DISCUSSION PAPER SERIES

IZA DP No. 804

Dynamic Econometric Program Evaluation

Jaap H. Abbring

June 2003

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Dynamic Econometric Program Evaluation

Jaap H. Abbring
Free University of Amsterdam and IZA Bonn

Discussion Paper No. 804
June 2003

IZA

P.O. Box 7240
D-53072 Bonn
Germany

Tel.: +49-228-3894-0
Fax: +49-228-3894-210
Email: iza@iza.org

IZA Discussion Paper No. 804
June 2003

ABSTRACT

Dynamic Econometric Program Evaluation*

H. Theil has made important contributions to the analysis of simultaneous-equations models. This paper gives an exposition of some closely related recent developments in micro-econometrics, with a focus on efforts to develop robust methods for dynamic policy evaluation. We set the stage with a brief discussion of the static treatment-effect approach to program evaluation and non-parametric structural models. We then critically analyze the dynamic treatment-effects approach adopted from statistics. Finally, we review the event-history approach. We clarify some of the fundamental problems that arise in the analysis of such models by rephrasing a canonical version as a simultaneous-equations model.

JEL Classification: C14, C30, C41

Keywords: dynamic policy evaluation, structural models, treatment effects, event-history analysis

Jaap H. Abbring
Department of Economics
Free University of Amsterdam
De Boelelaan 1105
1081 HV Amsterdam
The Netherlands
Email: jabbring@econ.vu.nl

* This paper was prepared for the H. Theil Memorial Conference in Amsterdam, 16-18 August 2002. Part of this paper was written while I was visiting the Department of Economics of University College London and the Institute for Fiscal Studies. My research is supported by a fellowship of the Royal Netherlands Academy of Arts and Sciences. Thanks to James Heckman, Geert Ridder, Gerard van den Berg, and Bas van der Klaauw for very helpful discussions and comments.

1 Introduction

Economists have traditionally addressed the problem of causal inference by specifying and estimating structural models. This approach was pioneered in the 1920s and 1930s, then rigorously pursued in influential work by the Cowles Commission, and later enriched with more sophisticated dynamics.[1] Theil has made important contributions to this literature, notably the two-stage least-squares estimator (Theil, 1953), and the three-stage least squares estimator (Zellner and Theil, 1962). Theil's (1971) classic text book has taught many econometricians, among other things, the principles underlying these methods. This paper gives an exposition of some closely related recent developments in micro-econometrics, with a focus on efforts to develop robust methods for dynamic policy evaluation.

The paper is organized as follows. Section 2 sets the stage with a brief discussion of the static treatment-effect approach to program evaluation and recent work on non-parametric structural models. We stress the close connection between a stability concept used in statistics and the concept of autonomy used in econometric structural-equations modelling. We briefly consider what can be learned from inference on non-parametric structural models. We conclude that reduced-form analysis has gained importance relative to structural analysis with the move away from parametric, linear models.

In Section 3, we critically analyze the dynamic treatment-effects approach to policy evaluation adopted from statistics. We stress the importance of accounting for the information structure of the programs evaluated. In particular, the Lucas (1976) critique of early structural econometrics applies to the current generation of dynamic treatment-effects models. Methods for computing the optimal dynamic assignment of programs developed in statistics are not directly useful to economic policy makers.

Section 4 reviews an alternative, event-history approach to the microeconometric evaluation problem. We clarify some of the fundamental problems that arise in the analysis of such models by rephrasing a canonical version as a simultaneous-equations model.

Finally, Section 5 concludes.

[1] See Heckman (2000), who reflects extensively on the twentieth-century history of causal analysis, in particular policy evaluation, in economics. Goldberger (1972) provides a historical account of structural-equations methods.

2 Some background

2.1 A continued interest in structural-equations models

The early structural-equations models were typically not based on explicit models of the behavior of individual agents. Also, strong restrictions were placed on functional forms— models were typically linear with normal errors. Over time, structural models have gained considerably in economic-theoretic sophistication. In particular, dynamic structural models with forward-looking agents and heterogeneity are routinely used now. However, for the sake of tractability and computational convenience, they have mostly remained low-dimensional.[2]

The recent "treatment-effects" literature in econometrics seeks to evaluate programs without making the strong functional-form assumptions that are typically made in structural econometrics. Most of this literature focuses on static problems and econometricians have developed a wide variety of robust statistical methods based on instrumental-variables and conditional-independence assumptions for this case.[3] Their statistical robustness has brought these methods wide popularity, but the lack of economic-theoretical foundations has sparked discussions on their economic relevance.[4]

Along with the treatment-effects literature, a closely related literature on explicit non-parametric structural models has developed. Again, the focus is on robust analysis of relatively simple, static problems. This literature is, more explicitly than the treatment-effects literature, grounded in the earlier literature on structural-equations models. Among its advances are the development of non-parametric two-stage methods that are closely related to Theil's (1953) two-stage least squares method.[5]

2.2 Randomized experiments

The static potential-outcome framework of Neyman (1923) and Rubin (1974) is extensively used in the statistical and econometric evaluation literature. We briefly discuss it here to introduce some basic concepts and notation.

Suppose we are interested in the causal effect of a treatment on some outcome. The

[2]Rust (1994) discusses the estimation of sophisticated, but usually highly parametric, dynamic structural models. More recently, structural econometrics has turned to the specification and identification of dynamic structural models that are less tightly specified. For example, Taber (2000) and Magnac and Thesmar (2002) discuss identification of dynamic discrete-choice models under general conditions. Another example is Abbring and Campbell (2003), who discuss identification of structural models of firm growth, learning, and survival.

[3]Heckman, LaLonde and Smith (1999) provide a review of the use of such methods in labor economics.

[4]See *e.g.* Heckman (1997), Heckman and Vytlacil (2000a), and Rosenzweig and Wolpin (2000).

[5]See *e.g.* Angrist and Imbens (1995), Blundell and Powell (2000), and Imbens and Newey (2001).

treatment takes its values in some set $\mathcal{D}$. In the most basic setup, $\mathcal{D} = \{0, 1\}$. Then, the point 1 represents assignment to a treatment group and the point 0 assignment to a control group. Alternatively $\mathcal{D}$ could be $\mathbb{R}_+ := [0, \infty)$, representing a continuum of doses of some medication. To each treatment $d \in \mathcal{D}$ corresponds a random variable Y_d, the potential outcome in the case that we would intervene and assign treatment d. The randomness of Y_d may represent both *ex ante* heterogeneity between individuals and *ex post* shocks.

Causal inference is concerned with contrasting potential outcomes corresponding to different treatments. Because the treatments are mutually exclusive, we can never observe potential outcomes corresponding to different treatments simultaneously. In the words of Dawid (2000), potential outcomes are complementary. This is what Holland (1986) calls the "fundamental problem of causal inference".

Suppose that treatment is assigned according to a $\mathcal{D}$-valued random variable D. Again, the randomness in D may reflect both *ex ante* heterogeneity that affects the assignment of treatment, and *ex post* shocks like those arising from explicit randomization of treatment assignment. The actual outcome Y and the potential outcomes $\{Y_d\}$ are linked by a natural consistency condition,[6]

Assumption 1. *Consistency.* $Y = Y_D$.

Assumption 1 states that the actual outcome is simply the potential outcome corresponding to the treatment actually assigned to the individual.

In a randomized experiment, which is the focus of Neyman (1923) and Rubin (1974), $D \perp\!\!\!\perp \{Y_d\}$. For now, we maintain this assumption. Suppose that we are interested in the effect of the treatment on the means $\mu(d) := \mathbb{E}[Y_d]$, $d \in \mathcal{D}$. In a randomized experiment, $\mu(D) = \mathbb{E}[Y|D]$ almost surely. So, under some additional smoothness assumptions, we can estimate μ by standard non-parametric regression techniques.

The consistency condition in Assumption 1 is closely connected to Rubin's (1986) stable-unit-treatment-value assumption (SUTVA). SUTVA requires that potential outcomes for any given subject are independent of the treatment assignment mechanism and of the treatments assigned to other subjects. Thus, SUTVA ensures that we do not have to index potential outcomes by treatments assigned to other subjects or by the assignment mechanism used. More generally, it requires that all (versions of) treatments are represented in $\mathcal{D}$. In economics, violations of SUTVA typically arise if we only index treatments by the agents' own treatment and there are (*e.g.* strategic) interactions between agents

[6] We assume that $\{Y_d; d \in \mathcal{D}\}$ is a measurable stochastic process. This ensures that Y_D is a random variable (by application of Billingsley, 1995, Theorem 13.1). In the binary-treatment case this is trivial. In the sequel, we will implicitly assume that such measurability issues are settled.

or equilibrium effects of the program under study. We will argue later that equilibrium effects are particularly important in dynamic econometric program evaluation.

SUTVA ensures that we can specify $\{Y_d\}$ independently of the treatment assignment rule D. Economists would rather say that $\{Y_d\}$ is autonomous, or structurally invariant (Frisch, 1938; see also Aldrich, 1989, and Hendry and Morgan, 1995). Indeed, under randomized assignment and SUTVA, we can interpret the Neyman-Rubin framework as a completely specified non-parametric structural model.[7] First, the process $\{Y_d\}$ is, of course, simply a random function $d \in \mathcal{D} \mapsto Y_d$ and therefore, under SUTVA, a non-parametric structural equation with non-separable errors. Second, the most obvious interpretation of randomized assignment is that treatment is not causally affected by the outcome and that there are no common determinants of treatment and outcome. Formally, we could posit a second autonomous function $y \in \mathcal{Y} \mapsto D_y$ that gives the treatment for each hypothetically assigned outcome, and add a consistency condition $D = D_Y$. Then, in our interpretation randomized assignment boils down two the assumptions that (*i*) $\{D_y\} \perp\!\!\!\perp \{Y_d\}$ and (*ii*) $D_y = D_{y'}$ for all $y, y' \in \mathcal{Y}$. Altogether, this gives a recursive structural model $(\{Y_d\}, D)$ with independent, non-separable errors.

The Neyman-Rubin model can be enriched by including covariates X that are not causally affected by either treatment or outcomes, but that may affect both treatment and outcomes. Analogously to the simple model above, we can assume that treatment assignment is randomized conditional on X. In obvious notation, this gives a model $(\{Y_{dx}\}, \{D_x\}, X)$ with $\{Y_{dx}\}$, $\{D_x\}$ and X independent. This framework allows for the evaluation of policies that involve profiling on X in the assignment of D. Even this extended framework, however, does not cover data nor policies that involve (self-)selection on unobservables. These are typically of considerable importance in economics, and the applicability of this framework is therefore limited (*e.g.* Heckman and Smith, 1995 and 1997). We postpone further discussion of observed covariates to Section 3, and first discuss some models that allow for selection on unobservables.

2.3 Instrumental variables

In response to the limitation of the basic Neyman-Rubin framework to (stratified) randomized experiments, statisticians and econometricians have extended the framework to include instrumental variables. Here, we follow the expositions of Imbens and Angrist (1994) and Heckman and Vytlacil (2000b), which cover the case of a binary treatment (*i.e.* $\mathcal{D} = \{0, 1\}$).

The Neyman-Rubin model with instrumental variables Z can be represented as $(\{Y_d\}, \{D_z\}, Z)$,

[7] The close link between the potential-outcomes model and structural models has been discussed by *e.g.* Pearl (2000).

with $D_z = I(p(z) \geq U)$, $Z \perp\!\!\!\perp (\{Y_d\}, U)$, and consistency of potential and actual treatments and outcomes:

Assumption 2. *Consistency.* $D = D_Z$ and $Y = Y_D$.

Here, $p(Z) = \Pr(D = 1|Z)$ is the propensity score at Z, and U is a uniformly distributed random variable. Z causally affects treatment D, but does not affect the outcome Y directly and is not related to the error in the structural equation $\{Y_d\}$. The latent-variable representation of $\{D_z\}$ embodies Imbens and Angrist's (1994) monotonicity assumption without imposing additional structure on the model (Vytlacil, 2002).

Unlike the model of the previous subsection, this model allows for dependence of $\{Y_d\}$ and $\{D_z\}$. However, it does not specify the source of this dependence. The most obvious interpretation of the model is that it is a reduced form of a recursive structural model $(\{Y_{dv}\}, \{D_{zv}\}, Z, V)$ with $\{Y_{dv}\}$, $\{D_{zv}\}$, Z, V all independent and V some random variable that is unobserved to the analyst (*e.g.* Heckman and Vytlacil, 2000a).[8] However, in closely related settings with a continuous D, econometricians have entertained a simultaneous-equations interpretation of the dependence of $\{Y_d\}$ and $\{D_z\}$ (*e.g.* Blundell and Powell, 2000). Without imposing further structure, we can learn about the causal effects of the instrument Z on Y from the reduced form $(\{(Y_{D_z}, D_z)\}, Z)$ in which Y and D are jointly determined by Z.[9] If Z is a policy instrument, and not just a statistical instrument, the reduced-form effect of Z on Y is of direct policy interest. However, without further structure, an empirical analysis of the model is not informative on the effects of manipulations of the treatment D that cannot be expressed in terms of Z. In this sense, the traditional objective of linear instrumental-variables analysis seems to have been lost in the drive to non-parametric methods, in favor of reduced-form analysis.

The debate on this issue has focused on the multiplicity of treatment-effect parameters that can be defined in the present context, and the lack of invariance of certain parameters to the choice of instruments Z (see Heckman, 1997, and the discussion following it). In this light, Heckman and Vytlacil (2001) argue that econometric interest should focus on *policy-relevant* treatment effects (PRTEs). A PRTE can be defined as the mean causal effect on the outcome Y of changing the distribution of the instrument Z from some

[8]The vector V may include observed covariates, which we ignore for now. Even then, V is typically in part unobserved as it has include sufficiently many variables to make all assumed independencies hold. Heckman and Vytlacil (2000a) ensure this by taken V so large that $\{Y_{dv}\}$ and $\{D_{zv}\}$ are degenerate. Alternatively, one may leave scope for external *ex post* random shocks.

[9]It is a text-book fact that the reduced form is a useful tool for analyzing the effect of an "exogenous" variable (*e.g.* Theil, 1971, Section 9.1). In the present general setting, this has been pointed out by Heckman (1997). Angrist, Imbens and Rubin (1996) discuss the case in which Z is an intention-to-treat variable under control of the analist, and D is actual treatment. Also, Blundell and Powell (2000) discuss this issue in their slightly different framework.

distribution G to some other distribution G^*,

$$\text{PRTE}_{G,G^*} := \int \mu(z) dG^*(z) - \int \mu(z) dG(z),$$

with $\mu(z)$ now defined as $\mathbb{E}[Y_{D_z}]$. Because $Z \perp\!\!\!\perp \{Y_{D_z}\}$, we have that $\mathbb{E}[\mu(Z)] = \mathbb{E}[Y|Z]$ almost surely, in analogy to the case of a randomized experiment. Therefore, under some obvious support conditions, the PRTE of changing the distribution of Z from G to G^* can be directly identified from the reduced form regression $\mathbb{E}[Y|Z]$, and is given by

$$\text{PRTE}_{G,G^*} := \int \mathbb{E}[Y|Z=z] dG^*(z) - \int \mathbb{E}[Y|Z=z] dG(z).$$

Ichimura and Taber (2000) develop direct estimators of PRTEs based on this idea.

Clearly, even if we focus on treatment-effects analysis that is policy-relevant in the sense discussed above, we need instrumental variables— to span a non-trivial policy space. We can, however, do without instrumental-variable estimators. This raises the question what instrument-variable methods are good for in the present non-parametric context. The answer has to be that a PRTE is an effect of Z on Y that is channelled through participation D, and that it is natural and informative to analyze this PRTEs in terms of $\{Y_d\}$ and $\{D_z\}$ (*i.e.* $p(z)$). The instrumental-variables approach to policy evaluation provides such an alternative cut of the data. Heckman and Vytlacil (2000b, 2001) show that both the PRTE and all (non-PRTE) treatment-effect parameters that are usually defined on $\{Y_d\}$ can be expressed in terms of the marginal treatment effect

$$\text{MTE}(U) := \mathbb{E}[Y_1 - Y_0|U]$$

at propensity U (see Björklund and Moffit, 1987).[10] They develop estimators of the PRTE based on local instrumental-variables estimators of MTEs.

Provided that Z has some structural meaning, which it presumably should have if its use has to successfully defended, the margins of participation on which the MTEs are defined are of structural interest. Then, the alternative cut of the data is of interest as well. Nevertheless, the main role of the instruments is now to span the set of policies that can be considered. An analyst that has access to a richer set of instruments can compute treatment effects at a finer partition of margins of participation. This superiorly informed analist can therefore compute the effects of a wider range of policies that affect participation through the instruments. But, if the policy of interest involves manipulation of only a subset of instruments that are available to both analysts, both can compute the relevant PRTEs— from their own MTEs if they like— and would come to exactly the same policy conclusions. Unlike the MTE, the PRTE is invariant to the choice of instruments. This choice only affects the range of policy interventions that can be considered.

[10] Imbens and Angrist's (1994) LATE can be expressed in MTEs provided certain differentiability conditions hold. With a truly binary instrument, for example, a LATE can be defined, but not an MTE.

Example 1. Suppose that $Z = (Q, R)$ with $Q \perp\!\!\!\perp R$ both scalar and $p(Z) = F(Q + R)$, for some distribution function F. Suppose that $(\{Y_d\}, \{D_z\}, Z)$ satisfies the Neyman-Rubin framework with instrumental variables $Z = (Q, R)$. Now suppose there is a second analyst that only observes R and not Q. This analyst could compute a propensity score $\tilde{p}(R) = \Pr(D = 1|R) = \mathbb{E}[F(Q + R)|R]$ and define $D_r = I(\tilde{p}(r) \geq \tilde{U})$ for some uniform random variable $\tilde{U} \perp\!\!\!\perp (\{Y_d\}, \{D_r\})$. The resulting model $(\{Y_d\}, \{D_r\}, R)$ satisfies the Neyman-Rubin framework with instrumental variables R and is consistent with the original model if we take $\tilde{U} = \tilde{p}(r) - F(Q + r) + U$. The MTEs that can be measured by the second, less informed analyst are $\mathbb{E}[Y_1 - Y_0|\tilde{U}] = \mathbb{E}[\text{MTE}(U)|\tilde{U}]$. Note that $\sigma(\tilde{U}) \subset \sigma(U)$ (strictly), so that $\mathbb{E}[Y_1 - Y_0|\tilde{U}]$ is an aggregate of the MTEs identifies by the first observer (here, $\sigma(U)$ is the σ-algebra generated by U, etcetera). Nevertheless, both analysts can compute PRTEs that involve a change in the distribution of R only, either from their reduced-form regressions or from their respective MTEs.

2.4 Stability and autonomy

Clearly, SUTVA in statistics is closely related to the concept of autonomy in structural-equations modelling. In economic applications, violations of SUTVA can therefore be expected for the same reasons that autonomy has been disputed. We illustrate this with an example, the Roy model.

Example 2. This example closely follows Heckman and Honoré (1989) and Heckman and Vytlacil (2000a). The basic Roy model can be written as $(\{Y_d\}, D)$, with $D = I(Y_1 \geq Y_0)$ and consistency as in Assumption 1. Clearly, $\{Y_d\}$ and D are dependent. The model is typically interpreted as a reduced form of $(\{Y_{dv}\}, \{D_v\}, V)$, with $D_v = I(Y_{1v} \geq Y_{0v})$ and $\{Y_{dv}\}$, $\{D_v\}$ and V independent (Heckman and Vytlacil, 2000a). For the model to be fully structural, we need autonomy of all three equations of the model (implying SUTVA on $\{Y_{dv}\}$ as before). This is not guaranteed in economic applications. Suppose that Y are earnings and D is sectoral choice, as in the original Roy model. Let V be skills. The sector choice D_v is based on the earnings in both sectors for a given set of skills v. Now, if the distribution of skills V changes, this may affect skill prices and therefore both $\{Y_{dv}\}$ and $\{D_v\}$. This is an example of the most typical violation of autonomy in economic applications of the treatment-effects approach, market equilibrium effects (Heckman, LaLonde and Smith, 1999). It should be noted though that autonomy may even fail to hold if skill prices are exogenous, but agents have rational expectations. This is a manifestation of Lucas's (1976) critique. It can be resolved by including skill prices in V. This, however, either greatly increases data requirements, or reduces applicability of the model— to experiments that do not change the aggregate skill distribution.

The example links violation of SUTVA, an assumption explicitly made in statistical treatment-effects analysis, to the well-known violation of autonomy due to rational-expectations effects (Lucas, 1976). This will be particularly relevant in the dynamic case.

3 Treatment effects in discrete time

3.1 Introduction

One problem that has mostly been ignored in the econometric treatment-effects literature and the related literature on non-parametric structural models is the fact that policy evaluation problems are usually dynamic. Economic programs are announced and implemented in real time. Economic agents act, in particular enroll in programs, dynamically. In some evaluation studies it may be possible to phrase the problem and organize the data such that it fits the static setup. More often, the proper economic interpretation of parameters and identifying assumptions is hard if a dynamic problem is framed as a static problem. Standard statistical approaches may fail to estimate or test anything useful. This is particularly true if outcomes are inherently dynamic variables like unemployment durations (Abbring and Van den Berg, 2003).

Biostatisticians face similar problems in the analysis of the causal effects of complex dynamic medical treatments on health outcomes. In response, they have developed methods based on dynamic extensions of the Neyman-Rubin potential-outcome model that underlies the static treatment methods (*e.g.* Robins, 1986, 1997, 1998a, 1998b; Gill and Robins, 2001; Lok, 2001). Econometricians have recently explored the possibility of using these models in dynamic economic policy evaluation (*e.g.* Lechner and Miquel, 2002). Because the more recent treatment literature is explicitly dynamic, it does not suffer from the fundamental problems associated with the application of static models to dynamic problems. Of course, a dynamic structural model with explicit and precise assumptions about behavior, information and market conditions would not suffer from such problems anyhow. It generates potential outcomes and program assignment rules as a by-product. However, the dynamic treatment-effect setup allows a more generic discussion of statistical methods and may lead to the development of robust statistical methods as for the static model (*e.g.* Lechner, 2003).

In a dynamic version of the Neyman-Rubin framework, the risk of under-representation of treatments— violation of SUTVA— is considerable. Not only should we account for all relevant aggregate conditions; we should also include all informational events related to the treatment under study in the treatment index. We will discuss this in more detail next.

3.2 Sequential randomization

In a series of papers, Robins has extended the static Neyman-Rubin model with selection on observables to a dynamic setting. Here, we briefly review this extension, following the exposition of Gill and Robins (2001). We slightly rephrase their setup to highlight some dynamic features that are of particular interest to economics. We point out that SUTVA, considered to be pivotal in the statistical literature, is particularly likely to be violated in econometric applications.

Consider an evaluation study in which measurements are taken and treatment decisions are made at $T+1 < \infty$ distinct times $0, 1, 2, \ldots, T$. Let $\mathcal{T} := \{0, 1, \ldots, T\}$. At each time $t \in \mathcal{T}$, "prognostic" factors $W(t)$ are measured and a treatment decision $D(t)$ is made. The prognostic factors $W(t) = (Y(t), X(t))$ consist of $\mathbb{R}^k$-valued intermediate outcomes of interest $Y(t)$ and other prognostic factors $X(t)$. The only difference between $Y(t)$ and $X(t)$ is that we are interested in causal inference on the effects of treatment on the former, but only include the latter to control for dynamic selection on observables. Some time after T, say at time $T+1$, some final $\mathbb{R}^k$-valued outcome $Y(T+1)$ is measured. Denote $\bar{W}(t) := (W(0), \ldots, W(t))$, $\bar{D}(t) := (D(0), \ldots, D(t))$, and $\bar{Y}(t) := (Y(0), \ldots, Y(t))$. Let $\bar{W} := \bar{W}(T)$, $\bar{D} := \bar{D}(T)$, and $\bar{Y} = \bar{Y}(T+1)$. For expositional convenience, let $\bar{W}$ and $\bar{D}$ be discrete.[11]

As discussed at the end of Subsection 2.2, we will contrast outcomes between policies that assign treatments contingent on (the history of) covariates. In the treatment-effects literature, such policies are called *treatment regimes* or *plans*. A treatment regime g specifies a treatment given available data $\bar{w}(t)$ at each time $t \in \mathcal{T}$. So, it is a collection of (non-random[12]) functions $(g_t; t \in \mathcal{T})$ such that $d(t) = g_t(\bar{w}(t))$ is the treatment assigned at time t if prognostic factors $\bar{w}(t)$ are observed. Define $\bar{g}_t(\bar{w}(t)) := (g_0(\bar{w}(0)), \ldots, g_t(\bar{w}(t)))$. If there is no risk of confusion, we abbreviate g_t and $\bar{g}_t$ as g. Denote the set of all treatment plans by $\mathcal{G}$. A *static* treatment regime is a plan $g \in \mathcal{G}$ such that $\bar{w}(t) \mapsto g(\bar{w}(t))$ is a trivial function of the covariates $(w(1), \ldots, w(t))$ for all $t \in \mathcal{T}$. Note that we do allow treatments specified by static plans to be contingent on the "initial conditions" $w(0)$. Denote the set of static treatment plans by $\mathcal{G}_0 \subset \mathcal{G}$.

Example 3. If $T = 0$, we are back in the static model. There are only static treatment plans, *i.e.* $\mathcal{G} = \mathcal{G}_0$, which are (possibly trivial) mappings $w(0) \mapsto g(w(0))$.

We restrict attention to treatment regimes that are *observable*.

[11] Gill and Robins (2001) show that the analyses can be straightforwardly extended to the case of continuous $\bar{W}$ and $\bar{D}$ under some auxiliary regularity conditions.

[12] It is straightforward to generalize the analysis to *random* plans (*e.g.* Gill and Robins, 2001). Such plans randomize treatment choices conditional on the treatment and covariate histories. See also Subsection 3.5.

Assumption 3. *Observability of treatment regimes.* For all treatment regimes $g \in \mathcal{G}$

$$\Pr\left(\bar{W}(t)=\bar{w}(t), \bar{D}(t-1)=\bar{d}(t-1)\right)>0 \Longrightarrow \Pr\left(\bar{W}(t)=\bar{w}(t), \bar{D}(t)=\bar{d}(t)\right)>0 \quad (1)$$

for all $\bar{d}(t)$ and $\bar{w}(t)$ such that $\bar{d}(t)=g(\bar{w}(t))$, $t \in \mathcal{T}$.

Here and in the sequel it is implicitly understood that events like $\bar{D}(-1) = \bar{d}(-1)$ should be ignored. So, for $t=0$ the first probability in equation (1) should be read as $\Pr\left(\bar{W}(0)=\bar{w}(0)\right)$.

We attach a vector of potential outcomes $\bar{Y}_g$ to each treatment plan g. Each $\bar{Y}_g$ is an $\mathbb{R}^{k(T+2)}$-valued random variable. The potential outcomes are again connected to the actual outcomes by a consistency condition,

Assumption 4. *Consistency.* $\bar{Y}=\bar{Y}_g$ on the event $\{\bar{D}=g(\bar{W})\}$.

This condition is usually strengthened, either implicitly or explicitly, by adding

Assumption 5. *No anticipation.* $\bar{Y}(t)=\bar{Y}_g(t)$ on the event $\{\bar{D}(t-1)=g(\bar{W}(t-1))\}$ for all $t \in \mathcal{T}$.

Assumption 4 is the direct equivalent of the consistency condition in the static Neyman-Rubin model. Assumption 5 is discussed extensively below. Its suggestive name is not taken from statistics, but follows Abbring and Van den Berg (2003).

Statistical inference typically relies on a sequential randomization assumption,

Assumption 6. *Sequential randomization.* For all $t \in \mathcal{T}$

$$D(t) \perp\!\!\!\perp (Y_g(t+1), \ldots, Y_g(T+1)) | (\bar{W}(t), \bar{D}(t-1)=g(\bar{X}(t-1))).$$

Assumption 6 is a sequential version of the conditional independence assumption that underlies *e.g.* statistical matching. It does allow for so called observed "dynamic confounders", variables that both are intermediate outcomes of past treatment and affect future treatment decisions. With Assumption 6, the dynamic potential-outcome model set up so far is a natural dynamic extension of the Neyman-Rubin model for a static (stratified) randomized experiment.

Under Assumptions 3–6, the *g-computation formula* can be used to compute the (marginal) distribution of the potential outcome Y_g from the joint distribution of the factual data $(\bar{X}, \bar{D}, \bar{Y})$.

Proposition 1. G-computation formula. *If Assumptions 3–6 hold, the distribution of $\bar{Y}_g$ follows from*

$$\begin{aligned}\Pr\left(\bar{Y}_g \in B\right) = & \sum_{w(0)} \cdots \sum_{w(T)} \Pr\left(\bar{Y} \in B | \bar{W}(T)=\bar{w}(T), \bar{D}(T)=g(\bar{w}(T))\right) \\ & \times \prod_{t=0}^{T} \Pr\left(W(t)=w(t) | \bar{W}(t-1)=\bar{w}(t-1), \bar{D}(t-1)=g(\bar{w}(t-1))\right)\end{aligned} \quad (2)$$

for any Borel set $B \subset \mathbb{R}^{k(T+2)}$.

For any (Assumption 3) observable treatment g, Proposition 1 ensures identification of the joint distribution of $\bar{Y}_g$ from the distribution of $(\bar{X}, \bar{D}, \bar{Y})$ under Assumptions 4, 5, and 6.

Assumptions 4 and 5 impose a natural recursive structure on the potential-outcomes model. In particular, they demand that outcomes $\bar{Y}_g(t)$ and $\bar{Y}_{g'}(t)$ corresponding to treatment plans g and g' coincide if the treatments actually assigned under the plans coincide up to time $t-1$, *i.e.* $\bar{g}_{t-1}(\bar{W}(t-1)) = \bar{g}'_{t-1}(\bar{W}(t-1))$. In particular, Assumption 5 requires that $Y_g(0) = Y_{g'}(0) = Y(0)$ for all treatment plans $g, g' \in \mathcal{G}$. So, we do not allow for causal effects of treatment on $Y(0)$. This is natural as $Y(0)$ is a pre-treatment variable.

In the g-computation formula (1) in Proposition 1, Assumptions 4 and 5 allow an attractive (semi-)causal interpretation of the factor

$$\Pr\left(W(t) = w(t) | \bar{W}(t-1) = \bar{w}(t-1), \bar{D}(t-1) = g(\bar{w}(t-1))\right)$$

as

$$\Pr\left(Y_g(t) = y(t), X(t) = x(t) | \bar{W}(t-1) = \bar{w}(t-1), \bar{D}(t-1) = g(\bar{w}(t-1))\right).$$

Note that Assumption 4 is not sufficient for this to be true, because, unlike Assumption 5, it does not guarantee that outcomes at time $t \in \mathcal{T}$ are not affected by future treatment. Also note that we could trivially generate a full causal interpretation by assuming the existence of counterfactuals $\bar{X}_g$ and imposing conditions like Assumptions 4 and 5 on $\bar{X}$ and $\bar{X}_g$ as well (or, equivalently, including all covariates in $\bar{Y}(T)$ and none in $\bar{X}$).

Assumption 5 is particularly natural if the outcome is survival of a patient. In this case, let $Y = Y(T+1)$ be a nonnegative continuous survival time. Let $Y(t) := I(Y > t)$ indicate survival up to measurement time t, $t \in \mathcal{T}$. Then, Assumption 5 imposes that treatment after death does not affect survival. The g-computation formula (2) reduces to

$$\begin{aligned}\Pr\left(Y_g > y\right) = \sum_{x(0)} \cdots \sum_{x(T_y)} &\Pr\left(Y > y | \bar{X}(T_y) = \bar{x}(T_y), \bar{D}(T_y) = g(\bar{w}(T_y)), Y > T_y\right) \\ &\times \prod_{t=0}^{T_y} \Pr\left(X(t) = x(t) | \bar{X}(t-1) = \bar{x}(t-1), \bar{D}(t-1) = g(\bar{w}(t-1)), Y > t\right) \\ &\times \Pr\left(Y > t | \bar{X}(t-1) = \bar{x}(t-1), \bar{D}(t-1) = g(\bar{w}(t-1)), Y > t-1\right)\end{aligned}$$

for all $y \in (0, \infty)$, where $T_y := \max\{t \in \mathcal{T} : t < y\}$ is the last measurement time before y. A simplification of equation (2) arises because (i) only the covariate and treatment history up to T_y matters for inference on the probability of the event $\{Y_g > y\}$ and (ii)

only covariate paths $\bar{w}(T_y) = (\bar{y}(T_y), \bar{x}(T_y))$ such that $\bar{y}(T_y) = 1$ produce nonzero terms in the g-computation formula.

Propositions 1 establishes identification under Assumptions 3–6. We also have

Proposition 2. *Given any distribution of the factual data $(\bar{X}, \bar{D}, \bar{Y})$, random variables $\bar{X}$, $\bar{D}$, $\bar{Y}$ and $\bar{Y}_g$, $g \in \mathcal{G}$, can be constructed that satisfy Assumptions 4–6.*

Proof. See Gill and Robins (2001, Section 6). Their analysis only involves causal inference on a final outcome (*i.e.* our $Y(T+1)$) and does not rest on Assumption 5. Their proof does however apply directly here. $\square$

Gill and Robins conclude that the model assumptions are "neutral", "for free", or "harmless". As we will argue later, from an econometric perspective some of the model assumptions, notably— as its name suggests— Assumption 5, can be interpreted as substantial behavioral/informational assumptions. In this sense, econometricians may prefer to phrase the neutrality result more negatively as a non-identification result (Abbring and Van den Berg, 2003).

3.2.1 Path analysis

The dynamic model of the previous section may seem disappointing to an econometrician used to dynamic econometric models. First, even though the extended model explicitly recognizes causal effects of treatment on intermediate outcomes— one aspect of dynamic confounding— it leaves the second aspect of dynamic confounding, the effect of intermediate outcomes on treatment choice, implicit. In biostatistics an asymmetric treatment–outcome setup may indeed be the most natural. In economics, however, we may be as interested in the effects of outcomes on treatment choices. Indeed, more often than not, the "treatment" versus "outcome" terminology does not do justice to the problem at hand and a more symmetric, "simultaneous-equations" approach is more appropriate. Second, even though we have pursued a dynamic outcomes model, we have ended up with a framework that is essentially a trivial extension of the static potential-outcomes model. After all, we have not specified a dynamic model of the outcomes per se. Rather, we have focused on the joint determination of the outcome path by the treatment path. Thus, this model does not allow for the type of dynamic causal path analysis that economic problems often call for.

Example 4. Consider the evaluation of a system of government labor market programs that dynamically provides agents with access to various training programs, job search assistance schemes, etcetera. The model set up so far suggests defining sequences of labor market outcomes (wages, labor market status, earnings) and (treatment) indicators of program participation over the course of the agents' labor market careers. An underlying causal structure follows by defining appropriate program plans and corresponding potential outcomes. Under the standard assumptions, such a framework would allow for inference on the outcomes that would have occurred under any one specific participation plan.

Now consider the problem faced by a policy maker who only controls part of the participation plan. For example, the policy maker could be in charge of a youth general training program for school-leavers that do not find jobs quickly. The present framework would allow the policy maker to contrast potential outcomes between *hypothetical* participation and non-participation in the youth training program evaluated at the *actual* participation status in the remainder of their careers (see Lechner and Miquel, 2002, for an example of this approach). However, this contrast would not reflect the *overall* effect of the youth training program on labor market outcomes, because it disables any effects of participation in the program on future participation in programs that are not under the control of the policy maker. Thus, the measured effect is a *direct* effect only. Alternatively, the policy maker could index potential outcomes by the participation in the program under his control only.[13] Such a model would allow for inference on the overall effect of the youth program, but not on the causal pathways leading there.

Typically, policy makers will be interested in such pathways. For example, the youth training program may benefit the agent through direct employability effects or may rather facilitate later participation in programs that have such effects. A full dynamic causal analysis therefore requires a complete causal model of the dynamic determination of outcomes and program participation.

Structural econometrics often delivers such models, but the basic asymmetric treatment–response setup does not. However, as noted before, potential-outcome models can be seen as non-parametric structural models (in this context, see Robins, 1997, and Pearl, 2000). We could simply enrich the asymmetric setup to the level of a standard structural model by modelling the causal determination of treatment. Rather than distinguishing treatments and responses, we can model all causal relations within the collection of variables $\{D(0),\ldots,D(T);Y(0),\ldots,Y(T+1)\}$. The natural extension of the basic potential-outcomes setup is a model in which (*i*) all causal relations are backward-looking, with $D(t)$ temporarily ordered after $Y(t)$ in each period $t \in \mathcal{T}$, and (*ii*) the structural errors are mutually independent. Such a fully recursive system is trivially identified and imposes no structure on the factual data.

[13]Obviously, such potential outcomes would not be stable under changes in the process of later program enrollment. This is not a problem, as long as the evaluation results are used in an, in this dimension, unchanged environment.

The use of recursive systems, usually represented by directed acyclic graphs (DAGs), is standard in statistical causal modelling (*e.g.* Pearl, 2000). The result that the factual data can be represented by a recursive causal system with independent errors is, because of the lack of structure, trivial. It could be seen as a non-parametric variant on Wold's more substantial result for the linear normal model in economics. In this context, economists have worried about simultaneity and the autonomy (and therefore causal interpretation) of the equations in a (non-unique) recursive representation.[14] Statisticians seem to worry less about such issues, presumably because they are non-statistical problems.

3.3 The information structure of economic programs

In economic program evaluation, $\bar{Y}$ typically consists of wage, employment or earnings outcomes (see *e.g.* Heckman, LaLonde and Smith, 1999, for a review). The programs under evaluation can be anything from training and job search assistance programs to unemployment benefits programs.

The problem of program heterogeneity is well-understood in the context of the static framework. If multiple versions of programs are offered, it is important to represent all versions in the potential-outcomes model. If versions are aggregated into broad categories, SUTVA is violated because potential outcomes depend on the mechanism used to assign treatment versions (Rubin, 1986). Program heterogeneity will plague dynamic evaluation studies in the same way. We will now argue that in dynamic econometric program evaluation, we should in particular worry about the incomplete specification of the informational structure of programs under study.

A naive specification of treatment plans would be sets of static or dynamic rules for participation in training or job search assistance and varying the benefits level, respectively. Static rules simply stipulate times at which agents (are offered to) participate in certain programs or benefits are changed. Dynamic rules would make these events contingent on the covariate and outcome history. Either way, treatment plans would be defined in terms of actual participation in programs. Such a naive specification is incomplete in the context of economies inhabited by rational forward-looking agents.

Assumption 5 is usually read as requiring that each cause should precede its effect. Outcomes $\bar{Y}(t)$ up to time t are not causally affected by treatment choices after t. In the econometric setup sketched above, this seems to exclude anticipatory effects of future participation in training or job search assistance and future changes in benefits. Such effects are routinely predicted by economic-theoretic models with forward-looking agents.

However, this is a false impression due to an incomplete account of causes and, as a

[14] See *e.g.* the discussion of work by Marshak, Havelmoo and Wold on simultaneous equations in Hendry and Morgan (1995).

consequence, an incomplete characterization of treatments. Anticipatory effects of future events are not caused by those events, but are triggered by signals containing information about these events. Once we include all causes, both signals and actual participation in programs, in the model, the recursive structure of Assumption 5 is natural. In terms of Rubin (1986), we have different versions of the treatments corresponding to different signals. We have to ensure that all versions of the treatments are represented to guarantee stability.

Example 5. Let $T = 1$. Suppose that the outcome $Y(t)$ in period t is (discretized) earnings, $t = 0, 1, 2$, and that there are no covariates $\bar{W}$. At time 1, agents are either offered a slot in a training program at a school ($d(1) = 1$) or they are not ($d(1) = 0$). All agents that are offered a slot actually enroll in the program and there are no substitute programs available to agents that are not offered a slot at the school under study.

The training program is not fully characterized by specifying the assignment of slots at time 1. At time 0 the school sends letters to the agents. The letters state whether the agents will be offered a slot in the program at time 1 or not. This can be modelled as a binary signal, *i.e.* $d(0) = 1$ if the letter claims that a slot will be available and $d(0) = 0$ otherwise.

If we would have naively indexed treatments by $d(1)$ only, we would have multiple versions of each treatment. Within classes of treatments defined by $d(1) = 0$ and $d(1) = 1$, respectively, we can distinguish treatments such that $d(0) = 0$ and treatments such that $d(0) = 1$. If the signals are informative (see below), this would lead to a violation of SUTVA.

We are interested in causal inference on the effect of the training program, consisting of the letter at time 0 and the training slot at time 1, on pre-program wages $Y(0)$ and $Y(1)$ and post-program wages $Y(2)$. Without covariates $\bar{X}$, the relevant training (treatment) plans $g = (g_0, g_1)$ specify a signal $d(0) = g_0(y(0))$ for each wage $y(0) \in \text{supp}(Y(0))$ and a training offer $d(1) = g_1(\bar{y}(1))$ for each pair of wages $\bar{y}(1) \in \text{supp}(\bar{Y}(1))$. As we have included all signals in the model, it is natural to impose Assumption 5.

Black *et al.* (1999) illustrate the relevance of this example. They analyze the effect of compulsory training and employment services provided to unemployment insurance (UI) claimants in Kentucky on the exit rate from UI and earnings. In the program under study, letters are sent out to notify agents some time ahead whether they are selected to participate in the program. This information is recorded in a database and is available to Black *et al.* The main empirical finding of the paper is that the threat of future mandatory training conveyed by the letters is more effective in increasing the UI exit rate than training itself.

The data used by Black *et al.* carefully record the signals provided to agents. In general, if full records are kept of all interactions between administrators of programs and the agents to which the programs are provided and if we are able to properly translate these records in program sequences that include all relevant signals the standard framework with Assumption 4 can be applied.

In many econometric applications, the information on the program under study is less rich. Data sets may provide information on actual participation in training programs and some background information on how the program is administered in general. Typically, however, data do not keep track of all letters sent to agents nor of each phone conversation between administrators and agents. In this case, we can make various assumptions on how agents collect information on the program. Possibly, allocations of slots in the program are announced a fixed period of time ahead of actual enrollment and agents respond to the announcement. Alternatively, no advance notice is given at all.

We can only guarantee SUTVA if we make explicit assumptions on the informational structure of the program. Such assumptions are unavoidable by Proposition 2, which implies that we can impose the recursive structure of Assumption 5 without restricting the factual data. This is equivalent to assuming that no advance notice is given. In many application, general institutional information can be used to justify specific informational assumptions.

Example 6. Abbring, Van den Berg and Van Ours (1997) analyze the effect of punitive benefits reductions, or sanctions, in Dutch UI on re-employment rates. In the Netherlands, UI claimants have to comply with certain rules concerning search behavior and registration. If a claimant violates these rules, a sanction may be applied. A sanction is a punitive reduction in benefits for some period of time and may be accompanied by increased levels of monitoring by the UI agency. See Grubb (1999) for a review of sanction systems in the OECD. The data used in Abbring *et al.* are administrative and provide the re-employment duration, the duration at which a sanction is imposed if a sanction is imposed and some background characteristics for each UI case.

Without any prior knowledge of the Dutch UI system, one can make a variety of informational assumptions. In one extreme, UI claimants know at the start of their UI spells that benefits will be reduced at some specific duration if they are still claiming UI at that duration. This results in a UI system with entitlement periods that are tailored to individual claimants and that are set and revealed at the start of the UI spells. In this case, claimants will change their labor market behavior from the start of their UI spell in response to the future benefits reduction (*e.g.* Mortensen, 1977). In another extreme, claimants receive no prior signals of impending sanctions and there are no anticipatory effects of actual benefits reductions. However, agents may still be aware of the properties of the sanctions process and to some extent controlling this process. Abbring *et al.*

analyze a search model with these features. The data cannot distinguish between both informational assumptions (see Subsection 4.2 for details). However, Abbring *et al.* use institutional background information to argue in favor of the latter assumption.

3.4 Equilibrium effects

The discussion in the previous section is partial in the sense that it does not specify how signals are interpreted by agents. The same signal may be interpreted differently in different environments. In rational expectations equilibrium, the informational content attached to signals depends on the actual relation of signals to future treatment options in the population. This implies that potential outcomes are not invariant to the choice of the assignment mechanism. If we would consider a parametric class of assignment mechanisms, the distributions of the potential outcomes would depend on the parameters of the assignment process. In the terminology of Engle, Hendry and Richard (1983), the assignment mechanism is not exogenous to the potential outcomes. Without further qualifications we have a violation of Rubin's (1986) SUTVA. Macroeconomists recognize the Lucas (1976) critique.

This is akin to the problem of equilibrium effects of large-scale programs on prices that has been discussed by Heckman *et al.* (1999) within the context of the static binary-treatment potential-outcome model. They differentiate between the no-treatment outcome in a world with treatments and outcomes in a world without treatments. These are not the same if the program under study is large and affects market outcomes. We have discussed the example of the Roy model in Section 2.

Such effects may occur for large-scale dynamic programs as well. The informational equilibrium effects may also occur for well-established small-scale programs. In this sense, equilibrium effects are more likely to plague dynamic econometric evaluation studies than static studies.

Example 2. (continued). Suppose that the signals carried by the letters are not contingent on initial earnings $Y(0)$ and that the assignment of slots by the school is not contingent on the earnings history $\bar{Y}(1)$. At time 0, the school randomly allocates signals such that $\Pr(D(0) = 0) = \frac{1}{2}$. We consider two cases for the training slot allocation at time 1.

(*i*) the school randomly allocates training slots ($D(1) \perp\!\!\!\perp D(0)$) such that $\Pr(D(1) = 0) = \frac{1}{2}$;

(*ii*) the school allocates training slots to those agents who have received a letter claiming that they will be offered a slot in the training program, *i.e.* $D(1) = D(0)$.

First note that in Case (i) all training plans g are observable, *i.e.* satisfy Assumption 3. In Case (ii), however, only plans such that $g_0(y(0)) = g_1(\bar{y}(1))$ for some $\bar{y}(1) \in \text{supp}(\bar{Y}(1))$ are observable. In particular, the only static training plans that are observable are $(0,0)$ and $(1,1)$. In either case, the distributions of the potential outcomes $\bar{Y}_g(2)$ corresponding to the observable training plans $g \in \mathcal{G}$ can be computed using the g-computation formula.

We should expect different outcomes in training programs (i) and (ii), even though the programs have the same structure and treatments are indexed in the same way. In rational equilibrium, the agent's expectations based on a signal $d(0)$ should be consistent with the conditional conditional distribution of training offers $D(1)|(D(0) = d(0))$ in the population. So, in Case (i) all agents believe at time 0 that they will be offered a slot in a program at time 1 with probability $\frac{1}{2}$. In this case, the signal is not informative and the agents will not respond to it. In Case (ii), however, all agents will know for sure at time 1 whether they will be offered a slot in a program at time 2. Agents will respond to the message in the letter from the school.

Example 2 shows that we should generally expect equilibrium effects to affect outcomes in dynamic econometric evaluation studies. After all, agents can only value the signal received at time 1 by rationally considering the relation between the signal and actual assignment of training slots in the population. The potential outcomes thus depend on the parameters of the assignment process. We further investigate this issue in a more worked out example along the same lines.

Example 3. Again, let $T = 1$. Suppose that $Y(t)$, $t = 0, 1, 2$, are binary employment outcomes. More precisely, $Y(t) = 0$ if an agent is unemployed at time t and $Y(t) = 1$ if the agent is employed. For now, suppose that all agents are unemployed at time 0, so that $Y(0) = 0$. Also, ignore covariates $\bar{X}$ for the time being.

The agent operates in a simple job search environment. At the end of period 0, agents receive a single job offer characterized by a wage W_1 drawn from a continuous distribution F such that $F(0) = 0$. The agent can either accept the job or reject the job. If the agent accepts the job offer W_1, he is employed at the wage W_1 at both time 1 and time 2 ($Y(1) = Y(2) = 1$). If the agent rejects the job, he is unemployed at time 1 ($Y(1) = 0$).

An agent that is unemployed at time 1 receives another wage offer W_2 at the start of period 2 that is drawn independently of W_1 from the same distribution F. At time 2, the time 1 job offer W_1 cannot be recalled and the agent only has to decide whether to accept or reject the new wage offer W_2. If the agent accepts W_2, he is employed at time 2 ($Y(2) = 1$) and receives compensation W_2. Otherwise the agent is unemployed at time 2 ($Y(2) = 0$).

We are interested in evaluating the effect of a unemployment compensation program on employment outcomes (suppose that wages are not observed). The program is organized as follows. All agents that are unemployed at time 1 receive unemployment benefits $b > 0$. At time 2, some agents loose entitlement to their benefits ($D(1) = 0$), but others are still entitled ($D(1) = 1$). Agents only actually receive benefits if they are unemployed.

At time 0, agents receive a phone call from the unemployment benefits agency in which they are told whether they will ($D(0) = 1$) or will not ($D(0) = 0$) receive benefits if they are unemployed at time 2. As in Example 2, we cannot analyze the effect of this phone call without some assumption on the information that is revealed. So, assume that agents have rational expectations, $(D(0), D(1)) \perp\!\!\!\perp (W_1, W_2)$, and that

$$\begin{aligned} &\Pr(D(0) = 0) = p, \\ &\Pr(D(1) = 0|D(0) = 0] = q_0 \quad \text{and} \\ &\Pr(D(1) = 0|D(0) = 1] = q_1 \end{aligned} \tag{3}$$

for some parameters $0 \leq p, q_0, q_1 \leq 1$.

Agents choose to accept or reject W_1 and, if unemployed at time 1, W_2 so that total income over times 1 and 2 is maximized. We ignore any income at time 0 because it cannot be affected by behavior in our model. The agent's optimization problem is straightforward to solve by backward induction.

If the agent is unemployed and entitled to benefits b at time 2, he will only accept a job offer $W_2 \geq b$. If the agent is unemployed and not entitled to benefits at time 2, he will accept any job offer $W_2 \geq 0$. Therefore, the continuation value in unemployment at time 1 given that the agent is entitled to benefits at time 2 is

$$\begin{aligned} V_1 &= b + bF(b) + (1 - F(b))\mathbb{E}[W_2|W_2 > b] \\ &= 2b + \int_b^\infty (w - b) dF(w) \end{aligned}$$

and the continuation value in unemployment at time 1 given that the agent is not entitled to benefits at time 2

$$V_0 = b + \mathbb{E}[W_2] < V_1.$$

Now suppose that the agent believes at time 1 that he will be entitled to benefits in period 2 (if unemployed) with probability $1 - q$. With rational expectations, $q = q_0$ if $d(0) = 0$ and $q = q_1$ if $d(0) = 1$. Let $\bar{V}(q) := qV_0 + (1 - q)V_1$. Then, the agent accepts any time 1 job offer W_1 that exceeds the perceived per period continuation value $\bar{V}(q)/2$ in unemployment.

We can distinguish 4 static treatments, $(0, 0)$, $(1, 0)$, $(0, 1)$ and $(1, 1)$. All randomness in the corresponding potential outcomes originates in the offered wages W_1 and W_2. The distributions of the potential outcomes are easy to derive because of the assumed independence of W_1 and W_2.

First, recall that we have assumed that $Y_g(0)$ is degenerate at 0 for all g. Next, at time 0 agents that have received a signal $d(0)$ have reservation wage $\bar{V}(q_{d(0)})/2$. So, we have that

$$\Pr(Y_{(d(0),d(1))}(1) = 0) = F\left(\frac{\bar{V}(q_{d(0)})}{2}\right)$$

for $d(0), d(1) = 0, 1$.

Finally, agents that are employed at time 1 will be employed at time 2 for sure, *i.e.*

$$\Pr(\bar{Y}_g(2) = 1|\bar{Y}_g(1) = 1) = 1$$

for all treatments g. Agents that are unemployed at time 1 and have $d(1) = 0$ lose their benefits and will be employed for sure at time 2. Unemployed agents with $d(1) = 1$ will accept a job at time 2 with probability $1 - F(b)$. So, we have that

$$\Pr(Y_{(d,0)}(2) = 0|Y_{(d,0)}(1) = 0) = 1 \quad \text{and}$$

$$\Pr(Y_{(d,1)}(2) = 0|Y_{(d,1)}(1) = 0) = F(b)$$

for $d = 0, 1$.

The distributions of the potential outcomes depend on the parameters of the assignment process. In particular, $V'(q) = V_0 - V_1 < 0$. So, $\Pr(Y_{(0,d)}(1) = 0)$ and $\Pr(Y_{(1,d)}(1) = 0)$ are decreasing in respectively q_0 and q_1. This implies that SUTVA is violated. Causal inference on the effect of the UI program without explicit reference to the assignment mechanism is impossible.

Depending on the evaluation problem that is to be solved, we may be willing to select a class of assignment mechanisms within which the potential outcomes are invariant. We should then have data on a population for which benefit plans are assigned according to a mechanism in this class. The causal analysis has to be qualified as only providing causal contrasts between UI plans assigned according to mechanisms in this class.

Note that outcomes only depend on q_0 and q_1 and not on p. Two examples stand out. First, suppose that $q_0 = q_1 =: q$. Then, $\Pr(D(1) = 0) = q$ and the signal at time 0 is non-informative about the benefits reduction. In this case, the outcomes are only invariant if we fix $\Pr(D(1) = 0)$. Second, let $q_0 = 1$ and $q_1 = 0$. Then, $D(1) = D(0)$ almost surely and outcomes are invariant to changes of $\Pr(D(1) = 0) = p$ over $[0, 1]$. Agents have perfect foresight about benefits entitlement. Given the realized entitlement, agents are not interested in the assignment mechanism.

In either case, we have to fix (q_0, q_1) to ensure consistency. If we are interested in evaluating outcomes for different values of (q_0, q_1), we have to index the potential outcomes not only by $\bar{d}(1)$ but also by (q_0, q_1). This would ensure that all versions of the treatment are represented and consistency holds. For this extended model to be operational, we need variation of (q_0, q_1) in the data. Also, we have to consider a mechanism that assigns (q_0, q_1) in a first stage before the assignment of $\bar{d}(1)$. We should check whether the new potential outcomes, indexed by both $\bar{d}(2)$ and (q_0, q_1) are invariant to the choice of this new assignment mechanism. Depending on the specific informational assumptions it is thinkable that, for example, the initial conditions $Y(0)$ are affected by the assignment mechanism.

In general, we can always fix a class of assignment mechanisms within which potential outcomes are invariant and contrast potential outcomes within this class. The actual assignment mechanism should be in the class. Under perfect foresight we can allow the assignment mechanism to vary arbitrarily. If we have sufficiently rich data, we can index potential outcomes by the (parameters of the) assignment mechanism and contrast outcomes between assignment mechanisms and assigned plans. This requires a specification of a "meta-mechanism" for the selection of assignment mechanisms.

3.5 Optimal policy estimation

In an interesting recent development, Murphy (2002) has developed methods to choose an optimal treatment plan based on an empirical analysis of Robins' dynamic potential-outcome model (see also Robins, 2002). This is of direct interest to economic policy makers, provided that they accept Robins' framework as such.[15] The key problem in directly applying Murphy's methods to dynamic economic decision problems lies, of course, in the latter premise.

Our discussion implies that consistency Assumptions 4 and 5 are likely to fail in economic applications. These assumptions require that different dynamic policies yield the same outcome at some point in time if they happen to deliver the same treatments up to that time. This excludes anticipatory effects. Furthermore, policies that randomize treatments— we have not considered these so far (see *e.g.* Gill and Robins, 2001)— should yield the same outcomes as non-random static policies if they happen to realize the same treatment paths. Thus, the choice of the optimal dynamic policy is bound to ignore the potential expectations effects of different dynamic assignment rules. The dynamics of the plans only serve to dynamically allocate the treatments to those who benefit most. In doing so, the planner is forced to presume that outcomes are invariant across different policies that assign the same treatment paths. In this sense, the analysis is essentially static and not very different from a similar analysis with the static model

[15] From the perspective of this paper, it is interesting to note that such dynamic programming problems certainly had the interest of H. Theil (*e.g.* Theil, 1957).

of stratified randomized experiments at the end of Subsection 2.2. This seems perfectly reasonable in the medical applications for which these methods are developed. In an economic environment, however, we should expect that agents are aware of the actual assignment rule and respond to that.

3.6 Selection on unobservables

In econometric program evaluation, randomization assumptions like Assumption 6 are not very likely to be satisfied. Observational economic data suffer from a lot of heterogeneity between agents (*e.g.* Heckman, LaLonde and Smith, 1999). Some of this heterogeneity is bound to be unmeasured. In a dynamic context, such unmeasured heterogeneity leads to violations of randomization assumptions. This is true even if the unmeasured variables only affect the availability of slots in programs but not outcomes directly. If agents are rational, forward-looking and, unlike the econometrician, observe the unmeasured variables, they will typically respond to these variables because these affect future opportunities to participate in programs (Abbring and Van den Berg, 2003 and 2004). Thus, there may be indirect effects on outcomes even if there are no direct effects. In any case, the randomization condition fails.

For the same reason, identification based on instrumental variables is relatively hard in dynamic models. If the candidate instruments only vary cross-sectionally, the argument above implies that they are not likely to be valid instruments. Rather than instrumental variables, we need *instrumental processes* (Robins, 1997) that yield some random variation in treatment assignment at each point in time. In the context of continuously assigned treatments, the implied data requirements seem onerous (Abbring and Van den Berg, 2003).

Possibly the most fruitful econometric approach to selection on unobservables is the construction of explicit models of dynamic selection and outcomes. Carneiro, Heckman and Hansen (2003) have recently developed such a model based on a dynamic factor structure. Similar ideas underly the event-history approach to program evaluation, which we will discuss next.

4 The event-history approach to policy evaluation

4.1 Introduction

Researchers facing the problem of analyzing the effect of dynamic programs on dynamic outcomes have often resorted to event-history methods. Examples in economics include Ridder (1986), Card and Sullivan (1988), Gritz (1993), Lillard (1993), Ham and Lalonde

(1996), Lillard and Panis (1996), Eberwein, Ham and Lalonde (1997), Bonnal, Fougère and Sérandon (1997), Abbring, Van den Berg and Van Ours (1997), and Van den Berg, Van der Klaauw and Van Ours (2004). In fields like epidemiology, the use of event-history models to analyze treatment effects is widespread (see *e.g.* Andersen *et al.*, 1993, and Keiding, 1999).

The event-history approach to program evaluation is firmly rooted in the econometric literature on state-dependence and heterogeneity (Heckman and Borjas, 1980, and Heckman, 1981). Event-history models along the lines of Heckman and Singer (1984) are used to jointly model transitions into programs and transitions into outcome states. Causal effects of programs are modelled as the dependence of individual transition rates on the individual history of program participation. Dynamic selection effects are modelled by allowing for dependent unobserved heterogeneity in both the program and outcome transition rates. A typical, simple example is a mixed semi-Markov model in which the causal effects are restricted to program participation in the previous spell (*e.g.* Bonnal, Fougère and Sérandon, 1997).

The mainstream treatment-effects literature has recently stressed semi-parametric and non-parametric methods of inference. Applied papers employing the event-history approach, on the other hand, usually estimate highly parameterized models. There is a substantial literature on the identifiability of state-dependence effects and heterogeneity in duration and event-history models (see Heckman and Taber, 1994, and Van den Berg, 2001, for reviews). This literature can be exploited and extended to reconcile both approaches.[16] Here, we provide discussion for some canonical cases.

In Subsection 4.2, we discuss the simplest case of mutual dependence of events in continuous time, involving only two binary events. This case is sufficiently rich to capture the effect of a dynamically assigned binary treatment on a duration outcome. Binary events in continuous time can be fully characterized by the time at which they occur and a structural model for their joint determination is a simultaneous-equations model for durations. We will develop such a model along the lines of Abbring and Van den Berg (2003). Rephrasing their model explicitly as a simultaneous-equations model does not only provide a link with Theil's work, but also highlights the fundamental problems that arise if we try to infer the causal effects of a dynamically assigned treatment on a duration outcome using techniques developed for static models. In Subsection 4.3, we conclude with a short discussion of the event-history approach to program evaluation from a more general (multiple-treatment, multiple-outcome) perspective.

[16] Abbring and Van den Berg (2004) discuss the relation between the event-history approach to program evaluation and more standard latent-variable and panel-data methods, with a focus on identification issues.

4.2 A simultaneous-equations model for durations

Consider two random durations Y and D that assume values in $\overline{\mathbb{R}}_+ := \mathbb{R}_+ \cup \{\infty\}$, where $\mathbb{R}_+ := [0, \infty)$. We refer to one of the durations, D, as the treatment (time) and to the other duration, Y, as the outcome. Such an asymmetry arises naturally in many applications. For example, in Abbring, Van den Berg and Van Ours (1997)'s study of unemployment insurance, the treatment is a punitive benefits reduction (sanction) and the outcome re-employment. The re-employment process continues after imposition of a sanction, but the sanctions process is terminated by re-employment. The current exposition, however, is symmetric and unifies both cases. The point ∞ represents the event that the outcome or treatment never occur.

Define collections of nonnegative potential treatments $\{D_y; y \in \overline{\mathbb{R}}_+\}$ and potential outcomes $\{Y_s; s \in \overline{\mathbb{R}}_+\}$. Here, D_y is the random treatment that would prevail if the outcome is externally set to y. Similarly, Y_d is the outcome resulting from setting the treatment time to d. Suppose that $\{D_y; y \in \overline{\mathbb{R}}_+\}$ and $\{Y_d; d \in \overline{\mathbb{R}}_+\}$ are measurable processes. Assume that D_y and Y_d are continuously distributed and denote the corresponding integrated hazard rates by $\Theta_{D_y} : \overline{\mathbb{R}}_+ \mapsto \overline{\mathbb{R}}_+$ and $\Theta_{Y_s} : \overline{\mathbb{R}}_+ \mapsto \overline{\mathbb{R}}_+$, respectively. These integrated hazards fully determine the marginal distributions of D_y and Y_d. For example, by the well known exponential formula for the survival function, $\Pr(D_y > t) = \exp(-\Theta_{D_y}(t))$. For expositional convenience, we assume that both Θ_{D_y} and Θ_{Y_s} are strictly increasing on $\mathbb{R}_+$. We also assume that the duration distributions are non-defective by requiring that $\Theta_{D_y}(\infty) = \Theta_{Y_s}(\infty) = \infty$.[17]

Differences in the integrated hazards between potential outcomes or between potential treatments can be given a causal interpretation. To see this, note that, under the assumptions above, $\Theta_{D_y}(D_y)$ and $\Theta_{Y_d}(Y_d)$ are unit exponential for all $y, d \in \mathbb{R}_+$. So, we can write

$$D_y = \Theta_{D_y}^{-1}(E_{D_y}) \text{ and } Y_d = \Theta_{Y_d}^{-1}(E_{Y_d}),$$

with E_{D_y} and E_{Y_d} appropriate unit exponential random variables for all $y, d \in \mathbb{R}_+$. The distributions of E_{D_y} and E_{Y_d} are invariant to interventions. Thus, the integrated hazards link the potential treatments and outcomes to errors that are invariant *in distribution* to the interventions. Therefore, they have causal interpretations (see Freedman, 2002).

The model is again closed by requiring consistency of actual and potential outcomes and treatments,

Assumption 7. *Consistency.* $Y_D = Y$ and $D_Y = D$.

[17] Abbring and Van den Berg do allow for defects, which often have structural interpretations. See Abbring (2002) for some discussion and results.

We should be careful here, as our model may not be coherent: it may not have a solution (Y, D).[18] If it does, it is unique by consistency. Consider the following assumptions.

Assumption 8. *No anticipation.* For all $t \in \mathbb{R}_+$,

$$\Theta_{Y_t} = \Theta_{Y_\infty} \text{ and } \Theta_{D_t} = \Theta_{D_\infty} \text{ on } [0, t].$$

Assumption 9. *Strong invariance.* For all $t \in \overline{\mathbb{R}}_+$, $E_{Y_t} = E_Y$ and $E_{D_t} = E_D$ for some unit-exponential random variables E_Y and E_D.

Assumption 8 implies that the hazard rates for any two potential outcomes Y_d and $Y_{d'}$ coincide almost everywhere until the smallest of the hypothetical treatments times d and d'. We say that there are no effects of anticipation of the treatment on the outcome. Similarly, Assumption 8 excludes anticipation effects of future outcomes on the treatment hazard. Together, Assumptions 8–9 ensure

Lemma 1 (Coherency). *Under Assumptions 7–9, there is a unique (Y, D) such that $Y = Y_D$ and $D = D_Y$.*

Proof. Draw (E_Y, E_D) and compute (Y_∞, D_∞). If $Y_\infty \leq D_\infty$, then $Y = Y_\infty$ and $D = D_Y \geq Y$. Otherwise, $Y_\infty > D_\infty$, $D = D_\infty$ and $Y = Y_D > D$. □

To gain some more insight in the role of Assumption 9, we need some additional notation. Let $N_{Y_d}(t) := I_{[0,t]}(Y_d)$ be a binary random variable that indicates whether the outcome event would have occurred at time t or before if a treatment was assigned at time d. Then, $\{N_{Y_d}\}$ is a process that counts the number of such potential-outcome events— at most one— up to and including each point in time. Similarly, define $\{N_{D_y}\}$, $\{N_Y\}$ and $\{N_D\}$. Assumption 9 essentially ensures that a strong version of Assumption 7, like Assumption 5 in the discrete-time model, holds:

Lemma 2 (Strong no-anticipation). *Assumptions 8 and 9 imply that for all $t \in \mathbb{R}_+$, $N_{Y_t}(\tau) = N_{Y_\infty}(\tau)$ and $N_{D_t}(\tau) = N_{D_\infty}(\tau)$ for all $\tau \in [0, t]$.*

Under Assumptions 7–9, our model is a fully recursive dynamic structural model (possibly with dependent errors) for the processes $\{N_Y, N_D\}$ even though it is not recursive in terms of Y and D. Therefore, coherency problems do not arise.

We illustrate these ideas with two examples. We need

Assumption 10. *Randomization.* $\{D_y; y \in \overline{\mathbb{R}}_+\} \perp\!\!\!\perp \{Y_s; s \in \overline{\mathbb{R}}_+\}$.

[18] Abbring and Van den Berg (2003) do not explicitly model the (lack of) causal determination of D by Y. Therefore, their structural model of Y and D is triangular and they do not have to worry about coherency.

Example 4. Consider a standard partial search model describing the job search behavior of an unemployed individual (e.g. Mortensen, 1986). Job offers arrive at a rate $\lambda > 0$ and are random draws from a given distribution F. An offer is either accepted or rejected. A rejected offer cannot be recalled at a later time. The individual initially receives a constant flow of unemployment-insurance benefits. However, the individual faces the risk of a sanction— a permanent reduction of his benefits to some lower, constant level— at some point during his unemployment spell. During the unemployment spell, sanction arrive independently of the job-offer process at a constant rate $\mu > 0$. The individual cannot foresee the exact time a sanction is imposed, but he does know the distribution of these times.[19] The individual chooses a job-acceptance rule as to maximize his expected discounted lifetime income. Under standard conditions this is a reservation-wage rule: at time t, the individual accepts each wage of $\underline{w}(t)$ or higher. The corresponding re-employment hazard rate is $\lambda(1 - F(\underline{w}(t))$. Apart from the sanction, which is not foreseen and arrives at a constant rate during the unemployment spell, the model is stationary. This implies that the reservation wage is constant, say equal to $\underline{w}_0$, up to and including time d, jumps to some lower level $\underline{w}_1 < \underline{w}_0$ at time d and stays constant at $\underline{w}_1$ for the remainder of the unemployment spell if benefits would be reduced at time d.

The model fits our simultaneous-equations model for durations in the following way. Let Y be the re-employment duration and D the sanction time. The potential-outcome hazards are

$$\theta_{Y_d}(t) = \begin{cases} \lambda_0 & \text{if } 0 \leq t \leq d \\ \lambda_1 & \text{if } t > d \end{cases}$$

and the corresponding integrated hazards are

$$\Theta_{Y_d}(t) = \begin{cases} \lambda_0 t & \text{if } 0 \leq t \leq d \\ \lambda_0 d + \lambda_1 (t-d) & \text{if } t > d, \end{cases}$$

where $\lambda_0 := \lambda\,[1 - F(\underline{w}_0)]$ and $\lambda_1 := \lambda\,[1 - F(\underline{w}_1)]$. Similarly, the potential-treatment hazards are

$$\theta_{D_y}(t) = \begin{cases} \mu & \text{if } 0 \leq t \leq y \\ 0 & \text{if } t > y \end{cases}$$

and the corresponding integrated hazards are $\Theta_{D_y}(t) = \mu \min\{t, y\}$. For definiteness, we have set the sanction hazard to 0 after re-employment.

[19]This is a rudimentary version of the partial search model with punitive benefits reductions, or sanctions, of Abbring, Van den Berg and Van Ours (1997). The main difference is that in the present version of the model the sanctions process cannot be controlled by the agent.

Note that Assumption 7 follows naturally from the recursive structure of the economic decision problem in this case in which we have properly accounted for all relevant events. The model furthermore specifies that $\{E_{Y_d}\} \perp\!\!\!\perp \{E_{D_y}\}$ (Assumption 10), but is agnostic about Assumption 9. Assumption 9 would imply a deterministic relation between the potential re-employment times. If we hypothetically move the sanction back in time from d to $d' < d$, then the potential outcome would change from

$$Y_d = \min\left\{\frac{E_Y}{\lambda_0}, d\right\} + \frac{\lambda_0}{\lambda_1}\left[\frac{E_Y}{\lambda_0} - \min\left\{\frac{E_Y}{\lambda_0}, d\right\}\right]$$

to

$$Y_{d'} = \begin{cases} Y_d & \text{if } Y_d \leq d', \\ d' + \frac{\lambda_0}{\lambda_1}\left(Y_d - d'\right) & \text{if } d' < Y_d \leq d, \text{ and} \\ Y_d - \frac{\lambda_1 - \lambda_0}{\lambda_1}\left(d - d'\right) & \text{if } Y_d > d. \end{cases}$$

Clearly, strong no-anticipation (as in Lemma 2) holds.

Example 5. We can add some perspective to an early example of a structural bivariate duration model, the bivariate exponential model of Freund (1961), by restating it as a special case of our simultaneous-equations model for durations. Freund considers the survival of the components in a two-component system that can operate even if one of the components has failed. One example is a plane with two engines. Failure of one engine increases the stress on and therefore presumably the failure rate of the remaining engine.

Freund constructs a model for the failure times Y and D of the system's two components— the asymmetric notation is less appropriate here— as follows. First, he considers the distribution of the failure time of either component in the hypothetical case in which failing components are immediately replaced by new components. This intervention corresponds to our "no-treatment" case ∞ and the corresponding failure times are Y_∞ and D_∞. Freund assumes that Y_∞ and D_∞ are independently and exponentially distributed, say with hazard rates α and β.

Next, Freund considers the case of interest in which failed components are not replaced. He assumes that the failure of the second component causes a change in the failure rate of the first component from α to a possibly different, constant level α'. Similarly, the failure rate of the second component changes from β to β' when the first component fails. Freund assumes that Y and D are only dependent through these causal effects, which is what we have called randomization (Assumption 10). Clearly, this model satisfies no-anticipation (Assumption 8). We can impose Assumption 9, without necessarily accepting its strong causal implications, to ensure coherency and to facilitate an explicit construction of all potential failure times.

If we have data on (Y, D) and under Assumptions 7–10, we can identify Θ_{Y_d} and Θ_{D_y} (up to almost-sure equivalence) from standard hazard regressions (e.g. Andersen *et al.*, 1993; Fleming and Harrington, 1991). Moreover, the proof of Proposition 1 in Abbring and Van den Berg (2003) can be directly extended to prove

Proposition 3 (Non-identifiability). *Let F be any distribution on $\mathbb{R}_+^2$ with strictly positive Lebesgue density. Then, there exists some $(\{Y_d\}, \{D_y\})$ that satisfies Assumptions 7–10 and such that $(Y, D) = (Y_D, D_Y)$ has distribution F.*

Proposition 3 shows that no-anticipation (Assumption 8) and randomization (Assumption 10) can be imposed without restricting the data. In the words that Gill and Robins (2001), used in the context of the closely-related Proposition 2, these assumptions are "free". Abbring and Van den Berg point out that, from a substantial perspective, Proposition 3 implies that we cannot (i) disentangle selection effects and causal effects and (ii) identify anticipation effects without imposing further structure on the model.

Economists are typically not willing nor interested to make the type of strong causal statements that require Assumption 9. Interest usually focuses on weaker statements based on the invariance of the distribution of shocks only. Proposition 3 however implies that Assumption 9 has no empirical implications. We can therefore impose it as a modelling tool, just to ensure coherency.

It is instructive to express the model as a simultaneous-equations model in terms of Y and D. Note that we can write

$$\begin{aligned} Y &= u(E_Y, D) \\ D &= v(E_D, Y), \end{aligned} \tag{4}$$

where $u(e, d) := \Lambda_{Y_d}^{-1}(e)$ and $v(e, y) := \Lambda_{D_y}^{-1}(e)$ for $e, d, y \in \mathbb{R}_+$. The initial, dynamic setup of the model was inspired by substantial economic considerations that we would like to address in an empirical analysis. Its simultaneous-equations representation in (4) better reflects the data actually available for this analysis: at the very best an uncensored sample from the distribution of (Y, D). Clearly, (4) is not identified (and may not even be coherent) even if we assume that $E_Y \perp\!\!\!\perp E_D$ (Assumption 10). We have seen though that it is identified if, in addition, we impose Assumption 8, an assumption inspired by substantial considerations.

We are willing to take the no-anticipation Assumption 8 as fundamental. In practice, this requires that we measure all relevant informational events. We would however like to relax the randomization Assumption 10. Equation (4) suggests that we find instruments $Z \perp\!\!\!\perp (E_Y, E_D)$ that appear in one equation and not in the other, and apply some instrumental-variables method to identify and estimate the model. However, as argued in Subsection 3.6, such static instruments are not likely to be valid in the present inherently dynamic setting with forward-looking agents. Abbring and Van den Berg (2003), who explicitly recognize this problem, analyze identification in a model framework with additional structure, but without exclusion restrictions.

4.3 More general event-history models

The results of Abbring and Van den Berg (2003) are in the vein of the approach to program evaluation in which dynamic selection into programs and outcomes are jointly modelled by event-history models. Causal effects of programs are modelled as genuine state dependence. Dynamic selection effects are modelled by allowing for dependent unobserved heterogeneity in both the program and outcome transition rates. Obviously, if we do not *a priori* restrict the class of models that we consider, we can always formulate a model that attributes all observed dependence of treatment and outcome events over time to state dependence and that leaves no role for dynamic selection. This, in a nutshell, is the fundamental problem of distinguishing state-dependence and heterogeneity (Heckman, 1981, and Heckman and Borjas, 1980).

In applied work, researchers avoid this problem by imposing additional structure. Econometric research over the last 25 years has produced a variety of identification results for duration and event-history models (see Heckman and Taber, 1994, and Van den Berg, 2001, for reviews). Unfortunately, fairly little is known for some of the most popular models for the empirical analysis of dynamically assigned programs. A typical example is the mixed semi-Markov model, in which the causal effects are restricted to program participation in the previous spell (*e.g.* Bonnal, Fougère and Sérandon, 1997). Abbring (2000) provides some first results for this framework.

Of course, the importance of taking the information structure of programs into account and concerns about the validity of instruments carry over to the general event-history approach.

Example 6. An nice illustration of this point is offered by Eberwein, Ham and Lalonde (1997), who study the effects of a training program on labor-market transitions. Their data are particularly nice, as potential participants are randomized into treatment and control groups at some baseline point in time. Compliance to this intention-to-treat is however imperfect: some agents in the control group are able to enroll in substitute programs, and some agents in the treatment group never enroll in a program. Those in the treatment group are more likely to enroll though, which, together with randomization, suggests that the intention-to-treat indicator can be used as an instrument for actual participation in a training program.

An important aspect of the data is that actual enrollment is dispersed over time. One could maintain two hypotheses. First, agents could be informed about the actual time a training slot will become available. Then, they will presumably anticipate future participation in the program and change their behavior from the baseline time onwards (as in Example 5). Eberwein *et al.*, however, specify a model that excludes such anticipatory effects. Thus, they seem to opt for the second hypothesis, which is that agents cannot perfectly foresee the time at which training slots will be offered. In that case, agents will presumably respond to their intention-to-treat status even before they actually enroll in a training program. Then, intention-to-treat is not a valid instrument. Specific additional assumptions are needed to ensure that the intention-to-treat indicator is a valid instrument.

In any case, a direct analysis of the effect of the intention-to-treat indicator on labor market transitions is valid under very general conditions. Eberwein *et al.* provide such an analysis, which is directly informative on the effect of offering a larger choice set of training programs to the population under study. As in the static Neyman-Rubin framework with instruments (Subsection 2.3), a reduced-form analysis of the effect of the *instrument* on outcomes may be the more interesting one if policy makers only control the program choice set, but not actual participation.

5 Conclusion

It is clear that the analysis of dynamically assigned programs on dynamic outcomes using statistically robust methods is still in its infancy. The extensive set of tools developed for this problem in statistics does not apply directly to evaluation of programs in economies with forward-looking agents. Recent progress in econometrics focuses on phrasing and robustly analyzing appropriate dynamic econometric selection models.

References

Abbring, J.H. (2000), "The non-parametric identification of mixed semi-Markov event-history models", Mimeo, Free University, Amsterdam.

Abbring, J.H. (2002), "Stayers versus defecting movers: A note on the identification of defective duration models", *Economics Letters*, 74, 327–331.

Abbring, J.H. and J.R. Campbell (2003), "A structural empirical model of firm growth, learning, and survival", Working Paper 9712, National Bureau of Economic Research, Cambridge, MA.

Abbring, J.H. and G.J. van den Berg (2003), "The non-parametric identification of treatment effects in duration models", *Econometrica*, 71, forthcoming.

Abbring, J.H. and G.J. van den Berg (2004), "Analyzing the effect of dynamically assigned treatments using duration models, binary treatment models, and panel data models", *Empirical Economics*, 29(1), forthcoming.

Abbring, J.H., G.J. van den Berg, and J.C. van Ours (1997), "The effect of unemployment insurance sanctions on the transition rate from unemployment to employment", Working paper, Tinbergen Institute, Amsterdam.

Aldrich, J. (1989), "Autonomy", *Oxford Economic Papers*, 41, 15–34.

Andersen, P.K., Ø. Borgan, R.D. Gill, and N. Keiding (1993), *Statistical Models Based on Counting Processes*, Springer-Verlag, New York.

Angrist, J.D. and G.W. Imbens (1995), "Two-stage least squares estimation of average causal effects in models with variable treatment intensity", *Journal of the American Statistical Association*, 90, 431–442.

Angrist, J.D., G.W. Imbens, and D.B. Rubin (1996), "Identification of causal effects using instrumental variables", *Journal of the American Statistical Association*, 91, 444–455.

Billingsley, P. (1995), *Probability and Measure*, Wiley, New York, third edition.

Björklund, A. and R. Moffitt (1987), "The estimation of wage gains in self-selection models", *Review of Economics and Statistics*, 69, 42–49.

Black, D.A., J. Smith, M.C. Berger, and B.J. Noel (1999), "Is the threat of training more effective than training itself? Experimental evidence from UI claimant profiling", Mimeo, Syracuse University, Syracuse, NY.

Blundell, R. and J.L. Powell (2000), "Endogeneity in nonparametric and semiparametric regression models", Mimeo, University College London and UC Berkeley.

Bonnal, L., D. Fougère, and A. Sérandon (1997), "Evaluating the impact of French employment policies on individual labour market histories", *Review of Economic Studies*, 64, 683–713.

Card, D. and D. Sullivan (1988), "Measuring the effect of subsidized training programs on movements in and out of employment", *Econometrica*, 56, 497–530.

Carneiro, P., K.T. Hansen, and J.J. Heckman (2003), "Estimating distributions of treatment effects with an application to the returns to schooling and measurement of the effects of uncertainty on college choice", Working Paper 9546, National Bureau of Economic Research.

Dawid, A.P. (2000), "Causal inference without counterfactuals", *Journal of the American Statistical Association*, 95, 407–448.

Eberwein, C., J.C. Ham, and R.J. Lalonde (1997), "The impact of being offered and receiving classroom training on the employment histories of disadvantaged women: Evidence from experimental data", *Review of Economic Studies*, 64, 655–682.

Engle, R.F., D.F. Hendry, and J.-F. Richard (1983), "Exogeneity", *Econometrica*, 51, 277–304.

Fleming, T.R. and D.P. Harrington (1991), *Counting Processes and Survival Analysis*, Wiley, New York.

Freedman, D.A. (2002), "On specifying graphical models for causation, and the identification problem", Technical Report 601, Department of Statistics, University of California, Berkeley, CA.

Freund, J.E. (1961), "A bivariate extension of the exponential distribution", *Journal of the American Statistical Association*, 56, 971–977.

Frisch, R. (1938), "Autonomy of economic relations", Mimeo, League of Nations [Unpublished; Reprinted in D.F. Hendry and M.S. Morgan (1995), *The Foundations of Econometric Analysis*, Cambridge University Press].

Gill, R.D. and J.M. Robins (2001), "Causal inference for complex longitudinal data: The continuous case", *Annals of Statistics*, 29, 1785–1811.

Goldberger, A.S. (1972), "Structural equation methods in the social sciences", *Econometrica*, 40, 979–1001.

Gritz, R.M. (1993), "The impact of training on the frequency and duration of employment", *Journal of Econometrics*, 57, 21–51.

Grubb, D. (1999), "Making work pay: The role of eligibility criteria for unemployment benefits", Mimeo, OECD, Paris.

Heckman, J.J. (1981), "Heterogeneity and state dependence", in S. Rosen, editor, *Studies in Labor Markets*, University of Chicago Press.

Heckman, J.J. (1997), "Instrumental variables: A study of implicit behavioral assumptions used in making program evaluations", *Journal of Human Resources*, 32, 441–462.

Heckman, J.J. (2000), "Causal parameters and policy analysis in economics: A twentieth century retrospective", *Quarterly Journal of Economics*, 115, 45–97.

Heckman, J.J. and G.J. Borjas (1980), "Does unemployment cause future unemployment? Definitions, questions and answers from a continuous time model of heterogeneity and state dependence", *Economica*, 47, 247–283.

Heckman, J.J. and B.E. Honoré (1989), "The empirical content of the Roy model", *Econometrica*, 58, 1121–1149.

Heckman, J.J., R.J. Lalonde, and J.A. Smith (1999), "The economics and econometrics of active labor market programs", in O. Ashenfelter and D. Card, editors, *Handbook of Labor Economics, Volume III*, North-Holland, Amsterdam.

Heckman, J.J. and J.A. Smith (1995), "Assessing the case for social experiments", *Journal of Economic Perspectives*, 9, 85–110.

Heckman, J.J. and J.A. Smith (1998), "Evaluating the welfare state", in S. Strom, editor, *Econometrics and Economic Theory in the 20th Century. The Ragnar Frisch Centennial Symposium*, Econometric Society Monographs 31, Cambridge University Press, Cambridge.

Heckman, J.J. and C.R. Taber (1994), "Econometric mixture models and more general models for unobservables in duration analysis", *Statistical Methods in Medical Research*, 3, 279–302.

Heckman, J.J. and E. Vytlacil (2000), "Causal parameters, structural equations, treatment effects and randomized evaluations of social programs", Mimeo, University of Chicago, Chicago.

Heckman, J.J. and E. Vytlacil (2000), "The relationship between treatment parameters within a latent variable framework", *Economics Letters*, 66, 33–39.

Heckman, J.J. and E. Vytlacil (2001), "Policy-relevant treatment effects", *American Economic Review: Papers and Proceedings*, 91, 107–111.

Hendry, D.F. and M.S. Morgan (1995), *The Foundations of Econometric Analysis*, Cambridge University Press, Cambridge.

Holland, P.W. (1986), "Statistics and causal inference", *Journal of the American Statistical Association*, 81, 945–960.

Ichimura, H. and C.R. Taber (2000), "Direct estimation of policy impacts", Mimeo, University College London and Northwestern University.

Imbens, G.W. and J.D. Angrist (1994), "Identification and estimation of local average treatment effects", *Econometrica*, 62, 467–475.

Imbens, G.W. and W.K. Newey (2001), "Identification and estimation of triangular simultaneous equations models without additivity", Mimeo, UC Berkeley and MIT.

Keiding, N. (1999), "Event history analysis and inference from observational epidemiology", *Statistics in Medicine*, 18, 2353–2363.

Lechner, M. (2003), "Sequential matching estimation of dynamic causal models", Mimeo, University of St. Gallen.

Lechner, M. and R. Miquel (2002), "Identification of effects of dynamic treatments by sequential conditional independence assumptions", Mimeo, University of St. Gallen.

Lillard, L.A. (1993), "Simultaneous equations for hazards", *Journal of Econometrics*, 56, 189–217.

Lillard, L.A. and C.W.A. Panis (1996), "Marital status and mortality: the role of health", *Demography*, 33, 313–327.

Lok, J.J. (2001), *Statistical Modelling of Causal Effects in Time*, PhD thesis, Division of Mathematics and Computer Science, Faculty of Sciences, Free University, Amsterdam.

Lucas, Jr., R.E. (1976), "Econometric policy evaluation: A critique", in K. Brunner and A. Meltzer, editors, *The Phillips Curve and the Labor Market*, North-Holland, Amsterdam.

Magnac, T. and D. Thesmar (2002), "Identifying dynamic discrete choice processes", *Econometrica*, 70, 801–816.

Mortensen, D.T. (1977), "Unemployment insurance and job search decisions", *Industrial and Labor Relations Review*, 30, 505–517.

Mortensen, D.T. (1986), "Job search and labor market analysis", in O. Ashenfelter and R. Layard, editors, *Handbook of Labor Economics*, North-Holland, Amsterdam.

Murphy, S.A. (2002), "Optimal dynamic treatment regimes", *Journal of the Royal Statistical Society Series B*, forthcoming.

Neyman, J. (1923), "On the application of probability theory to agricultural experiments. Essay on principles", *Roczniki Nauk Rolniczych*, 10, 1–51 [in Polish; edited and translated version of Section 9 by D.M. Dabrowska and T.P Speed (1990), *Statistical Science*, **5**, 465–472].

Pearl, J. (2000), *Causality: Models, Reasoning, and Inference*, Cambridge University Press, Cambridge, UK.

Ridder, G. (1986), "An event history approach to the evaluation of training, recruitment and employment programmes", *Journal of Applied Econometrics*, 1, 109–126.

Robins, J.M. (1986), "A new approach to causal inference in mortality studies with a sustained exposure period— application to control of the healthy worker survivor effect", *Mathematical Modelling*, 7, 1393–1512.

Robins, J.M. (1997), "Causal inference from complex longitudinal data", in M. Berkane, editor, *Latent Variable Modeling and Applications to Causality. Lecture Notes in Statistics 120*, 69–117, Springer Verlag, New York.

Robins, J.M. (1998), "Marginal structural models versus structural nested models as tools for causal inference", in *AAAI Technical Report Series: Spring 1998 Symposium on Prospects for a Common Sense Theory of Causation*, Stanford, CA.

Robins, J.M. (1998), "Structural nested failure time models", in P. Armitage and T. Colton, editors, *The Encyclopedia of Biostatistics*, John Wiley and Sons, Chichester.

Robins, J.M. (2002), "Optimal-regime estimation", Paper presented at the Statistical Day 2002 of the Netherlands Society for Statistics and Operations Research, Harvard University, Boston.

Rosenzweig, M.R. and K.I. Wolpin (2000), "Natural "natural experiments" in economics", *Journal of Economic Literature*, 38, 827–874.

Rubin, D.B. (1974), "Estimating causal effects of treatments in randomized and nonrandomized studies", *Journal of Educational Psychology*, 66, 688–701.

Rubin, D.B. (1986), "Statistics and causal inference: Comment. Which ifs have causal answers", *Journal of the American Statistical Association*, 81, 961–962.

Rust, J. (1994), "Structural estimation of Markov decision processes", in R.F. Engle and D.L. McFadden, editors, *Handbook of Econometrics*, volume 4, 3081–3143, North-Holland, Amsterdam.

Taber, C. (2000), "Semiparametric identification and heterogeneity in discrete choice dynamic programming models", *Journal of Econometrics*, 96, 201–229.

Theil, H., "A note on certainty equivalence in dynamic planning", *Econometrica*, 25, 346–349.

Theil, H. (1953), "Repeated least squares applied to complete equation systems", Mimeo, Centraal Planbureau, The Hague [Unpublished; Reprinted in B. Raj and J. Koerts

(1992), *Henri Theil's Contributions to Economics and Econometrics*, Kluwer Academic Publishers, Dordrecht].

Theil, H. (1971), *Principles of Econometrics*, Wiley, New York.

Van den Berg, G.J. (2001), "Duration models: Specification, identification, and multiple durations", in J.J. Heckman and E. Leamer, editors, *Handbook of Econometrics*, volume V, North Holland.

Van den Berg, G.J., B. van der Klaauw, and J.C. van Ours (2004), "Punitive sacntions and the transition rate from welfare to work", *Journal of Labor Economics*, 22, forthcoming.

Vytlacil, E. (2002), "Independence, monotonicity, and latent index models: An equivalence result", *Econometrica*, 70, 331–341.

Zellner, A. and H. Theil, "Three-stage least squares: Simultaneous estimation of simultaneous equations", *Econometrica*, 30, 54–78.

IZA Discussion Papers

No.	Author(s)	Title	Area	Date
790	A. Cigno A. Luporini A. Pettini	Hidden Information Problems in the Design of Family Allowances	3	06/03
791	M. Güell	Fixed-Term Contracts and the Duration Distribution of Unemployment	1	06/03
792	L. Borghans B. ter Weel	What Happens When Agent T Gets a Computer? The Labor Market Impact of Cost Efficient Computer Adoption	5	06/03
793	T. Palokangas	Foreign Direct Investment, Labour Market Regulation and Self-Interested Governments	3	06/03
794	P. Frijters M. A. Shields S. Wheatley Price	Investigating the Quitting Decision of Nurses: Panel Data Evidence from the British National Health Service	1	06/03
795	B. T. Hirsch	Reconsidering Union Wage Effects: Surveying New Evidence on an Old Topic	3	06/03
796	P. Apps	Gender, Time Use and Models of the Household	5	06/03
797	E. Bratberg Ø. A. Nilsen K. Vaage	Assessing Changes in Intergenerational Earnings Mobility	2	06/03
798	J. J. Heckman J. A. Smith	The Determinants of Participation in a Social Program: Evidence from a Prototypical Job Training Program	6	06/03
799	R. A. Hart	General Human Capital and Employment Adjustment in the Great Depression: Apprentices and Journeymen in UK Engineering	2	06/03
800	T. Beissinger C. Knoppik	Sind Nominallöhne starr? Neuere Evidenz und wirtschaftspolitische Implikationen	7	06/03
801	A. Launov	A Study of the Austrian Labor Market Dynamics Using a Model of Search Equilibrium	2	06/03
802	H. Antecol P. Kuhn S. J. Trejo	Assimilation via Prices or Quantities? Labor Market Institutions and Immigrant Earnings Growth in Australia, Canada, and the United States	1	06/03
803	R. Lalive	Social Interactions in Unemployment	3	06/03
804	J. H. Abbring	Dynamic Econometric Program Evaluation	6	06/03