## GrubbD-2020-simulated-preallocation-of-treatment-dates-estimates-impact-for-a-target-group-subject-to-ongoing- turnover-and-dynamic-random-assignment-to-the-treatment

[ other possible titles of this note could be:

In a dynamic random assignment context, post-hoc virtual preallocation of treatment dates creates valid control groups

Post-hoc virtual preallocation of treatment dates allows unbiased estimation of impact for a target group subject to ongoing entries, exits and random assignment to the treatment

Simulated-preallocation-of-treatment-dates-in-a-dynamic-random-assignment-context ]

David Grubb, early draft/outline 25.2.2020

### Background

This note originates as comment on a paragraph in DEASP (2019). This describes a context in Ireland where people who have been on the Live Register for more than a year are randomly selected for referral to an employment measure called JobPath. For the group eligible for referral, the Jobseekers Longitudinal Dataset (JLD) includes information about age, etc., social welfare status including start and end date of episodes on the Live Register, participation in employment measures and earnings from employment.

DEASP (2019) presents “Without JobPath” and “With JobPath” outcomes (annual total earnings, mean earnings per week of insurable employment, and annual social welfare payments) and differences between them. The “With JobPath” data relate to people who were referred to JobPath and actually commenced it in Q1 2016, not including those who were referred but did not commence (see note 3 on p.20). The “Without JobPath” data relate to people in the group eligible for referral in Q1 2016 but who were not actually referred then, again not including those were referred but did not commence.

Due to the non-inclusion of those who were referred but did not commence, the average elapsed duration of the ongoing claim at the time of referral is greater for the “With JobPath” group than for the “Without JobPath” group and in reporting the outcomes it was “necessary to reweight the two groups... to ensure the measurement of outcomes at a later stage is a reasonable comparison.” However, for a direct and unadjusted comparison of outcomes between a “treatment” and “control” groups to provide an unbiased of impact, the data would need to be analysed on the “intent to treat” basis, i.e. comparing outcomes for all individuals who were referred with outcomes for all individuals (in the target group for randomisation) who were not referred.

The authors however point out that a second problem arises with this type of comparison due to the continuous nature of the referral process, as follows (DEASP, 2019, p.44):

“As seen below, a number of people who did not start JobPath in Q1 2016 did commence subsequently. It is problematic to remove these people from this study since, by definition, they are people who remain unemployed long enough to start JobPath in later periods, so that excluding them would bias the remainder of the control group towards those who left the Live Register before they could receive JobPath. Equally, it is problematic to retain these cases in the control group because we know that they did in fact receive JobPath at a later point. The solution to this dilemma lies in applying dynamic treatment (see ‘Further Analysis’ below). In the present paper, however, all of those who do not start JobPath in Q1 2016 are retained.

The ‘Further Analysis’ section does not in practice mention ‘dynamic treatment’ or any other planned solution to this dilemma. The current note proposes a solution, starting with the observation that the dilemma could in principle be solved by assigning, in Q1 2016, a future date for referral to each individual who is eligible for referral in Q1 2016, then arguing that this technique can be simulated at the data processing stage without needing to be actually implemented

### Hypothetical preallocation of dates for future referral to Jobpath

A somewhat more complex analysis of JobPath statistics for the period up to end 2017 could generate a statistic described as “the impact of receiving JobPath in Q1 2016 as compared with not receiving it at any time up to end 2017”.

Here it is assumed for presentational purposes that random assignment to JobPath takes place each quarter. DEASP (2019) does not mention a particular frequency, and it may be that referrals by random assignment can be made at any time (as necessary to bring the JobPath provider’s active caseload at each location up to an agreed capacity, or according to other defined principles). Assuming that the JLD records dates of entry to and exit from “available for referral” status and the date of any referral actually made, the referral strategies described here could in principle be applied to daily data, or to data binned into week or month periods, rather than quarters.

* Imagine that all people eligible for JobPath in Q1 2016 were, in Q1 2016, randomly preallocated a date to be referred, including a last category “not to be referred up to end 2017”. A comparison of outcomes for the Q1 2016 treatment group and this last category would provide an unbiased estimate of the impact of referral to JobPath in Q1 2016 against the alternative of non-referral, for each quarter up to Q4 2017.
  + A general principle is that after random assignment across multiple treatment statuses, a comparison of outcomes for any two treatment statuses is a valid estimator of impact (the impact of receiving the first treatment rather than the second).
  + The “intention to treat” principle would need to be applied as described above and also in the sense that those who dropped their claim before end 2017 (so that referral to JobPath was no longer possible) are retained in the calculation of control group outcomes.
  + Preallocation status would have to not be revealed prior to the (preallocated) date for referral, since an individuals’ knowledge of when they will be referred may affect their behaviour before the referral date.

### Implementation of virtual (simulated) preallocation of dates for future referral to JobPath

* It appears to be the case that, with some additional processing of the available statistics, impact on this basis could also be reported without literally preallocating a date for future referrals to the entire sample in Q1 2016. A retrospectively-simulated preallocation of future dates for referral can be used while retaining the statistical properties of an actual preallocation of future dates for referral. This is a valuable flexibility when dynamic random assignment is implemented in an ongoing operational intervention, rather than a fully-controlled experimental setting. The number of places that will be available for new referrals to JobPath in a future quarter is not known until shortly beforehand for various reasons, e.g. it takes time for providers to get their operations up to speed; the proportion of referrals that will result in commencement, providers’ capacity, and the average duration of participation in JobPath, are not predictable far in advance. Random assignment to the treatment will therefore be done each quarter across the entire group eligible for referral at that time.
* The following statistical procedure is proposed:
  + Records (i.e. lists of ID numbers) would be kept for the group eligible for referral in Q1 2016 and the subset of this group that was actually referred in Q1 2016, and similarly for subsequent quarters (including only the people who were already in the group eligible for referral in Q1 2016).
  + When Q2 2016 randomisation has taken place, the number of people who would have had to be pre-allocated in Q1 2016 to the group preallocated for referral in Q2 2016, in order to generate the outcome number of referrals, is calculated. It is the number actually referred in Q2 2016, multiplied by the ratio of the number eligible for referral but not actually referred in Q1 2016 to the number still eligible for referral in Q2 2016. This ratio is the inverse of the quarterly survival rate on (net of exits due to employment, transfer to a different benefit, emigration, etc.) on the Live Register (for the group “eligible for referral in Q1 2016 but not referred then”).
  + Individuals in the group that was eligible for referral in Q1 2016 but not referred then are selected at random and added to the group actually referred in Q2 2016, until a group of the right total size has been created, which can be labelled “pseudo-preallocated” in Q1 2016 to referral in Q2 2016.
  + Repeating this procedure each quarter (with the individuals “pseudo-preallocated” in Q1 2016 to referral in Q2 2016 removed from the pool available for “pseudo-preallocation” to referral in Q3 2016, etc), by end 2017 the whole sample that was available for randomisation in Q1 2016 will have been allocated across the groups “actually referred in Q1 2016”, “pseudo-preallocated to referral in Q2 2016”, “pseudo-preallocated to referral in Q3 2016”, etc., and a final (residual) group “pseudo-preallocated for non-referral up to end 2017”.
  + By the beauty of randomisation - and given that preallocation dates are not revealed in advance of the actual referral and therefore have no impact on outcomes until the referrals actually occur - a comparison of outcomes between any two of the “pseudo-preallocated” groups will have the same statistical properties as a comparison based on preallocations that were actually done in Q1 2016.
* A comparison of outcomes for the group actually referred in Q1 2016 vs. the group pseudo-preallocated to “non-referral up to end 2017” will then provide an unbiased estimate for the impact of referral to JobPath in Q1 2016 during the first 7 quarters after referral (although not including threat effects where the general risk of future referral to JobPath affects behaviour – threat effects might be the same for different treatment groups).

Comparisons of outcomes for groups other than “Q1 2016” vs. “non-referral up to end 2017” could be useful:

* + For averaging out random error, e.g. if the Q1 2016 sample is small, application of the procedure to the Q2 2016 sample would provide a useful second reading.
  + For examining how the impact of JobPath varies with elapsed duration on the LR: in the procedure described, only individuals in the group eligible for randomisation in Q1 2016 are included in outcome statistics for later quarters. Therefore a “Q2 2017 vs Q3 2017” comparison estimates impact for a target group that has been on the LR about year longer, on average, than a “Q2 2016 vs Q3 2016” comparison.

If JobPath remained in place for many years, the full set of historical data could allow modelling of how impact has varied with calendar date (the impact could change as the Jobpath system settles down, with the macroeconomic cycle, etc.), jobseeker duration on the LR at the time of referral, and variant treatments such as second referrals to JobPath. Randomisation makes it in principle possible to estimate the impact of multiple implementation parameters that vary within the data window.

### Repeat simulation runs

The simulated preallocation procedure involves adding randomly selected individuals (from the group that was eligible for referral in Q1 2016 but not actually referred in either Q1 2016 or Q2 2016), to make up the group “pseudo-preallocated” in Q1 2016 to referral in Q2 2016. Chance variation in the random selection process could be minimised by running the pseudo-preallocation randomisations repeatedly and taking an average of the outcomes. (Estimates from a single run are already unbiased, but estimates averaging across multiple runs will have lower variance).

**Subgroup analysis**

Since the simulated preallocation procedure identifies specific sets of individuals making up the treatment and control groups, outcomes can easily be tablulated for subgroups defined by individual variables both time-invariants (sex, date of birth, etc.) and recent-event variables (e.g. first child born less than 4 years before Q1 2016), insofar as sufficient data are available. This may be operationally more helpful than regression analysis that uses these as explanatory variables - although one approach does not preclude the other.

### Applicability to a wide range of ongoing operations

The “pseudo-preallocation” analysis that identifies a specific set of individuals as the control group would enhance the value of embedding randomised assignments to a treatment into a wide range of ongoing operations.

* For example, a national health service policy could be that
* men at risk of prostate cancer who test with a PSA (prostate-specific antigen) level above a high threshold are immediately referred to an intensive treatment regime
* men with a PSA level below a low threshold are dropped from monitoring
* men with an intermediate level are referred at random (each quarter, or each week) to a service that offers intensive treatment: the proportion referred might vary depending e.g. on capacity available in the intensive treatment service.

In this case, a “pseudo-preallocation” analysis would give an unbiased estimate for the future mortality and quality-of-life profiles of the members of the intermediate group, comparing those who were referred to the intensive treatment service immediately with those who were not referred for at least the next several years.

In many areas of life, wisdom about the effectiveness of different approaches still lies largely with a few individuals who have closely observed the implementation of different approaches and the results for many years. Where a systematic management framework and outcome measures exist –which might be in just one region, or just operations of one company - dynamic random assignment across certain alternative treatments could be feasible. For example, in agriculture varying the fertilisers or crop rotation sequences; in retail management, allowing some outlets to use different staff hiring criteria or sales promotion methods.

### A comparison with inverse probability weighting of dynamic treatment assignment situations

Vikstrom (2017) considers an environment in which treatment can occur at any point in time, and the probability of allocation to a treatment at each time can be modelled as a function of covariates in the available data set. Vikstrom’s description of the impact estimates that can be generated, and the conditions for their validity, has parallels with the description here of situations where the dynamic treatment assignment is randomised.

* Vikstrom (2017) provides a “dynamic inverse probability weighting (DIPW) estimator for the average treatment eﬀect on the treated for treatment in a certain period against no treatment now nor thereafter”.
  + Vikstrom’s estimator is thus a solution, in a related context, to the “dilemma” described in DEASP (2019).
  + Vikstrom’s “inverse probability weighting” refers to the “probability of obtaining treatment in period s given survival until time period s and covariates X”. This resembles the approach described here, where the number of individuals simulated to have been preallocated to a given treatment date is the number actually treated at that date, multiplied by the inverse probability that the preallocation will actually result in the treatment (given that not all individuals will remain in the eligible group at the preallocated treatment date).
  + Vikstrom notes that “in each period only not-yet treated individuals are used, so that the control group successively changes as some previously non-treated individuals start treatment”. This resembles the approach described here, where the individuals simulated to have been preallocated to a given quarter are a random selection across all who do not yet have a simulated preallocated or an actual out-turn treatment date. In Vikstrom’s approach some individuals probably appear in several control groups with fractional weights, while in the approach described here each individual is (in a single run) simulated to have been randomly preallocated to a specific control group, a difference that would make the results slightly more variable but not biased.
* Vikstrom (2017) specifies an assumption required for validity that “for unemployed workers still unemployed and non-treated at t, treatment assignments in period t are independent of the potential outcomes (re-employment rates) after conditioning on covariates measured shortly before t”. The randomisation of assignments ensures that they are independent of re-employment rates, so Vikstrom’s estimator should be applicable to situations of dynamic random assignment. Under random assignment, potential covariates (other than the date) do not affect the probability of assignment: so the regressions that implement Vikstrom’s estimator will predict the same probability of treatment for all individuals who remain in the control group at a given date.

### The case for random assigned variation within ongoing operations

Although the Vikstrom (2017) procedure can be implemented without random assignment, there is no general reason to expect that the assumptions necessary for its validity hold in practice. In some countries, the public employment service (PES) funds short-term training for unemployed people that qualifies them to fill vacancies created by a large employer recently arrived in the area, so that - through time and across individuals - assignment to the treatment (short-term training) is strongly correlated with future employment rates, but this correlation is not necessarily causal. There are various scenarios for re-employment probabilities to be correlated with, but not necessarily caused by, participation in an ALMP, and the Vikstrom procedure without random assignment will not solve this problem.

Random assignment experiments may give misleading results by delivering the treatment in contexts where it would probably not be delivered in operational practice. For example training in a skill that is only demanded by employers in one region may in fact be effective when it is implemented in that region, but appear to be ineffective in a random assignment experiment that implements it in all regions. This example illustrates how when treatments are randomly-assigned, they still need to be implemented in a realistic manner. This is an argument for bringing random assignment into operational procedures rather than using it only distinct experimental settings.

### References

DEASP (Department of Employment Affairs and Social Protection) 2019, “Working paper: Evaluation of JobPath outcomes for Q1 2016 participants”, March, [www.welfare.ie/en/pdf/JobPath\_econometric\_impact\_evaluation\_DEASP\_working\_paper.pdf](http://www.welfare.ie/en/pdf/JobPath_econometric_impact_evaluation_DEASP_working_paper.pdf) (or the parent page [www.gov.ie/en/publication/0939ba-working-paper-evaluation-of-jobpath-outcomes-for-q1-2016-participant](http://www.gov.ie/en/publication/0939ba-working-paper-evaluation-of-jobpath-outcomes-for-q1-2016-participant) which links to <https://assets.gov.ie/36499/ffdce98cddc34addb05cf41a70aaf4e7.pdf>.

Vikstrom, J. (2017), “Dynamic treatment assignment and evaluation of active labor market policies”, Labour Economics 49, pp. 42–54, <https://faculty.smu.edu/millimet/classes/eco7377/papers/vikstrom%202017.pdf>.