Essays in applied dynamic microeconometrics
Kastoryano, S.P.

Citation for published version (APA):
Kastoryano, S. P. (2013). Essays in applied dynamic microeconometrics Amsterdam: Rozenberg

General rights
It is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), other than for strictly personal, individual use, unless the work is under an open content license (like Creative Commons).

Disclaimer/Complaints regulations
If you believe that digital publication of certain material infringes any of your rights or (privacy) interests, please let the Library know, stating your reasons. In case of a legitimate complaint, the Library will make the material inaccessible and/or remove it from the website. Please Ask the Library: http://uba.uva.nl/en/contact, or a letter to: Library of the University of Amsterdam, Secretariat, Singel 425, 1012 WP Amsterdam, The Netherlands. You will be contacted as soon as possible.
Chapter 1

Introduction

The ideal approach in microeconomics to evaluate the effect of a policy on an outcome of interest is to conduct a randomized experiment. As its name suggests, this approach randomly assigns some individuals - the treatment group - to some policy measure and others - the control group - to a baseline without the policy. The effect of the policy is given by the difference in some outcome(s) of interest between these two groups at a later point in time. This thesis focuses on the evaluation of policies when the usual static framework of a randomized experiment can not or does not apply perfectly. The first two chapters look at microeconometric evaluation methods to analyze dynamic situations when the policy assigns treatment at different times and the measurement of the outcome of interest also depends upon a time dimension. The last two chapters of this thesis are empirical crime studies on tax evasion and street prostitution. These exploit a different variation over time using panel data methods to evaluate the policy effects.

1.1 A short example

Consider the following evaluation problem. In a developing country, a policymaker or researcher is interested in evaluating the effect of a cash transfer to poor households on their children’s years of schooling. These cash transfers were randomly allocated over time and were intended to be used for books or other schooling material. Answering whether the cash transfer had positive, negative or no impact on the number of years children spent enrolled in school first requires formulating the appropriate evaluation question. This evaluation question depends on several dynamic aspects. A cash transfer can be given at different moments of a child’s education (elementary, 1st grade, 2nd grade, etc.). It may be that the cash transfer has a different effect depending upon the time it is allocated to poor families. It might, for instance, have no effect when allocated in the early years.
of a child’s education but have positive effects if transferred in later years. The effects may also depend upon the moment of evaluating the school enrollment outcome. It could have positive effects on enrollment shortly after the cash transfer but negative effects over longer time horizons. These variations in effects are important for policymakers interested in optimally allocating the cash transfers.

Another important consideration is to understand how the behavior of economic agents may interfere with the evaluation of a policy. For example, when choosing the moment to evaluate the treatment, some students may have already left school before their families receive the cash transfer. These students can not be ignored when evaluating the effects of the cash transfer since they are not a random selection of students. These may be the students with the worst grades who drop out or the ones from poor families needing to work to contribute to the income of their household. Furthermore, if families find out about the cash transfer program, they may adjust their behavior prior to the moment of receiving it in expectation of additional future income or in order to qualify for the transfer. This dynamic selection complicates the analysis. Even if cash transfers are randomly assigned initially when children first enter school, the treatment and control groups will no longer be random at the moment the transfer is actually allocated nor when evaluating enrollment outcomes.

1.2 Outline of dynamic treatment evaluation

Chapter 2 and Chapter 3 in this thesis compare existing dynamic microeconometric approaches, propose new methods, and apply them to real and simulated data. The emphasis in these methods is to adapt the static potential outcomes framework (Holland, 1986) to dynamic settings in an effort to minimize functional form assumptions. The basic layout of the analysis applies to situations where we observe for each individual the duration $T > 0$ in some initial state (e.g. years of schooling). We define the binary variables $Y_t$ as indicators for remaining in the initial state ($Y_t = 0$) (e.g. enrolled in school) or leaving it ($Y_t = 1$) after $t$ periods, so $Y_t = I(T \leq t)$. This outcome variable describes the survival in the initial state.

We focus on a situation in which individuals can receive a single treatment (e.g. a cash transfer) only once and the timing at which individuals receive treatment differs. Let $S > 0$ denote the elapsed duration at the moment of receiving the treatment. In dynamic settings, some people may have left the initial state before receiving treatment. Without additional parametric assumptions, it is not possible to describe the effect of treatment on these individuals. Instead, the evaluation usually considers only the people who did
not leave the initial state at the moment of evaluating the treatment.

Let $Y^*_{1,t}(s)$ denote the potential outcome status after $t$ periods if the individual was treated after $s$ periods. A natural counterfactual to consider is the potential outcome $Y^*_{0,t}$, which is the outcome had the individual not received treatment prior to $t$. This thesis goes through the identifying conditions required to formulate an appropriate counterfactual in different treatment policy settings. Identification is based on assuming that there exists no information influencing an agent’s outcome which is not observed and can not be modeled by the econometrician. When this is the case, a relevant treatment effect on the surviving treated would be,

$$
\Delta(t, s) = E \left[ Y^*_{1,t}(s) - Y^*_{0,t} | S = s, Y_s = 0 \right] \quad \text{with } t > s
$$

This treatment effect denotes the effect of providing treatment at $s$ on outcomes at $t$ for those who were still in the initial state at $s$. This is the ex-post effect of the treatment on future outcomes. Individuals treated at $s$ are thus compared to individuals who (possibly) receive treatment after $t$. So even though we only consider a single treatment, it may have different effects when applied at different time periods. The main empirical complication is that it is unclear which individuals qualify for the control group. Chapter 2 and 3 extend this basic framework to allow for a wider variety of treatment effects such as ex-ante effects and total policy effects. We consider both non-parametric discrete dynamic methods and semi-parametric continuous-time methods.

Chapter 2 evaluates a job search assistance program for unemployment insurance recipients where the assignment to the program follows the dynamic framework outlined above. We provide a more detailed discussion on dynamic treatment effects and identification conditions. In the empirical analyses we use administrative data from a unique institutional environment which allows us to compare different microeconometric evaluation estimators. All estimators find that the job search assistance program reduces the exit rate to work, in particular when provided early during the spell of unemployment. Furthermore, continuous-time (timing-of-events and regression discontinuity) methods are more robust than discrete-time (propensity score and regression discontinuity) methods.

Chapter 3 focuses on extending the methodology of Chapter 2 to a more general framework. It considers the evaluation of a policy effect in dynamic settings when there is an initial randomization to treatment and a treatment at a later time. We assess again in this chapter the underlying assumptions and relative performance of several discrete and continuous-time microeconometric treatment evaluation techniques when actual treatment depends on dynamic selection. We provide a discussion on defining parameters of interest in different policy settings with ongoing entry in treatment and anticipation behavior. We compare the commonly applied 2SLS to dynamic IV methods and propose a
new method to estimate ex-ante and ex-post dynamic treatment effects when agents adapt their behavior prior to entering treatment. To illustrate the performance of the competing methods we present a simulation study using a simple structural dynamic model. The results underline the sensitivity of IV methods, and in particular of 2SLS, to the choice of population entering the control group. Our dynamic non-parametric and dynamic least squares methods produce reliable estimates of ex-ante, ex-post and total effects in our simulation when agents anticipate treatment.

1.3 Effects of economic policies on crime

The last two chapters depart from the dynamic framework presented above and use different forms of exogenous variation over time to identify the effects of a policy. These two chapters look at topics in the economics of crime and put a stronger focus on the underlying incentives influencing criminal choices and the behavioural responses to different policies. The microeconomic behaviour of criminals in both chapters takes it’s roots in Becker’s (1968) economics of crime model. Becker explains the choice to commit an offense as the result of an individual weighing the potential gains from a crime with the probability of apprehension and the punishment if caught. In his model, a rational economic agent maximizes expected utility $E[U],$

$$E[U] = pU(Y - C) + (1 - p)U(Y)$$

where $U(\cdot)$ is the agent’s utility function. The agent will commit an offense if the monetary and psychic benefits $Y$ are sufficiently high, and the probability of apprehension $p$ and the monetary equivalent of the punishment $C$ are sufficiently low. Much of the discussion in Chapters 4 and 5 draws from extensions and refinements of this model when applied to the topics of tax evasion and crime displacement.

Chapter 4 considers the effect of random auditing announcements on tax declarations. In the Netherlands, the tax authorities announce at the beginning of the tax-filing period a specific component in tax reports which will receive special attention when auditing. Theoretical models predict that rational taxpayers will reduce misreporting on these components in response to stricter auditing (i.e. when the probability of apprehension $p$ goes up). We use detailed administrative data from the Dutch tax authorities (Belastingdienst) to detect unusual patterns in tax reports due to two announcements. We find increased declarations of secondary property, residual assets and freelance income not subject to third-party reporting. We also observe patterns in property and residual assets declarations which presuppose that increases are not always directly visible on the
tax item targeted by the announcement but reveal themselves in other overlapping topics. The substitution patterns suggest that taxpayers try to reduce their declarations in the announcement topic whenever possible and declare previously underreported income and wealth across other topics in an effort to minimize their tax burden. When shifting declarations to other sections is not possible, taxpayers increase their declarations in the topic targeted by the auditing announcement.

The last chapter, chapter 5, considers the effect of legal street prostitution zones (tippelzones) on crime in the 30 largest cities in the Netherlands. Using a difference-in-difference model we evaluate the opening and closing effects of tippelzones on both reported and perceived crime. We find that open street prostitution zones are associated with a decrease in registered crime but an increase in perceived crime. These results are mainly driven by strong closing effects. For registered crime, the decreases are noticeable after a year while the increase in perceived crime is strongest immediately after the closing of a tippelzone.