Pure-Chance Jobs vs. a Labor Market:

The Impact on Careers of a Random Serial Dictatorship for First Job Seekers

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Does a worker's first job affect her long-run career trajectory? This depends on defining characteristics of the labor market she enters into. Do such *first job effects* vary across workers of different types? This in turn determines the scope for policy to affect the aggregate realized effects of workers' first jobs by altering initial matches between workers and employers. Do policy options that would improve upon a normal, "free" labor market actually exist? This depends on how well decentralized labor markets function relative to feasible alternatives, such as the centralized assignment mechanisms used in the public sector in many countries, or the coordinated job market for academic economists.[1]

The answer to the first question above appears to be "yes", but the existing evidence comes from cohort- and group-level variation in labor market conditions upon entry (see e.g. Oyer, 2006; Kahn, 2010; Heisz et al., 2012; Arellano-Bover, 2020; Staiger, 2020). We are not aware of existing, causal evidence on the career consequences of first jobs resulting from shocks to the choices available to individual job seekers.[2] More importantly, "market design" policy's impact on entry-level labor markets' aggregate performance—as measured through first job effects—has yet to be studied by economists.

In this paper we take advantage of Norway's use of a Random Serial Dictatorship (RSD) for assigning first jobs to doctors—residencies—and its replacement with a decentralized labor market in 2013.[3] We first estimate the consequences for long-run earnings of each type of initial employer (hospital) for each type of worker (doctor), focusing on in-demand vs. less desired employers and male vs. female workers. We do so by exploiting RSD-generated random, individual level variation in workers' initial choice set over employers. We then decompose preferences over employers into a component that is due to first job effects and another that is due to the "amenity value" workers of a given type associate with employers of a given type. Finally, we show how realized first job effects and amenity values differ—for each group of workers and in total—in the two systems, by describing how matches changed after 2013.

I. Setting

Equitable access to health care and to desirable first jobs was the main motivation behind the RSD system used to match medical school graduates with 18-month residency positions in Norway from 1954 to 2013. Graduates simply chose a residency hospital from the remaining positions when their lottery draw's turn was up, in Norway's RSD. We thus observe each candidate's most preferred option from their choice set, and can recover the full choice set itself.

In 2013, the RSD system was discountinued. Graduates now apply to residencies directly as in a normal labor market, and hospitals are responsible for selection and recruitment.

We obtained the draws of all lottery participants who were assigned a residency during 1992-2013 from the Norwegian Registration Authority for Health Personnel. We linked these with the employer-employee registry and other administrative registers provided by Statis-

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[1] At least thirty countries assign workers in some public service occupations to first jobs through centralized mechanisms.

[2] An exception is Angrist (1990)'s seminal study of the Vietnam draft.

[3] An RSD mechanism starts with a lottery. The person who draws #1 chooses her preferred object freely among all options. #2 then chooses among the remaining objects, and so on. Among other desirable properties, the RSD is incentive compatible (Abdulkadiroğlu and Sönmez, 1998).

tics Norway to match graduates to their first employer and data on their post-residency careers.[4]

We observe 40 biannual cohorts of graduates—9,049 workers—start their careers during the 1992-2012 RSD period, and nine cohorts—2,903 workers—during the 2013-2017 post-RSD period.[5] The 55 and 50 employers observed to employ residents pre- and post-reform are very similar in size, location, and popularity among RSD-participants. Graduates are also very similar during the two periods in age—28 years old on average—and origin—about 83% are Norwegian born—though slightly more female post-reform (61%, vs. 54% pre-reform).

II. Empirical strategy

Intuitively, randomization ensures that a graduate's choice set over first jobs is independent of her characteristics. At the same time, her choice is constrained to be in the choice set, which will depend on the graduate's place in line. This allows us to construct instruments for first job effects (FJEs) that are both exogenous and relevant. However, since we do not observe workers' rankings over employers, heterogeneity in preferences requires care in our IV strategy.

We first focus on a single instance of the lottery (cohort of graduates) to clarify the exposition. Let h denote employers, $\mathcal{H}$ the set of all employers, and i workers. Let $C_i \subseteq \mathcal{H}$ be a worker's realized choice set, and H_i their realized choice from C_i. Let N be the number of workers and $J = |\mathcal{H}|$ the number of employers.

A. *Exogeneity of choice sets*

With a finite number of participants in the lottery, it does not immediately follow from randomization that a worker i's probability distribution over possible choice sets C_i is perfectly independent of her characteristics. Rather, any two workers having *the same preferences* will receive a C_i drawn from the same probability distribution (Abdulkadiroğlu et al. 2017). Thus choice sets are independent of potential outcomes, conditional on preferences. However preferences are not (fully) observed, so we cannot directly control for them.

We first partition workers into a set of observable groups $g \in G_i$. Consider the IV independence assumption, conditioning on G_i:

ASSUMPTION 1 (exclusion and independence): *For any $h \in \mathcal{H}$ and $c \subseteq \mathcal{H}$:*

(i) $Y_i(h, c) = Y_i(h)$

(ii) $(Y_i(h) \perp C_i)\,|G_i$

where $Y_i(h, c)$ is the potential outcome (for now, earnings five years after graduation) of worker i if they are placed at employer h, and have choice set c.[6] Item (i) is the exclusion restriction, in our case that a worker's choice set from the lottery does not affect her outcomes except through her chosen first job employer. Item (ii) states that the lottery randomized choice sets within each demographic group, and follows from Proposition 1 of Abdulkadiroğlu et al. (2017) if preferences are homogeneous within groups.

When the number of workers is "large" compared to the number of employers, Assumption 1(ii) will hold approximately even when there is heterogeneity in preferences within groups. Formally, the actual set of N workers is viewed as a sample from an underlying continuum of workers, with each employer accounting for a fixed proportion of the available jobs. In this "continuum economy", choice sets are random unconditionally, and IV estimation is consistent along an asymptotic sequence in which $N \to \infty$ with the share of jobs belonging to each employer fixed. In our full-length paper we provide simulation evidence that this asymptotic approximation is a good one in our context.

B. *Choice sets as instruments*

Given Assumption 1, we can use features of a worker's choice set C_i to construct instruments for the causal effect of her first job. We focus on the quantity $\mu_{gh} = \mathbb{E}[Y_i(h)|G_i = g]$. μ_{gh} is the average counterfactual outcome that would occur for a worker in group g if their first job were at employer h, with $\mu_{gh'} - \mu_{gh}$ being the average effect of moving workers in g from employer h to employer h'.

[4] Doctors' post-residency earnings vary more in Norway than one might expect in a public health care system.

[5] We exclude graduates belonging to special lottery categories, and one hospital with missing information.

[6] We leave out the first 2013 graduating cohort—the last RSD cohort—from our analysis because we do not observe their earnings five years after graduation.

We represent a choice set C_i as a vector of indicators Z_{hi} for the presence of each employer h in C_i, i.e. $Z_{hi} = \mathbb{1}(\text{hospital } h \text{ is in } C_i)$. A realization of C_i is equivalent to a realization of the full vector $\mathbf{Z}_i := (Z_{1i}, Z_{2i} \ldots Z_{Ji})'$, for some arbitrary ordering of the employers.

It is natural to expect that the Z_{hi} instruments may be informative about causal effects only among a group of relevant compliers. However, our setup does not fit neatly into the seminal local average treatment effects framework of Angrist and Imbens (1994) or its generalization to multi-valued treatments. This prevents us from using existing methods allowing for fully heterogenous treatment effects. Restricting selection on treatment effect heterogeneity as follows nevertheless justifies standard two-stage least squares estimation (cf. Kolesár 2013):

ASSUMPTION 2 (limited selection on gains): *For any h_0, h_1, h, c, g, the quantity:*

$$\mathbb{E}[Y_i(h_1) - Y_i(h_0)|H_i = h, C_i = c, G_i = g]$$

depends only on h_1, h_0, and g.

Assumption 2 states that for any pair of employers h_0 and h_1, the contrast $Y_i(h_1) - Y_i(h_0)$ is not correlated with actual employer choice H_i within a group. This rules out selection on unobserved heterogeneity in gains within observable group—what Heckman et al. (2006) call *essential* heterogeneity. Assumption 2 does however allow for sorting on levels—that workers choosing $H_i = h$ have a different average value of $Y_i(h)$ than those who do not—and is substantively weaker than assuming homogeneous treatment effects within groups.

Let $\mathbf{D}_i$ be a vector of D_{hi} across all employers, where $D_{hi} := D_{hi}(\mathbf{Z}_i)$ indicates that worker i chooses employer h. The random vector $\mathbf{D}_i$ encodes the same information as H_i. For any group g, let $\Sigma_g = \mathbb{E}[\mathbf{Z}_i\mathbf{D}_i'|G_i = g]$.

ASSUMPTION 3 (relevance): *Σ_g has full rank for each $g \in \mathcal{G}$.*

Similarly, we collect the μ_{gh} over all the employers into a vector $\boldsymbol{\mu}_g$, for any group g. Proposition 1 shows that the assumptions given are sufficient to identify this full vector of first job effects for each group:

PROPOSITION 1 (identification of FJE's): *Under Assumptions 1-3, for each $g \in \mathcal{G}$:*

$$\boldsymbol{\mu}_g = \Sigma_g^{-1}\mathbb{E}[\mathbf{Z}_i Y_i|G_i = g]$$

Proposition 1 reflects the standard identification logic for linear IV models via moment conditions, and the proof generalizes that of Kolesár (2013) from the case of an ordered treatment. In our case, for each fixed g we have J endogenous parameters μ_{hg}, and J binary instruments Z_{hi}.

The vector $\boldsymbol{\mu}_g$ across all employers is thus identified for each demographic group g. In practice we pool across lotteries and combine the 55 employers into *categories* of employers. To demonstrate the approach, we take just four such categories, defined by employers' overall desirability.[7] The notes to Table 1 report some observable characteristics of the categories. Employers within a category are treated as a single employer, and thus each $\boldsymbol{\mu}_g$ becomes a four-component vector. We also focus here on just two groups g of workers: male and female.

Although Assumption 1 only holds within an instance of the lottery, pooling cohorts is justified upon inclusion of cohort fixed-effects in our two-stage least squares estimation, provided that FJEs are stable across cohorts. We present the results in Section IV.

III. Preferences and the consequences of decentralization

The replacement of the RSD with a decentralized labor market in 2013 affected outcomes by changing the distribution of choice sets each group of workers face, and hence actual employer matches. To calculate the implied welfare changes for workers, we take preferences defined over employer categories to have the form:

$$U_i(h) = \mu_{G_i h} + A_{G_i h} + \eta_{hi} \tag{1}$$

where μ_{gh} is the first job effect of category h for group g, A_{gh} is the average "amenity" value of employer category h, and $\mathbb{E}[\eta_{hi}|G_i = g] = 0$

[7] We first define Category 4 as hospitals in Finnmark and Sogn og Fjordane (cf. Section III). Employers in these remote regions of northern and western Norway are especially unappealing to most graduates. We then compute the average lottery draw of workers that choose each remaining employer and separate into terciles: Category 1 is hospitals with the lowest (best) average lottery number, Category 2 with the next best, etc.

for each h and g.[8] The term $v_{gh} := \mu_{gh} + A_{gh}$ represents a systematic component of utility for employer h in group g, while η_{hi} captures individual heterogeneity in preferences. This allows "typical" preferences to differ flexibly between genders through the $A_{G_i h}$, and higher moments of η_{ih} may also depend on G_i. We take workers to anticipate mean earnings within their group at a given employer, so that $\mu_{G_i h} = \mathbb{E}[Y_i(h)|G_i]$ appears in utility rather than $Y_i(h)$ itself.

The quasi-linearity assumption pins down a scale for utility (such that it is measured in dollars), but we are still free to fix a location normalization. For an arbitrary employer category h_0, define $U_i(h_0) = 0$ for all i. This yields the following interpretation of amenities: A_{gh} is the average amount in excess of their expected earnings μ_{gh} at h that group g workers would be willing to pay to move from h_0 to h. In practice, we choose h_0 to represent Category 4.

Let R_i be worker i's random lottery number draw, normalized to the unit interval within each lottery, and define $r_{gh} := \mathbb{E}[R_i|H_i = h, G_i = g]$. We make the following assumption:

ASSUMPTION 4: $r_{gh} = \alpha_g - \beta \cdot v_{gh}$ *for some* $\beta > 0$ *and* α_g.

Intuitively, the average lottery number of workers choosing h is a proxy for their preference v_{gh} for that employer. If two employers share a value of r_{gh} (for some g), but differ in their FJEs μ_{gh}, then the difference in amenities at the two employers must offset this difference.[9]

Our goal is to use the estimated FJE's and observable r_{gh} to pin down the α_g and β, and hence the amenities A_{gh}. With four employer categories, two worker groups, and utility normalization implying $A_{gh_0} = -\mu_{gh_0}$ for each g, Assumption 4 involves nine unknowns (six A_{gh}, two α_g, and β), from eight equations. We further assume that men and women exhibit the same willingness to pay—in excess of the earnings difference—for the mostly large, urban employers in Category 1, compared with the smaller, rural employers in Category 4, enabling identification. We discuss this assumption, which on the basis of the data we can infer must hold approximately, further in our full-length paper.

Estimates of amenities A_{gh} allow us to construct v_{hg} for each group h, allowing us to approximate worker welfare and its μ_{gh} and A_{gh} components under the RSD system and under the distribution of workers over employers observed in the post-reform labor market.

IV. Results

We estimate that, relative to Category 4 employers, a first job in the most-in-demand employer category raises annual earnings five years post-graduation by about \$28,000, as seen in Table 1. The corresponding estimates for categories 3 and 2 are \$38,000 and (an insignificant) \$16,000. The estimated $\mu_{gh} = \mathbb{E}[Y_i(h)|G_i = g]$ reveal an annual earnings gap five years out of at least $\$20,000$ between men and women—about 13%—across first employers.

Amenities fall in the range $[-\mu_{gh}, 0]$, indicating that workers would give up some fraction of the earnings at their chosen employer to remain there instead of moving to Category 4. The A_{gh} are generally increasing (decreasing in magnitude) in category popularity while earnings FJEs exhibit a flatter trend. Workers' combined surplus falls at nearly identical rates between men and women as a function of average lottery draw.

Overall, both men and women lose persistent earnings effects, while gaining—to a greater extent—in employer amenities, with the post-reform distribution of workers over employers. The net effect of the decentralized labor market on worker welfare is positive but not large, representing about 4.67% of the pre-reform average of v_{gh}. Men gain more than women in employer amenities.

We conclude that, in the setting we study, first jobs affect workers' long-run career trajectories; they do so differentially for men and women; and "market design" policy can affect the aggregate realized effects of workers' first jobs.

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[8] Equation 1 can be obtained from a general additive-in-FJEs form: $U_i(h) = \mu_{G_i h} + \epsilon_{ih}$ with some generic ϵ_{ih} if we define $A_{gh} = \mathbb{E}[\epsilon_{hi}|G_i = g]$ and $\eta_{hi} := \epsilon_{ih} - A_{G_i h}$.

[9] This intuition supports assuming $r_{gh} = \phi_g(v_{gh}, \cdot)$ for some decreasing function ϕ_g that itself depends on the other v_{hg}, the distribution of (η_{ih}, G_i), and the number of slots available for each h. But even parametric assumptions on η_{hi} (such as the logit model) do not appear to readily imply reduced-form expressions for ϕ_g. Assumption 4 reflects the simplest functional form assumption that can reasonably fit the data. Note that linearity can only hold as an approximation for some range of v_{gh}. In practice, our r_{gh} range between 0.34 and 0.75.

TABLE 1—FIRST JOB EFFECTS ON EARNINGS AND THE IMPACT OF DECENTRALIZATION

Employers					Distribution of Workers (%)			
		FJEs vs. Cat. 4	FJEs	Amenity	Pre-Reform (RSD)		Post-Reform	
Category	Rank	Pooled	By Gender	Values	Women	Men	Women	Men
1	0.17	28.21* (14.64)	W: 145.07 M: 170.71	W: -64.54 M: -90.87	33.84	33.45	36.89	39.22
2	0.44	16.03 (13.00)	W: 126.38 M: 174.83	W: -78.70 M: -125.98	37.90	35.38	37.62	34.05
3	0.73	37.70** (18.99)	W: 150.61 M: 182.04	W: -130.37 M: -164.72	22.47	24.46	19.31	20.94
4	0.89	-	W: 122.89 M: 149.22	W: -122.89 M: -149.22	5.79	6.70	6.18	5.79
Average Predicted Earnings (5 Years Out)					137.95	173.68	137.74	173.06
Average Post-Reform Difference							-0.21	-0.61
Average Predicted Amenity Values					-88.10	-125.39	-86.19	-121.53
Average Post-Reform Difference							1.92	3.86
Total Change in Welfare (Per Worker)							1.71	3.25
Workers			9,049	9,049	4,855	4,194	1,781	1,122

Categories 1, 2, 3, and 4 include 17, 17, 16, and 5 employers; average # of employees (and proportion urban hospitals) are 1634 (0.82), 1457 (0.65), 453 (0.50), and 502 (0.20). Category 4 are employers in Finnmark and Sogn og Fjordane (see Section III). FJEs measure the impact of a first job in each category on earnings 5 years post-graduation (timeframe chosen to maximize the # of workers that can be included in the analysis). First stage F-statistics for Category 1, 2, and 3 are 322.65, 327.44, and 102.40. Earnings and amenity values in thousands of 2020 USD. Pre/post-reform total welfare$_g$ = $\sum_h prob(h|g)(\mu_{gh} + A_{gh})$ where $prob(h|g)$ = columns 6-7/8-9. Robust standard errors in parentheses. *** p<0.01 ** p<0.05 * p<0.10.

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