<i>vs offered: differences in characteristics</i>		
Employed at intake	0.026***	0.084***
Share of past 3 years in employment	0.002	0.007
Share of past 3 years on benefits	-0.036***	-0.018*
Past ND25+participation, twice or more	-0.026*	0.017
Past participation in voluntary programs	0.005	0.046***
<i>vs controls: differences in outcomes</i>		
Employment rate at $m=1$	0.030**	0.099***
Employment rate at $m=3$	-0.008	0.048***
Employment rate at $m=12$	-0.019	0.002
Days employed in first year	-1.8	3.9
<b>Formal refusers</b> (% of eligibles)	(13.6%)	(4.0%)
<i>% explained Var(consent) accounted by</i>		
District	39.4	57.8
Office	33.1	17.5
Inflow month	7.4	20.5
<i>vs consenters: differences in characteristics</i>		
Employed at intake	-0.011	0.060***
Share of past 3 years in employment	-0.025***	-0.023
Share of past 3 years on benefits	0.053***	0.040*
Past ND25+participation, twice or more	0.100***	-0.016
Past participation in voluntary programs	-0.039***	0.039
<i>vs controls: differences in outcomes</i>		
Employment rate at $m=1$	-0.045***	0.051*
Employment rate at $m=3$	-0.055***	0.021
Employment rate at $m=12$	-0.054***	-0.033
Days employed in first year	-15.0***	3.3

# Impact of RA on program participation

	ND25	NDLP
<b>Non-participants</b> (% of eligibles)	(23.0%)	(30.4%)
<i>% explained Var(participation) accounted by</i>		
District	47.0	50.5
Office	17.1	7.1
Inflow month	6.8	3.1
<i>vs participants: differences in characteristics</i>		
Employed at intake	0.003	0.084***
Share of past 3 years in employment	-0.016***	0.002
Share of past 3 years on benefits	0.020***	-0.001
Past ND25+participation, twice or more	0.054***	0.012
Past participation in voluntary programs	-0.023**	0.047***
<i>vs controls: differences in outcomes</i>		
Employment rate at $m=1$	-0.015	0.092***
Employment rate at $m=3$	-0.036***	0.044***
Employment rate at $m=12$	-0.038***	-0.003
Days employed in first year	-9.4***	3.8

# Methodology



Average effect on participants

$$ATE_1 \equiv E(Y_1 - Y_0 \mid Q=1)$$

Average effect on non-participants

$$ATE_0 \equiv E(Y_1 - Y_0 \mid Q=0)$$

Average effect on all eligibles

$$ATE \equiv E(Y_1 - Y_0) = (1-\pi) \cdot ATE_1 + \pi \cdot ATE_0 \quad \pi \equiv \Pr\{Q=0\}$$

Randomisation bias if

$$ATE_1 \neq ATE$$



## Follow-up data

$$ATE = (1-\pi) \cdot \{E(Y | R=1) - E(Y | R=0)\} + \pi \cdot \{E(Y_1 | Q=0) - E(Y | Q=0)\}$$

Akin to getting the **ATNT** using matching methods

### Assume (CIA-1)

(CIA-1)  $E(Y_1 | Q=0, X) = E(Y_1 | Q=1, X)$  and (Common Support)

Implement by matching methods

### Test (CIA-0)

(CIA-0)  $E(Y_0 | Q=0, X) = E(Y_0 | Q=1, X)$  i.e.  $E(Y | Q=0, X) = E(Y | R=0, X)$

Test null of

- zero average difference – OLS or matching
- zero *conditional* average difference  $\forall X$  – Crump's *et al.* (2008) sieve approach to non-parametric regression function estimation

### (CIA-1) implied by (CIA-0) and (CIA- $\beta$ )

(CIA- $\beta$ )  $E(\beta \mid Q=0, X) = E(\beta \mid Q=1, X)$  where  $\beta \equiv Y_1 - Y_0$

i.e. no residual selection based on unobserved idiosyncratic impact components

In  $Y_i = m(X_i) + \{b(X_i) \cdot R_i\} \cdot Q_i + \{b_i \cdot R_i\} \cdot Q_i + u_i$ , (CIA-1) amounts to  $(u_i, b_i) \perp Q_i \mid X_i$

### Under (CIA- $\beta$ )

If (CIA-0) – hence (CIA-1) – fail, correct matching estimates from selection bias:

$$\begin{aligned} E(Y_1 \mid Q=0, X) &= E(Y_1 \mid Q=1, X) + \{E(Y_0 \mid Q=0, X) - E(Y_0 \mid Q=1, X)\} \\ &= E(Y \mid R=1, X) + \{\textcolor{red}{E(Y \mid Q=0, X) - E(Y \mid R=0, X)}\} \\ &\quad \text{conditional matching} \quad \textcolor{red}{\text{bias in terms of (CIA-0)}} \\ &\quad \text{estimate under (CIA-1)} \end{aligned}$$

## Binary outcomes

(Negative) dependence between base levels ( $Y_0$ ) and differences ( $Y_1 - Y_0$ )

Linear probability model interpretation OR

Extend approach to non-linear setting following Blundell's *et al.* (2004) strategy for DiD models for binary outcomes, invoking (CIA-1) or (CIA- $\beta$ ) at the level of the expectations of the *latent* potential outcome variables

Potential outcomes  $Y_0 = 1(Y_0^* > 0)$  and  $Y_1 = 1(Y_1^* > 0)$ .

This assumes conditional expectation of binary potential outcome variables is related to conditional expectation of the latent outcome variables as:

$$E(Y_0 | Q, X) = H[E(Y_0^* | Q, X)] \quad \text{and} \quad E(Y_0^* | Q, X) = H^{-1}[E(Y_0 | Q, X)]$$

$$E(Y_1 | Q, X) = H[E(Y_1^* | Q, X)] \quad \text{and} \quad E(Y_1^* | Q, X) = H^{-1}[E(Y_1 | Q, X)],$$

with  $H(\cdot)$  strictly monotonously increasing and invertible

$$(\text{CIA-}\beta^*) \quad E(Y_1^* - Y_0^* | Q=1, X) = E(Y_1^* - Y_0^* | Q=0, X)$$

Hence

$$E(Y_1 | Q=0, X) = H\{ H^{-1}[E(Y | R=1, X)] + H^{-1}[E(Y | Q=0, X)] - H^{-1}[E(Y | R=0, X)] \}$$

Assuming a Probit model:

$$E(Y_1 | Q=0, X) = \Phi\{\Phi^{-1}(\Phi(x'\theta_{R=1})) + \Phi^{-1}(\Phi(x'\theta_{Q=0})) - \Phi^{-1}(\Phi(x'\theta_{R=0}))\} = \Phi(x'\theta_{R=1} + x'\theta_{Q=0} - x'\theta_{R=0})$$

$$= \sum_{i \in \{Q=0\}} \frac{\Phi(x_i'\theta_{R=1} + x_i'\theta_{Q=0} - x_i'\theta_{R=0})}{N_0}$$

Binary outcomes	Continuous outcomes
Non-parametric (CIA-1* or CIA-1)	
$ATE_0 = E[E(Y   R=1, X)   Q=0] - E(Y   Q=0)$	
Parametric (CIA-1*)	
$ATE_0 = \sum_{i \in \{Q=0\}} \frac{\Phi(x_i'\theta_{R=1})}{N_0} - E(Y   Q=0)$	
Parametric (CIA-β*)	Non-parametric (CIA-β)
$ATE_0 = \sum_{i \in \{Q=0\}} \frac{\Phi(x_i'\theta_{R=1} + x_i'\theta_{Q=0} - x_i'\theta_{R=0})}{N_0} - E(Y   Q=0)$	$ATE_0 = E[E(Y   R=1, X)   Q=0] - E[E(Y   R=0, X)   Q=0]$

# Partial and point identification of *ATE* under no and alternative assumptions on selection into the ERA study

Manski (1990) and Manski and Pepper (2000)

## Assumptions

No-assumption	Support of $Y$ bounded
<i>MTS</i>	$E(Y_1   Q=0) \leq E(Y_1   Q=1)$
given $X$	$E(Y_1   Q=0, X) \leq E(Y_1   Q=1, X)$
<i>MIV</i>	$E(Y_1   Z=z_1) \leq E(Y_1   Z=z_2)$ for all $z_1 \leq z_2$ in the support of $Z$
<i>MIV + MTS</i>	$E(Y_1   Z=z_1) \leq E(Y_1   Z=z_2)$ for $z_1 \leq z_2$ and $E(Y_1   Q=0, Z=z) \leq E(Y_1   Q=1, Z=z)$
<i>ETS</i>	$E(Y_1   Q=1) = E(Y_1   Q=0)$
(CIA-1)	$E(Y_1   Q=1, X) = E(Y_1   Q=0, X)$
(CIA- $\beta$ )	$E(Y_1 - Y_0   Q=1, X) = E(Y_1 - Y_0   Q=0, X)$

Note: *MTR* indefensible in this application.

## Bounds and point estimates for $E(Y_1)$

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No-assumption

Lower bound  $(1-\pi) \cdot E(Y|R=1)$

Upper bound  $(1-\pi) \cdot E(Y|R=1) + \pi \cdot Y_{max}$

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*MTS*

Upper bound  $E(Y|R=1)$

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*MTS given X*

Upper bound  $(1-\pi) \cdot E(Y|R=1) + \pi \cdot E_X[E(Y|R=1, X) | Q=0]$

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*MIV*

Lower bound  $\sum_z P(Z=z) \cdot \sup_{z_1 \leq z} \{E(Y | R=1, Z=z_1) \cdot P(Q=1 | Z=z_1)\}$

Upper bound  $\sum_z P(Z=z) \cdot \inf_{z_2 \geq z} \{E(Y | R=1, Z=z_2) \cdot P(Q=1 | Z=z_2) + Y_{max}(z_2) \cdot P(Q=0 | Z=z_2)\}$

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*MIV + MTS*

Upper bound  $\sum_z P(Z=z) \cdot \inf_{z_2 \geq z} E(Y | R=1, Z=z_2)$

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Point estimate

*ETS*  $E(Y|R=1)$

(CIA-1)  $(1-\pi) \cdot E(Y|R=1) + \pi \cdot E_X[E(Y|R=1, X) | Q=0]$

(CIA- $\beta$ )  $(1-\pi) \cdot E(Y|R=1) + \pi \cdot \{E_X[E(Y|R=1, X) | Q=0] + E(Y|Q=0) - E_X[E(Y|R=0, X) | Q=0]\}$

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Note that can test/corroborate whether *MTS* and *MIV* (plus *ETS* and (CIA-1)) hold in terms of  $Y_0$

## No follow-up data

$$ATE = (1-p) \cdot \textcolor{red}{ATE}_1 + p \cdot \textcolor{red}{E(Y_1 - Y_0 | Q=0)}$$

Akin to **attrition**

### Assume (CIA- $\beta$ ) and selective non-response based on $X$ (NR)

$$(CIA-\beta) \quad E(Y_1 - Y_0 | Q=1, X) = E(Y_1 - Y_0 | Q=0, X)$$

$$(NR) \quad E(Y_1 | R=1, S=1, X) = E(Y_1 | R=1, S=0, X)$$

$$E(Y_0 | R=0, S=1, X) = E(Y_0 | R=0, S=0, X)$$

$$\rightarrow ATE = E_X[E(Y | R=1, S=1, X)] - E_X[E(Y | R=0, S=1, X)]$$

### Implementation

$$ATE = E[\omega_1(X) \cdot S \cdot R \cdot Y - \omega_0(X) \cdot S \cdot (1-R) \cdot Y], \text{ where}$$

$$\omega_k(X) \equiv \frac{P(Q=1)}{P(Q=1|x)} \frac{P_{RS|Q}(k,1|1)}{P_{RS|Q,X}(k,1|1,x)} \quad \text{for } k=0, 1$$

Or construct the weights via matching.

## Empirical Findings – ND25+

	$ATE_1$	Regression- adjusted $ATE_1$	Raw Ave. Difference	Adjusted Ave. Difference	Zero Conditional Ave. Difference
DAYS EMPLOYED	4.1	4.6*	-9.4***	-9.7***	$p=0.000$
EMPLOYED M=12	0.022**	0.022**	-0.038***	-0.035***	$p=0.000$

		$ATE_1$	$ATE_0$	$ATE$	$ATE_1 \neq ATE$
DAYS EMPLOYED	Unadjusted, (CIA-1)		10.1***	5.9***	*
	Adjusted, (CIA- $\beta$ )	4.6*	0.5	3.7	**
EMPLOYED M=12	Unadjusted, (CIA-1)		0.045***	0.027***	*
	Adjusted, (CIA- $\beta$ )	0.022**	0.014	0.020*	**



## Empirical Findings – NDLP

	$ATE_1$	Regression- adjusted $ATE_1$	Raw Ave. Difference	Adjusted Ave. Difference	Zero Conditional Ave. Difference
DAYS EMPLOYED	-0.1	-2.2	3.8	-11.2**	$p=0.004$
EMPLOYED M=12	-0.007	-0.014	-0.003	-0.039*	$p=0.005$

		$ATE_1$	$ATE_0$	$ATE$	$ATE_1 \neq ATE$
DAYS EMPLOYED	Unadjusted, (CIA-1)	-2.2	-2.1	-2.2	no
	Adjusted, (CIA- $\beta$ )		-13.4**	-5.6	**
EMPLOYED M=12	Unadjusted, (CIA-1)	-0.014	0.000	-0.010	no
	Adjusted, (CIA- $\beta$ )		-0.039**	-0.022*	**

## Plausibility of (CIA- $\beta$ )

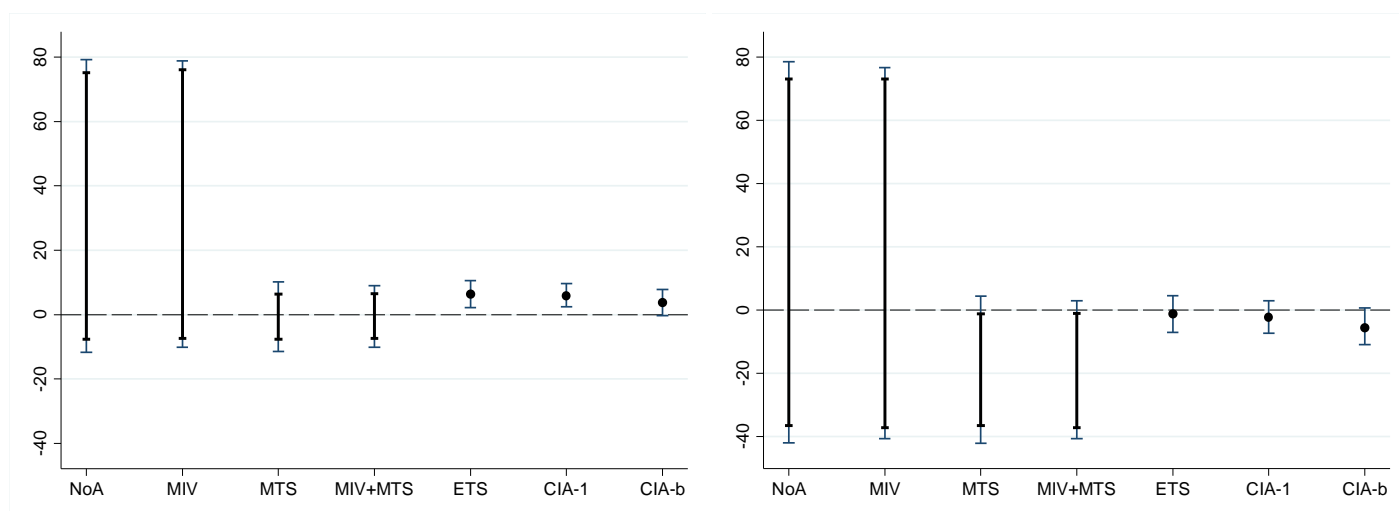
- People not good at estimating counterfactuals (not just *ex ante*, but even *ex post*)  
e.g. Smith *et al.* (2013), Bell & Orr (2002), Frölich (2001), Lechner & Smith (2007), Hirshleifer *et al.* (2014)
- Formal refusers had no substantive knowledge of what ERA was
- Advisers' incentives to divert based on non-ERA very short-term employment probability
- ERA a completely new program for advisers
- Allow for selection on completely general heterogeneity in impacts based on  $X$
- However: 'Mechanical' selection on unobserved impacts if impact depends on unobservables that differ between participants and non-participants

# Partial and point identification of $ATE$ under no/alternative assumptions on selection into ERA

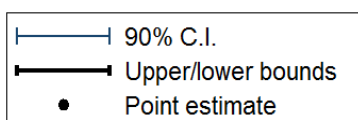
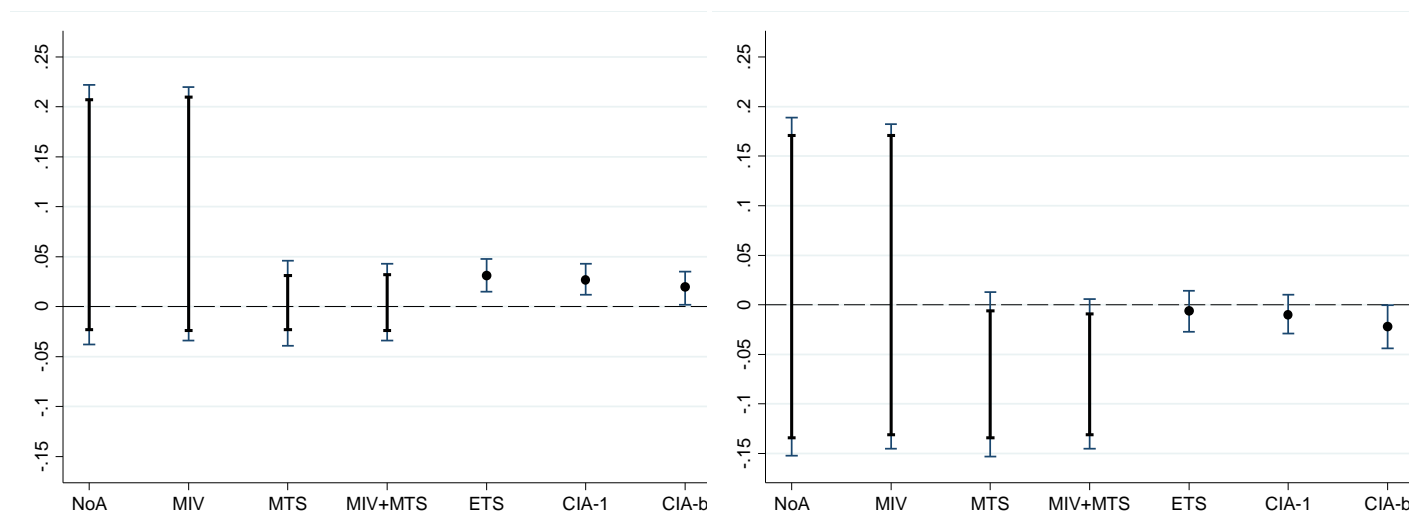
**ND25+**

**NDLP**

Days employed



Employment probability at  $m=12$



## Empirical Findings – Survey annual earnings

		<b>ND25+</b>		<b>NDLP</b>	
$\Delta_{S=1,X}$		445.4**	$\Delta_{S=1,X} \neq ATE$	788.1***	$\Delta_{S=1,X} \neq ATE$
$ATE$	Weighting	579.6**	not sig	762.1***	not sig
	Matching	551.2***	not sig	708.5***	not sig

Note:  $\Delta_{S=1,X}$  denotes experimental contrast for respondents, adjusted for  $X$

# Conclusions

- Framework to think about randomisation bias
  - can render causal inference from RCTs irrelevant for policy purposes
- First empirical evidence on randomisation bias in a social experiment
  - quantify extent to which RA affected participation in ERA study
  - non-experimental methods to assess extent to which experimental impacts are representative of impacts that would have been experienced by the population who would have been exposed to program in routine mode
- Employment outcomes
  - Under no ‘selection on the gain’:
    - Experimental set-up consistently overturned conclusions from non-experimental methods based on standard CIA  
Standard CIA →  $ATE_1$  under-estimates (ND25+) / representative (NDLP)  $ATE$   
Once ‘corrected’ →  $ATE_1$  over-estimates  $ATE$
    - Evidence that non-participation has introduced some randomisation bias
  - Taking all evidence into account:  $ATE$  remains most likely positive for ND25+ while likely negative – albeit shrouded in more uncertainty – for NDLP
- Earnings results for participating respondents appear reliable, i.e.  $\Delta_{S=1}$  representative of  $ATE$

# Covariate balance before and after matching:

## Admin outcomes

	Pseudo $R^2$	Prob>chi	Med bias	$B$	$R$	% of concern	% bad
<b>ND25+</b>							
Raw	0.069	0.000	4.0	63.4*	0.90	19	2
Matched	0.001	1.000	0.6	5.7	1.13	0	0
<b>NDLP</b>							
Raw	0.121	0.000	3.3	84.9*	0.58	19	4
Matched	0.001	1.000	0.5	8.0	1.12	0	0

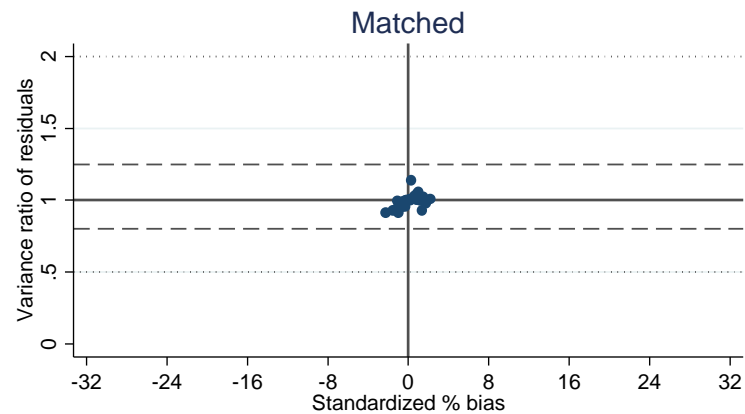
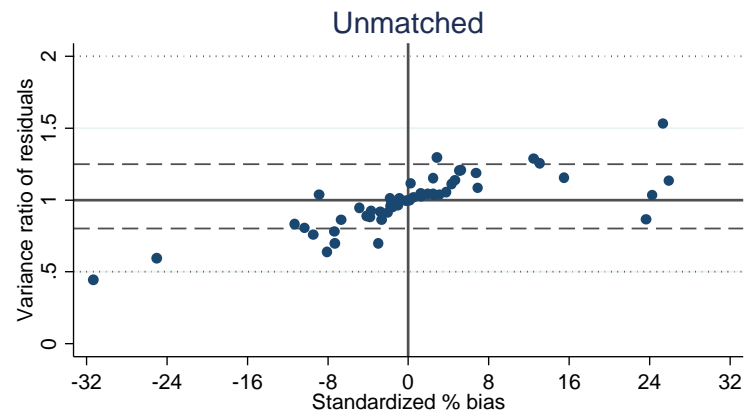
## Survey outcomes

	Eligibles vs responding program group					Eligibles vs responding control group				
	Pseudo $R^2$	Prob>chi	Med bias	$B$	$R$	Pseudo $R^2$	Prob>chi	Med bias	$B$	$R$
<b>ND25+</b>										
Raw	0.034	0.000	2.4	49.3*	0.89	0.039	0.000	3.2	53.5*	0.99
Matched	0.005	0.231	1.0	16.4	1.41	0.006	0.017	0.9	18.5	1.48
<b>NDLP</b>										
Raw	0.039	0.000	2.4	52.2*	0.86	0.048	0.000	3.4	58.7*	0.95
Matched	0.006	0.017	1.1	17.8	1.26	0.009	0.000	1.2	22.2	1.66

## Admin outcomes:

Standardised percentage bias and variance ratio for all model covariates, before and after matching

### ND25+



### NDLP

