Exploring the Usefulness of a Non-Random Holdout Sample for Model Validation: Welfare Effects on Female Behavior

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May, 2005

The authors are grateful for support from NICHD under grant HD-34019 and from several grants from the Minnesota Supercomputer Institute. Part of Keane's work on this project was completed while he was visiting Arizona State University as the Goldwater Chair in American Institutions.

I. Introduction

Opportunities for external validation of behavioral models in the social sciences that are based on randomized social experiments or on large regime shifts, that can be treated as experiments for the purpose of model validation, are extremely rare**.** Among the earliest examples in which such a regime shift is exploited is work by McFadden (1977) on forecasting the demand for rail rapid transport in the San Francisco Bay area. McFadden estimated a random utility model (RUM) of travel demand before the introduction of the Bay Area Rapid Transit (BART) system, obtained a forecast of the level of patronage that would ensue, and then compared the forecast to actual usage after BART's introduction.[1] Since that work, there have been, to our knowledge, only a handful of papers in the economics literature that have pursued a similar method of model validation.[2]

McFadden's model validation treats pre-BART observations as the estimation sample and post-BART observations as the validation sample. A similar opportunity was exploited by Lumsdaine, Stock, and Wise (1992). They estimated a model of retirement behavior of workers in a single firm who were observed before and after the introduction of a temporary one-year pension window. They estimated several models on data before the window was introduced and compared the forecast of the impact of the pension window on retirement based on each estimated model to the actual impact as a means of model validation and selection. Keane and Moffitt (1998) estimated a model of labor supply and welfare program participation using data after federal legislation (OBRA 1981) that significantly changed the program rules. They used

[1] A regime shift, as opposed to a randomized experiment, is characterized by a time lapse between observations on the estimation sample (the control group) and those on the validation sample (the treatment group). Over that period, changes may have occurred that would affect behavior in ways not captured in the estimation. In addition, whatever assumption is made about the exogeneity of a regime shift becomes part of the validation exercise.

[2] The use of models to forecast out-of-sample behavior is not uncommon. For example, in the marketing literature, considerable effort has been devoted to forecasting demand for new products. Few of the papers in that literature, however, compare predictions to subsequent demand after the product is introduced.

the model to predict behavior prior to that policy change. Keane (1995) used the same model to predict the impact of planned expansions of the Earned Income Tax Credit in 1994-1996.

Randomized social experiments have also provided opportunities for model validation and selection. Wise (1985) exploited a housing subsidy experiment as a means of evaluating a model of housing demand. In the experiment, families that met an income eligibility criterion were randomly assigned to control and treatment groups. Those in the latter group were offered a rent subsidy. The model was estimated using only control group data and was used to forecast the impact of the program on the treatment group. The forecast was compared to its actual impact. Lalonde (1986) used data from a manpower training experiment to evaluate the ability of non-experimental methods to replicate program effects. Heckman and Hotz (1989) developed methods for choosing among alternative non-experimental methods using data on the control group (and on a non-randomly chosen comparison group).[3]

More recently, Todd and Wolpin (2002) made use of data from a large-scale school subsidy experiment in Mexico, where villages were randomly assigned to control and treatment groups. Todd and Wolpin estimated a behavioral model of parental decisions about child schooling and work, as well as family fertility, using data on the control villages and used it to predict behavior in the treatment villages. The validity of the model was then assessed according to how well the forecast of the behavior of the treatment group under the program matched the actual behavior. Similarly, Lise, Seitz and Smith (2003) used data from a Canadian experiment designed to move people off of welfare and into work to validate a calibrated search-matching model of labor market behavior.[4]

When the model provides sufficient structure, and assuming that the model is deemed "valid", it is possible to simulate the impact of regime shifts other than the one used for validation. For example, Wise (1985) and Todd and Wolpin (2002) contrasted the effect of the

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[3] They also developed model selection methods based on pre-program data alone.

[4] The use of laboratory experiments to validate economic models has, of course, a long tradition. Bajari and Hortascu (forthcoming) provide a recent example of evaluating a structurally estimated auction model by comparing the estimated valuations to those randomly assigned in an experimental setting.

policies evaluated in the experiments to several alternative policies.

All of these papers make use of what is, from the researcher's perspective, a fortuitous event. The common and essential element is the existence of some form of a regime change that is radical enough to provide a degree of distance between the estimation sample and the validation sample. The further away are the regimes in the estimation and validation samples, the less likely the forecasted and actual behavior of the validation sample will be close purely by chance.

However, waiting for such events to arise, given their rarity, does not lead to a viable research approach to model validation and selection.[5] In this paper, we consider an alternative approach, namely mimicking the essential element of regime change by non-randomly holding out from estimation a portion of the sample that faces a significantly different policy regime. The non-random holdout sample is used for model validation/selection.[6] Of course, using random subsamples of the data as holdout samples as a means of model selection has been a common procedure in statistics and econometrics. Unlike these cross-validation methods, here the holdout sample is chosen in a non-random manner (i.e., precisely because it contains data from a very different policy regime).

In this paper, we illustrate the non-random holdout sample approach to model validation in the context of a model of welfare program participation. The policy heterogeneity that we exploit to generate a non-random hold-out sample takes advantage of the wide variation across states that has existed in welfare policy. Specifically, we formulate and estimate a dynamic programming (DP) model of the joint schooling, welfare take-up, work, fertility and marriage decisions of women using data from one group of U.S. states (the estimation or "control"

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[5] In this regard, the "natural `natural experiments`," literature suffers from the same problem. This phrase has been used by Rosenzweig and Wolpin (2000) to distinguish "natural experiments" that are both natural, i.e., provided by nature, and experiment-like, in the sense of random assignment, from those that are neither.

[6] Eckstein and Wolpin (1990) and Bontemps, Robin, and Vandenberg (2000) follow a related, but somewhat different, method of validation. The similarity to what we suggest is that both of these studies purposively hold out some piece of non-randomly selected data that could have been used in estimation. The difference is that in those papers all of the data are generated within the same regime.

sample) and forecast these same decisions on another state (the validation or "treatment" sample) that differs dramatically in the generosity of its welfare program. As a comparison to the performance of the DP model, we also estimate several multinomial logit (MNL) specifications, consistent with a static random utility model or a flexible approximation to a DP model, albeit, to conserve on parameters, only for a subset of the choices.

Our model extends the literature on welfare participation in several dimensions.[7] We augment the choice set to include schooling and fertility in addition to work, marriage and welfare participation and model these choices within a dynamic framework. We implement the model using 15 years of information from the 1979 youth cohort of the National Longitudinal Surveys of Labor Market Experience (NLSY79), supplemented with state level welfare benefit rules that we have collected for each state over a 23 year period prior to the new welfare reform. The model is estimated on five of the largest states represented in the NLSY79 (California, Michigan, New York, North Carolina and Ohio) and validated on data from Texas. In terms of generosity, California, Michigan and New York are high benefit states, North Carolina and Ohio are medium benefit states and Texas is a low benefit state.

All of the models, the DP model and the different specifications of the static MNL models, perform well in terms of their fit to the estimation sample. Indeed, it is difficult to choose among them. Performance on the validation sample is more varied. Specifically, based on a root mean squared error criterion, a MNL specification with state fixed-effects provides the best out-of-sample prediction.

However, when we perform a counterfactual experiment that replaces the welfare benefit realizations in the estimation sample states with those for Texas, the effects on behavior predicted by the MNL fixed-effects model are seemingly perverse - welfare participation and fertility increase substantially, while working declines substantially. The MNL specification that replaces the state fixed-effects with state-specific mean benefits, representing permanent differences in welfare generosity, leads to expected effects. Welfare participation declines and employment increases. However, the increase in employment rates (in some cases, as large as 20

[7] See Moffitt (1992) for a review of the early literature based on static models. Previous DP models of welfare participation include Sanders (1993) and Swann (1996).

percentage points) substantially exceeds the fall in welfare take-up rates, a result that does not seem plausible. Moreover, there is a significant drop in schooling, which contradicts the prediction of a human capital model that an agent who expects to spend more time working and less time on welfare has a greater incentive to invest in education. In contrast, the DP model predictions for the counterfactual experiment are quantitatively more reasonable. The decline in welfare participation rates exceeds the increase in employment rates (which are less than 5 percentage points), and schooling increases slightly.

Furthermore, the DP model has two important advantages. First, being more comprehensive, it can be used to forecast the effects of policy changes on additional variables of interest: marriage rates, part- and full-time work, parental co-residence rates, husband's income, and wage offers for part- and full-time work. Second, it is possible to forecast the effect of policies other than variations in benefit levels, for example, work requirements, time limits and wage and school subsidies, among others.

The next section of the paper presents the structure of the DP model. Section 3 describes the data, section 4 the estimation method and the following section the results. The final section concludes.

II. Model

In this section, we provide an outline of the model.[8] We consider a woman who makes joint decisions at each age "a" of her lifetime about the following set of discrete alternatives: whether or not to attend school, s_a, work part-time, h_a^p, or full-time, h_a^f, in the labor market (if an offer is received), be married (if an offer is received), m_a, become pregnant if the woman is of a fecund age, p_a, and receive government welfare if the woman is eligible, g_a. There are as many as 36 mutually exclusive alternatives that a woman chooses from at each age during her fecund life cycle stage and 18 during her infecund stage.[9] The fecund stage is assumed to begin

[8] A complete description with exact functional forms is provided in Appendix A of our working paper of the same title (Keane and Wolpin (2005)).

[9] Being married and receiving welfare is not an option. Although the AFDC-Unemployed Parent (AFDC-UP) program provided benefits for a family with an unemployed father, it accounts for only a small proportion of total spending on AFDC.

at age 14 and to end at age 45; the decision period extends to age 62. Decisions are made at discrete six month intervals, i.e., semi-annually. A woman who becomes pregnant at age a has a birth at age a+1, with n_{a+1} representing the discrete birth outcome.[10] Consumption, C_a, is determined uniquely by the alternative chosen.

The woman receives a utility flow at each age that depends on her current choices and consumption, and on state variables that reflect past choices, for example, on the number of children already born, N_a, and their current ages (which affect child-rearing time costs), and the current level of completed schooling, S_a (which affects utility from attendance). We also allow preferences to evolve with age and to differ among individuals by birth cohort, race and U.S. state of residence and by a permanent unobservable characteristic which we denote by a woman's type.[11] The utility of specific choices are subject to age-varying preference shocks. Expressing the utility function in terms of the current set of alternatives, the utility of an individual at age a who is of type j is

$$(1) \quad U_a^j = U_a(C_a,\ s_a,\ m_a,\ p_a,\ g_a,\ h_a^p,\ h_a^f;\ \epsilon_a,\ I(\text{type}=j),\ \Omega_a^u),$$

where ϵ_a is a vector of five serially independent preference shocks and Ω_a^u represents the subset of the state space (the set of past choices and fixed observables) that affects utility.[12, 13]

[10] In keeping with the assumption that pregnancies can be perfectly timed, we only consider pregnancies that result in a live birth, i.e., we ignore pregnancies that result in miscarriages or abortions. We assume that a woman cannot become pregnant in two consecutive six month periods.

[11] In the model, we assume that women do not change their state of residence and restrict our estimation to a sample with that characteristic.

[12] $I(\cdot)$ is the indicator function equal to one when the term inside is true and zero otherwise.

[13] Monetary costs, when unmeasured, are not generally distinguishable from psychic costs. It is thus somewhat arbitrary as to what is included in the utility function as opposed to the budget constraint. For example, we include in (1): (i) a fixed cost of working; (ii) a time cost of rearing children that varies by their ages; (iii) a time cost of collecting welfare (waiting at the welfare office); (iv) a school re-entry cost; and (v) costs of switching welfare and employment

The budget constraint, assumed to be satisfied each period, is given by:

$$(2)\quad \begin{aligned} C_a &= y_a^o(1-m_a)(1-z_a) + [y_a^o + y_a^m]m_a\tau_a^m + [y_a^o + y_a^z\tau_a^z]z_a \\ &\quad + \beta_1 g_a b_a + \beta_2 g_a z_a y_a^z - [\beta_3 I(S_a \geq 12) - \beta_4 I(S_a \geq 16)]s_a, \end{aligned}$$

where y_a^o is the woman's own earnings at age a, y_a^m is the spouse's earnings if the woman is married, τ_a^m is the share of household income the woman receives if she is married, y_a^z is her parents' income, a share, τ_a^z, of which she receives if she co-resides with her parents, b_a is the amount of welfare benefits the woman is eligible to receive. β_1 is a fraction that converts welfare dollars into a monetary equivalent consumption value, β_2 represents the fraction by which welfare benefits are reduced if the woman lives with her parents and varies with the level of the parents' income, β_3 is the tuition cost of college and β_4 the cost of graduate school, S_a is the completed level of schooling at age a and $I(\cdot)$ is an indicator function equal to unity when the argument in the parentheses is true.[14] By assumption, income is pooled when married, but not when co-residing with parents.

Living with parents and being married are taken to be mutually exclusive states. In particular, a woman who chooses to be married, conditional on receiving a marriage offer (see below), cannot live with her parents while a woman who does not choose to be married lives with her parents according to a draw from an exogenous probability rule, π_a^z. We assume that the probability of co-residing with her parents, given the woman is unmarried, depends on her age. The woman's share of her parents' income, when co-resident, depends on her age, her parents' schooling and whether she is attending post-secondary school.

It is assumed that there is stochastic assortative mating. In each period a single woman

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states.

[14] β_1 reflects the fact that welfare recipients are restricted in what they may purchase with welfare benefits, e.g., food stamps cannot be used to purchase alcohol. In addition, the exact treatment of parents' income is quite complicated, varying among and within states (at the local welfare agency level) and over time. Rather than attempting to model the rules explicitly, as an approximation we instead estimate the fraction of parents' income that is subject to tax as a parameter, β_3.

draws an offer to marry with probability π_a^m, that depends on her age and welfare status. If the woman is currently married, with some probability that depends on her age and duration of marriage, she receives an offer to continue the marriage. If she declines to continue, the woman must be single for one period (six months) before receiving a new marriage offer.

In each period a woman receives a part-time job offer with probability π^{wp} and a full-time job offer with probability π^{wf}. Each of these offer rates depends on the woman's previous-period work status. If an offer is received and accepted, the woman's earnings is the product of the offered hourly wage rate and the number of hours she works, $y_a^o = 500 \cdot w_a^p h_a^p + 1000 \cdot w_a^p h_a^f$. The hourly wage rate is the product of the woman's human capital stock, Ψ_a, and its per unit rental price, which is allowed to differ between part- and full-time jobs, r^j for j=p, f. Specifically, her ln hourly wage offer is

$$(3) \quad \ln w_a^j = r^j + \Psi(\cdot) + \epsilon_a^w, \quad j = p, f.$$

The woman's human capital stock is modeled as a function of completed schooling, the stock of accumulated work hours up to age a, H_a, whether or not the woman worked part- or full-time in the previous period, her current age and her skill endowment at age 14. As with permanent preference heterogeneity, the skill endowment differs by race, state of residence and unobserved type. Random shocks to a woman's human capital stock, $\epsilon_a{}^w$, are assumed to be serially independent.

The husband's earnings depends on his human capital stock, Ψ_a^m. Conditional on receiving a marriage offer, the potential husband's human capital is drawn stochastically. The human capital of the spouse that is drawn depends on a subset of the woman's characteristics, her schooling attainment, age, race, state of residence and unobserved (to us) type. In addition, there is an iid random component to the draw of the husband's human capital that reflects a permanent characteristic of the husband unknown to the woman prior to meeting, μ^m. The woman can therefore profitably search in the marriage market for husbands with more human capital, and can also directly affect the quality of their husbands by the choice of her schooling. There is a fixed utility cost of getting married, which augments a woman's incentive to wait for a good husband draw before choosing marriage (we allow for a cohort effect in this fixed cost). After marriage, the woman receives a utility flow from marriage, as well as a share of husband income. After marriage, husband's earnings evolve with a fixed trend subject to a serially independent random shock, ϵ_a^m. Specifically,

$$(4) \quad \ln y_a^m = \mu^m + \Psi_{0a}^m(\cdot) + \epsilon_a^m$$

where Ψ_{0a}^m is the deterministic component of the husband's human capital stock.

Welfare eligibility and the benefit amount for a woman residing in state s at calendar time t depends on the number of children residing with her and on her household income. For any given number of minor children (under the age of 18, N_a^{18}) residing in the household, the schedule of benefits can be accurately approximated by two line segments. The first line segment corresponds to the guarantee level; it is assumed (approximated) to be linearly increasing in the number of minor children and, in the case of a woman co-residing with her parents, linearly declining in parents' income, y_a^z. The second line segment is negatively sloped as a function of the woman's own earnings, y_a^o, plus parents' income if she is co-resident, and also linearly increasing in the number of minor children. The negative slopes reflect the benefit reduction (or tax) applied to income.

In general, benefits are equal to the guarantee level (for given numbers of children and parents' income if co-resident) up to a positive level of the woman's earnings (the two line segments intersect at positive earnings) in order to provide a child care allowance for working mothers. Denoting this (state-specific) level of earnings, the disregard, as $y_{at}^{s1}(N_a^{18})$ and the level of earnings at which benefits become zero (where the second line segment intersects the x-axis) as $y_{at}^{s2}(N_a^{18})$, the benefit schedule for a woman with $N_a^{18}>0$ children is given by

$$(5) \quad \begin{aligned} b_t^s(N_{at}^{18}, y_{at}^o, y_{at}^z) &= b_{0t}^s + b_{1t}^s N_{at}^{18} - b_{3t}^s \beta_2 y_{at}^z z_{at} && \text{for } y_{at}^o < y_{at}^{s1}(N_a^{18}), \\ &= b_{2t}^s + b_{4t}^s N_{at}^{18} - b_{3t}^s[(y_{at}^o - y_{at}^{s1}) + \beta_2 y_{at}^z z_{at}] && \text{for } y_{at}^{s1}(N_a^{18}) < y_{at}^o < y_{at}^{s2}(N_a^{18}) \\ &= 0 && \text{otherwise.} \end{aligned}$$

We refer to $b_t^s(N_{at}^{18}, y_{at}^o, y_{at}^z)$ as the benefit rule and to the b_{kt}^s's as the benefit rule parameters. We exclude β_2 from this set for reasons that will become clear.

The benefit rule parameters, and thus benefits themselves, change over time. Therefore, if women are at all forward-looking, they will incorporate their forecasts of the future values of the benefit rule parameters into their decision rules. We assume that benefit rule parameters evolve according to the following general vector autoregression (VAR) and that women use the VAR to form their forecasts of future benefit rules:

$$(6) \quad \mathbf{b}_t^s = \boldsymbol{\lambda}^s + \boldsymbol{\Lambda}^s \mathbf{b}_{t-1}^s + \mathbf{u}_t^s$$

where $\mathbf{b}_t^s$ and $\mathbf{b}_{t-1}^s$ are 5×1 column vectors of the benefit rule parameters, $\boldsymbol{\lambda}^s$ is a 5×1 column vector of regression constants, $\boldsymbol{\Lambda}^s$ is a 5×5 matrix of autoregressive parameters and $\mathbf{u}_t^s$ is a 5×1 column vector of iid innovations drawn from a stationary distribution with variance-covariance matrix $\boldsymbol{\Xi}^s$. We call (6) the evolutionary rule (ER) and $\boldsymbol{\lambda}^s$, $\boldsymbol{\Lambda}^s$, $\boldsymbol{\Xi}^s$ the parameters of the ER. Evolutionary rules are specific to the woman's state of residence.[15]

The woman is assumed to maximize her expected present discounted value of remaining lifetime utility at each age. The maximized value (the value function) is given by

$$(7) \quad V_a(\Omega_a) = \max\ E\ [\sum_{\tau=a}^{62} \delta^{\tau-a}\ U_\tau(\cdot)\,|\,\Omega_a]\,,$$

where the expectation is taken over the distribution of future preference shocks, labor market, marriage and parental co-residence opportunities, and the distribution of the future innovations of the benefit ER. The decision period is six months until age 45, the assumed age at which the women becomes infecund, but one year thereafter.[16] In (7), the state space Ω_a denotes the

[15] As noted, it is assumed that a woman remains in the same location from age 14 on. Clearly, introducing the possibility of moving among states in a forward-looking model such as this would greatly complicate the decision problem.

[16] Allowing for a longer decision period at ages past 45 reduces the computational burden of the model (see Wolpin (1992)).

relevant factors known at age a that affect current or future utility or that affect the distributions of the future shocks and opportunities.

The solution to the optimization problem is a set of age-specific decision rules that relate the optimal choice at any age, from among the feasible choices, to the elements of the state space at that age. Recasting the problem in a dynamic programming framework, the value function, $V_a(\Omega_a)$, can be written as the maximum over alternative-specific value functions, denoted as $V_a^j(\Omega_a)$, i.e., the expected discounted value of choice $j \in J$, that satisfy the Bellman equation, namely

$$V_a(\Omega_a) = \max_{j \in J}[V_a^J(\Omega_a)]$$

$$(8) \quad V_a^j(\Omega_a) = U_a^j + \delta E(V_{a+1}(\Omega_{a+1}) | j \in J, \Omega_a) \text{ for } a < A,$$

$$= U_A^j \qquad \text{for } a = A.$$

A woman at each age a chooses the option with the greatest expected present discounted value of lifetime utility.

The solution of the optimization problem is in general not analytic. In solving the model numerically, one can regard its solution as consisting of the values of $EV_{a+1}(\Omega_{a+1} | j \in J, \Omega_a)$ for all j and elements of Ω_a. We refer to this function as Emax for convenience. As seen in (8), treating these functions as known scalars for each value of the state space transforms the dynamic optimization problem into the more familiar static multinomial choice structure. The solution method proceeds by backwards recursion beginning with the last decision period.[17]

III. Data

The 1979 youth cohort of the National Longitudinal Surveys of Labor Market Experience

[17] Because the size of the state space is large, we adopt an approximation method to solve for the Emax functions. The Emax functions are calculated at a limited set of state points and their values are used to fit a polynomial approximation in the state variables consisting of linear, quadratic and interaction terms. See Keane and Wolpin (1994, 1997) for details. As a further approximation, we let the Emax functions depend on the expected values of the next period benefit parameters, rather than integrating over the benefit rule shocks.

(NLSY79) contains extensive information about schooling, employment, fertility, marriage, household composition, geographic location and welfare participation for a sample of over 6,000 women who were age 14-21 as of January 1, 1979. In addition to a nationally representative core sample, the NLSY contains oversamples of blacks and Hispanics. We use the annual interviews from 1979 to 1991 for women from the core sample and from the black and Hispanic oversamples.

The NLSY79 collects much of the relevant information, births, marriages and divorces, periods of school attendance, job spells, and welfare receipt, as dated events. We adopt a decision period of six months. Periods are defined on a calendar year basis, beginning either on January 1 or on July 1 of any given year. The first decision period is the first six month calender period that the woman turns age 14. The last period we observe is the second six month calendar period in 1990 (or, if the woman attrited before then, the last six-month period in which the data are available). The first calendar period observation, corresponding to that of the oldest NLSY79 sample members, occurs in the second half of 1971. There are fifteen other birth cohorts who turned age 14 in each six month period through January, 1979.

We restrict the sample to the six states in the U.S. that have the largest representations of NLSY79 respondents: California, Michigan, New York, North Carolina, Ohio and Texas. The estimation is performed using only the first five states. Texas is used as a holdout or validation sample on which to perform out-of-sample validation tests of the model. The reason for this choice is that Texas is by far the least generous state in terms of welfare benefits and thus requires an fairly extreme out-of-sample extrapolation.

As noted, we consider the following choices: whether or not to (i) attend school (ii) work (part- or full-time), (iii) be married, (iv) become pregnant and (v) receive welfare (AFDC). The variables are defined as follows:

School Attendance: The NLSY79 collects a monthly attendance record for each women beginning as of January, 1979. A woman was defined to be attending school if she reported being in school each month between January and April in the first six-month calendar period and each month between October and December in the second calendar period. Given the sample design of the NLSY79, school attendance records that begin at age 14 exist only for the cohort that turned 14 in January, 1979.

Employment Status: Using employment event history data, we calculated the number of hours worked in each six month period. A woman was considered working part-time in the period (500 hours) if she worked between 260 and 779 hours and full-time (1000 hours) if she worked at least 780 hours during the period. As with school attendance, employment data does not extend back to age 14 for many of the cohorts. We assume that initial work experience, that is, at age 14, is zero.

Marital Status: The NLSY79 provides a complete event-dated marital history that is updated each interview. However, dates of separation are not reported. Therefore, for the years between 1979 and 1990, data on household composition was used to determine whether the woman was living with her spouse. But, because these data are collected only at the time of the interview, marital status is treated as missing during periods in which there were no interviews, in most cases for one six-month period per year. Marital event histories were used for the periods prior to 1979 even though it is uncertain from that data whether the spouse was present in the household.

Pregnancy Status: Although pregnancy rosters are collected at each interview, conception dates are noisy and miscarriages and abortions are under-reported. We ignore pregnancies that do not lead to a live birth, dating the month of the conception as occurring nine months prior to the month of birth. Except for misreporting of births, there is no missing information on pregnancies back to age 14 for any of the cohort.

Welfare Receipt: AFDC receipt is reported for each month within the calendar year preceding the interview year, i.e., from January 1978. We define a woman as receiving welfare in a period if she reported receiving an AFDC payment in at least three of the six months of the period.[18] As with school attendance and employment, data are missing back to age 14 for most of the cohorts. It is assumed that none of the women received welfare prior to age 14, as is consistent with the fact that none had borne a child by that time.

[18] The use of almost any cutoff in establishing welfare participation would have only a small effect on the classification; most women who report receiving welfare in any one month during a six month period report receiving it in all six months.

Descriptive Statistics:

Table 1 provides (marginals of) the sample choice distribution by full-year ages and by race aggregated over the five states used in the estimation. As seen, school attendance is essentially universal until age 16, drops about in half at age 18, the normal high school graduation age, and falls to around 10 percent at age 22. About 3 percent of the sample attends school at ages after 25. The implied school completion levels that result from these attendance patterns are, at age 24, 12.9 for whites, 12.7 for blacks and 12.2 for Hispanics. Employment rates for white and Hispanic women (working either part- or full-time) increase rapidly through age 18 and then slowly thereafter, although they are higher for whites throughout by about 10-20 percentage points. Employment rates for black females rise more continuously, roughly doubling between age 18 and 25, and are comparable to that of Hispanics at ages after 25.

Marriage rates rise continuously for whites and Hispanics, reaching about 60 percent by age 25 for whites and 50 percent for Hispanics. However, for blacks, marriage rates more or less reach a plateau at about age 22, at between 20 and 25 percent. With respect to fertility, by age 20, white females in the sample on average had .28 live births, black females .47 live births and Hispanic females .40 live births. Teenage pregnancies that lead to a live birth are higher by 68 percent for blacks than for whites and by 43 percent for Hispanics than for whites. By age 27, the average number of live births are 1.06, 1.36 and 1.39, and by age 30, 1.54, 1.61 and 1.76. Welfare participation increases with age, at least through age 24. Race differences are large; participation peaks at 7 percent for whites, 28 percent for blacks and 17 percent for Hispanics

There are large differences in choices between women who reside in the five states used in estimation (the estimation sample) vs. Texas (the validation sample). The largest differences are for AFDC take-up and for full-time employment. For example, among black women, welfare receipt peaked at about 30 percent in the estimation sample, while it peaked at only about 10 percent in the validation sample. With respect to full-time employment, at age 25, for example, the difference in the proportion engaged in full-time work was 14.3 percentage points for whites, 18.9 percentage points for blacks and 19.6 percentage points for Hispanics.[19]

Benefit Rules:

[19] See Keane and Wolpin (2005) for further details.

In order to estimate the benefit schedules (5) and the evolutionary rules governing changes in benefit parameters (6), we collected information on the rules governing AFDC and Food Stamp eligibility and benefits in each of the 50 states for the period 1967-1990. The parameters of the benefit schedule are obtained by estimating (5) for each state separately in each year using the sum of the monthly benefits from AFDC and Food Stamps, with monthly benefit amounts expressed in 1987 New York equivalent dollars. Thus, for each state, s, we obtained an estimate of the benefit rule parameters, $b^s_{t0}, b^s_{t1}, b^s_{t2}, b^s_{t3}, b^s_{t4}$, for each year t.[20] Given the estimates of the benefit rule parameters, we then estimated (6), the evolutionary rule.

Table 2 transforms the benefit parameters obtained from the estimates of (5) into a more convenient set of benefit measures, namely the total monthly income of non-working women (with zero non-earned income) who have either one or two children and the total monthly income of women with one or two children who have part-time monthly earnings of 500 dollars or full-time earnings of 1000 dollars.[21] Referring to table 2, among the six states, NY, CA and MI are considerably more generous than NC, OH and TX. Among the first group Michigan is the most generous, with average benefits over the 24 years for a woman with one child being 654 (1987 NY) dollars per month, and among the second group Texas is the least generous, with the same average benefits figure only 377 dollars. CA and NY were about equally generous on average (589 and 574 dollars) over the period as were NC and OH (480 and 489 dollars).[22]

As table 2 reveals, there was a steep decline in benefit amounts between the early 1970's and the mid 1980's, and relative constancy thereafter. For example, in Michigan monthly benefits

[20] The approximation given by (5) fits the monthly benefit data quite well, with R-squared statistics for the first line segment mostly above .99 and for the second, mostly about .95. See Keane and Wolpin (2005) for the regression estimates.

[21] See Keane and Wolpin (2005) for summary statistics of the actual parameters themselves.

[22] Benefit reduction rates for AFDC and for Food Stamps are federally set. They differ across states in our approximation due to the fact that AFDC payments terminate at different income levels among the states while food stamp payments are still non-zero and the two programs have different benefit reduction rates. There is thus a kink in the schedule of total welfare payments with income that our approximation smooths over.

fell from 735 dollars for a woman with no earnings and two children in 1975 to 561 dollars in 1985. For the same woman with 500 dollars in monthly earnings, benefits fell from 762 dollars in 1975 to 405 dollars in 1985, and then rose slightly to 484 dollars in 1990.

IV. Estimation Method:

The numerical solution to the agents' maximization problem provides (approximations to) the Emax functions that appear on the right hand side of (8). The alternative-specific value functions, V_t^k for k=1,..,K, are known up to the random preference shocks, the wage offer shock of the woman and the earnings shock of the husband (if the woman receives a marriage offer), the implicit shocks that determine whether a marriage offer is received and whether the woman will reside with her parents if she is not married, and the benefit parameter shocks in the evolutionary rule.

Thus, conditional on the deterministic part of the state space, the probability that an agent is observed to choose option k takes the form of an integral over the region of the several-dimensional error space such that k is the preferred option. The error space depends on which option k is being considered. If option k corresponds to a work option, then the wage offer is observed by us, and the wage shock is not in the subset over which the integration occurs. In that case, the likelihood contribution for the observation also includes the density of the wage error. If the woman is married (living with parents), then the husband's (parents') income is observed by us, that shock is excluded from the integration and the likelihood contribution includes the husband's (parents') income density.

As noted, the choice set contains as many as 36 elements. It is well known that evaluation of choice probabilities is computationally burdensome when the number of alternatives is large. Recently, highly efficient smooth unbiased probability simulators, such as the GHK method (see, e.g., Keane (1993, 1994)), have been developed for these situations. Unfortunately, the GHK method, as well as other smooth unbiased simulators, rely on a structure in which there is a separate additive error associated with each alternative. Further, as discussed in Keane and Moffitt (1998), in estimation problems where the number of choices exceeds the number of error terms, the boundaries of the region of integration needed to evaluate a particular choice probability are generally intractably complex. Thus, given our model, the most practical method to simulate the probabilities of the observed choice set would be to use a kernel smoothed frequency simulator. These were proposed in McFadden (1989), and have been successfully applied to models with large choice sets in Keane and Moffitt (1998) and Keane and Wolpin (1997).[23]

In the present context, however, standard simulated maximum likelihood methods are not feasible because of severe problems created by unobserved state variables. Because, as we have noted, we do not have a complete history of employment, schooling or welfare take-up for most of the cohorts back to age 14, the state variables accounting for work experience, schooling and welfare dependence cannot be constructed. Parental co-residence is also observed only once a year as is marital status that takes into account spousal co-residence.

Further complicating the estimation problem is that the youth's initial schooling level at age 14 is observed only for one of the 16 cohorts. It is well known that unobserved initial conditions (Heckman (1981)), and unobserved state variables more generally, pose formidable computational problems for estimation of dynamic discrete choice models. If some or all elements of the state space are unobserved, then to construct conditional choice probabilities one must integrate over the distribution of the unobserved elements. Even in much simpler dynamic models than ours, such distributions are typically computationally intractabe.

In a previous paper (Keane and Wolpin (2001b)), we have developed a simulation algorithm that deals in a practical way with the problem of unobserved state variables. The algorithm is based on simulating complete (age 14 to the terminal age) outcome histories for a set of artificial agents. An outcome history consists of the initial school level of the youth, S_0, along with simulated values in all subsequent periods for all of the outcome variables in the model (school attendance, part- or full-time work, marriage, pregnancy, welfare participation, the woman's wage offer, the husband's earnings, parents' income). Denote by $\tilde{O}^n$ the simulated outcome history for the nth such person, $\tilde{O}^n = (S_{14}^n, \tilde{O}_{a=1}^n, \ldots, \tilde{O}_{a=A}^n)$, for n = 1,..., N.

In order to motivate the estimation algorithm, it is useful to ignore for now the

[23] Kernel smoothed frequency simulators are, of course, biased for positive values of the smoothing parameter, and consistency requires letting the smoothing parameter approach zero as sample size increases.

complication that some of the outcomes are continuous variables. Let O^i denote the observed outcome history for person i, which may include missing elements. Then, an unbiased frequency simulator of the probability of the observed outcome history for person i, $P(O^i)$, is just the fraction of the N simulated histories that are consistent with O^i. In this construction, missing elements of O^i are counted as consistent with any entry in the corresponding element of $\tilde{O}^n$. Note that the construction of this simulator relies only on unconditional simulations. It does not require evaluation of choice probabilities conditional on state variables. Thus, unobserved state variables do not create a problem for this procedure.

Unfortunately, because the number of possible outcome histories is huge, consistency of a simulated history with an actual history is an extremely low probability event. Hence, simulated probabilities will typically be 0, as will thus be the likelihood, unless an impractically large simulation size is used (see Lerman and Manski 1981). In addition, the method breaks down if any outcome is continuous, e.g., the woman's wage offer, regardless of simulation size, because agreement of observed with simulated wages is a measure zero event.

We solve this problem by assuming, as is apt, that all observed quantities are measured with error. With measurement error there is a nonzero probability that any observed outcome history might be generated by any simulated outcome history. Denote by $P(O^i|\tilde{O}^n)$ the probability that observed outcome history O^i is generated by simulated outcome history $\tilde{O}^n$. Then $P(O^i|\tilde{O}^n)$ is the product of classification error rates on discrete outcomes and measurement error densities for wages that are needed to make O^i and $\tilde{O}^n$ consistent. Observe that $P(O^i|\tilde{O}^n)>0$ for any $\tilde{O}^n$, given suitable choice of error processes. The specific measurement error processes that we assume are described below. The key point here is that $P(O^i|\tilde{O}^n)$ does not depend on the state variables at any age a, but only depends on the outcomes.

Using N simulated outcome histories we obtain the unbiased simulator

$$(11)\quad \hat{P}_N(O^i) = \frac{1}{N}\sum_{n=1}^{N} P(O^i|\tilde{O}^n).$$

Note that this simulator is analogous to a kernel-smoothed frequency simulator, in that $I(O^i = \tilde{O}^n)$ is replaced with an object that is strictly positive, but that is greater if $\tilde{O}^n$ is "closer" to O^i. However, the simulator in (11) is unbiased because the measurement error is assumed to be present in the true model.

It is straightforward to extend the estimation method to allow for unobserved heterogeneity. Assume that there are K types of women who differ in their permanent preferences for leisure, school, marriage, becoming pregnant and receiving welfare. In addition, women also differ in their human capital "endowment" at age 14 and in their potential husband's human capital stock. To handle unobserved heterogeneity (i.e. types) in this framework, define $\pi_{k|S_{14}}$ as the probability a person is type k given his initial school level, for k = 1,..., K, where K is the number of types. In this case, simulate N/K vectors $\tilde{O}^n_k$ for each type.[24] Then,

$$(12)\quad \hat{P}_N(O^i) = \frac{1}{N}\sum_{k=1}^{K}\sum_{n=1}^{N/K} P(O^i|\tilde{O}^n)\frac{\pi_{k|S_{14}}}{N/K}.$$

Observe that in (12), the conditional probabilities $P(O^i|\tilde{O}^n)$ are weighted by the ratio of the proportion of type k according to the model, $\pi_{k|S_{14}}$, to the proportion of type k in the simulator, N/K.

The simulator in (12) is not smooth because $P(O^i|\tilde{O}^n)$ will "jump" at points where a change in the model parameters causes the simulated outcome history $\tilde{O}^n$ to change discretely. However, this simulator can be made smooth in the model parameters if an importance sampling procedure is applied, with the simulated outcome histories are held fixed and re-weighted as parameters are varied. Given an initial parameter vector θ_0 and an updated vector θ', the appropriate weight to apply to sequence $\tilde{O}^n$ is the ratio of the likelihood of simulated history n under θ' to that under θ_0. Such weights have the form of importance sampling weights (i.e., the ratios of densities under the target and source distributions), and are smooth functions of the model parameters. Further, it is straightforward to simulate the likelihood of an artificial history $\tilde{O}^n$ using conventional methods because the state vector is fully observed at all points along the history. The choice probabilities along a path $\tilde{O}^n$ are simulated using a kernel smoothed frequence simulator. Given that $P(O^i|\tilde{O}^n)$ is now a smooth function of the model parameters,

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[24] Initial schooling is assumed exogenous conditional on type. We also take the parents' schooling as an initial condition exogenous conditional on type.

standard errors can be obtained using the BHHH algorithm.

With respect to the measurement error processes, we assume discrete outcomes are subject to classification error. The structure we adopt is simply that there is some probability that the reported response category is the truth and some probability that it is not.[25] For the continuous variables, we assume that the woman's wage offer error and the husband's income error are multiplicative and the parents' income error is additive. Measurement errors are assumed to be serially independent and independent of each other.

V. Results

To provide a comparison for assessing the fit of the dynamic programming (DP) model, we have also estimated a multinomial logit (MNL) that relates four of the five choice variables, welfare take-up, school attendance, work and pregnancy, to the state variables of the model at each age. We estimated four different specifications of the MNL, but present the results for now of only the one that best fit the estimation and validation samples.[26] The variables included are the benefit amount for a woman with one child and no earnings, state dummies, age and age squared, parents schooling, whether the woman was on welfare, worked or was pregnant in the previous period, whether the woman was pregnant two periods before, the number of children already born to the woman, the woman's years of schooling and its square, whether the woman was living in a nuclear family at age 14, and race dummies. There are 13 mutually exclusive choices (3 were combined because of small cell size) and 240 parameters. In comparison, the DP model is more comprehensive, including also a marriage decision and distinguishing between working full or part time, and also embedding additional structural relationships (functions describing the probability of living with a parent, husband's income if married and parent's income if co-resident, and full and part-time wage offers). Nevertheless, that DP model has a similar number of parameters (245).

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[25]To ensure that the measurement error is unbiased, the probability that the reported value is the true value must be a linear function of the predicted sample proportion (see Keane and Wolpin (2005) for details). Keane and Sauer (2005) have applied this algorithm successfully with more general classification error processes

[26] These MNL estimates are available on request.

Table 3 shows the fit to the estimation sample for the MNL and the DP models by four age groups (15-17.5, 18-21.5, 22-25.5, 26-29.5) for each race separately. Although there are clear differences in the fit of the two models, neither seems to be uniformly better. For example, the MNL fits welfare take-up better for blacks than does the DP model, but fits Hispanics worse and whites about the same. Similarly, the MNL model seems to fit the work alternative better for Hispanics at earlier ages, but the DP model fits better at later ages. Both models capture well age trends and quantitative differences by race. The table also compares the fit to two of the state variables, the mean number of children ever born before ages 20, 24 and 28, and the mean highest grade completed by age 24. The performance is similar with respect to these measures, except for the severe overstatement of schooling for Hispanics by the MNL model.

Table 4 presents the same comparison for the validation sample. The MNL clearly does better than the DP model in terms of welfare take-up, especially for blacks in the last two age groups. However, other differences seem to be small. As with the estimation sample, age trends and racial differences are captured well. Neither model is very far off in forecasting children ever born or schooling.[27]

To provide a summary of the overall fit to the estimation and validation samples, table 5 reports the root mean squared error (RMSE), calculated from the deviations between actual and forecasted age-specific means, for the four MNL models that were estimated and for the DP model. Starting from the MNL model described previously, denoted by MNL1 - FE in the table, where FE indicates the inclusion of state dummies, the other models were: (i) same as the base model without state fixed effects and including the mean one-child benefit for the state over the period 1967-1990, denoted as MNL1 - No FE; (ii) same as the base model except that the five state-specific benefit parameters were included in the specification separately, denoted as MNL2

[27] We also considered the fit of the DP model to all of the other variables for both the estimation sample and the validation sample (see Keane and Wolpin (2005)). The fit with respect to the estimation sample is uniformly good, capturing well age trends and racial differences. In some cases, the fit is remarkably close. For example, because of selection, fitting accepted wages when working percentages are low is challenging. Nevertheless, the DP model predictions are quite close to the actual data. For example, predicted mean accepted wage rates are often within 5 percent of the actual wage rates.

- FE ; (iii) same as MNL2-FE except that there are no state dummies and the means of the five benefit parameters over the 1967-1990 period are included, denoted by MNL2 - No FE.

With respect to the estimation sample, all of the MNL models appear about equally as good. In terms of RMSE, the DP model is also about as good. Notable exceptions are the better fit of the DP model to school attendance among whites (.027 vs. .044 for MNL1- No FE), the worse fit of the DP model to work (.064 vs. .030 for MNL- No FE) and to pregnancy (.021 vs. .015 for MNL1 - FE and No FE) for blacks, and the better fit of the DP model to welfare (.024 vs. .044 for MNL1 - FE) and to work (.048 vs. .059 for MNL2 - FE) for Hispanics.

Large differences in fit emerge for the validation sample.[28] Among the MNL models, the two that include state dummies (MNL1 - FE and MNL - FE) have the lowest root mean squared errors. Although adding the additional benefit parameters provides a statistically significant improvement in the estimation-sample fit, there is no discernible impact on the root mean squared error for the validation sample.[29]

Using the mean one-child benefit instead of the state dummies (MNL1 - No FE vs. MNL1- FE), does negatively affect the RMSE; for example, the largest changes are from .068 to .093 for work and from .046 to .086 for school attendance for whites, from .021 to .030 or welfare for blacks, and from .050 to .062 for work and from .059 to .034 for school attendance for blacks.

But, the differences are much greater for the MNL2 models. Dropping the state dummies, and instead including the five state-specific mean benefit parameters, increased the RMSE enormously. The fit to welfare was particularly adversely affected, rising from .010 (MNL2 - FE) to .815 (MNL2 - No FE) for whites, from .021 to .844 for blacks and from .014 to .842 for Hispanics. Essentially, the MNL - No FE specification predicted very high take-up rates in Texas (see below), presumably the opposite of what one would expect given the considerably less

[28] To forecast Texas for the MNL models with state dummies, we re-estimated the model on Texas data with a Texas state dummy, constraining all other parameters to be the same as in the estimation sample.

[29] The chi-square statistic for the joint test that all of the additional benefit parameters are zero has a p-value of .000.

generous welfare benefits in Texas. Recall that in specifications that included only the one-child benefit (MNL1), instead of the five benefit rule parameters (MNL2), dropping the state fixed-effects did not lead to such a serious deterioration of the fit to Texas. We take this result as evidence that the validation sample is capable of identifying over-fitting in a way that the within-sample significance test was not.

The DP model uniformly does not fit as well as MNL1 - FE and overall fits slightly worse than MNL1 - No FE, although in isolated instances it does fit better. Based on the evidence from this validation exercise, it would therefore appear that MNL1 - FE would be the best model to use for counterfactual experiments.

Table 6 reports on the results from a counterfactual experiment where the estimation sample states are given Texas' welfare benefits. We report on the effects for both MNL1 specifications and for the DP model. The predicted effects from the MNL1 - FE specification are seemingly perverse. Welfare take-up and fertility are predicted to increase substantially, while there is a similarly large decline in work. The predictions from the MNL1- No FE specification are exactly the opposite, a large reduction in welfare take-up, a large increase in work and a relatively small reduction in fertility.

Keane and Wolpin (2001a) noted an important distinction between specifications with and without state-specific effects. If women are forward looking, the effect of a change in welfare benefit rules on behavior depends critically on how that change affects expectations about future benefit rules. Changes in welfare benefits can have very different effects depending on whether they are perceived as being permanent or transitory. Estimates that use different sources of variation in benefits, variation across states versus variation within states over time, may result in different estimates simply because they identify responses to benefit changes that may be perceived as having different degrees of permanence. For example, if benefits are change from year-to-year, the effect of a change in the current year's benefits on fertility will depend on the degree to which the change is viewed as permanent. This, in turn, depends on the process by which benefits evolve and how potential welfare recipients form expectations. If the perceived benefit process is such that an increase in benefits in one year is anticipated to be followed by declines in subsequent years, then it is possible that fertility may actually respond negatively to the transitory increase. Thus, the counterfactual using MNL1 - FE is not, under this interpretation, identifying the effect of replacing the estimation sample states' welfare systems with Texas' system. Nevertheless, it does not seem plausible that this explanation alone could lead to the very large increases in welfare participation seen in Table 6.[30]

On the other hand, MNL1 - No FE replaces not only benefit realizations but also the mean, and thus, the permanent level of benefits as well. However, the effects predicted by MNL1 - No FE appear to be implausible as well. For example, while welfare participation among whites falls by 3.8 percentage points (from 4.5 to 0.7 percent) at ages 26-29.5, employment increases by 12.2 percentage points. Indeed, for all three race groups, the reduction in welfare participation is usually considerably less than the increase in employment at all ages. The prediction that employment rates would reach close to 90 percent with the adoption of Texas' welfare benefits is not credible. In addition, perhaps even less credibly, the reduction in benefits leads to a fall, rather than an increase, in schooling.

The counterfactual based on the DP model, which accounts for the entire set of welfare parameters, replaces each of the estimation sample state's benefit realizations as well as its evolutionary rule (as in (6)) with that of Texas' realizations and rule. The resulting effects are more modest than in the MNL1 - No FE specification. The largest effects are for Hispanics, where welfare participation falls by as much as 5 percentage points (from 15.3 to 10.2 percent) at ages 22-22.5 and employment increases by 3 percentage points at those ages. For all races, within each age group, the increase in employment is no larger than the fall in welfare participation. In addition, for each race, mean schooling by age 25 increases, though very slightly. Overall, the results from the DP model appear more reasonable than the MNL - No FE

[30] Another factor may be that the over-time variation in benefits, on which the fixed effects models rely, is correlated with other factors that drive welfare caseloads. For example, exogenous increases in caseloads, say due to demographic shifts, might lead states to reduce benefits. This could induce a short run negative correlation between caseloads and benefits, leading the fixed-effect model to produce the "wrong sign" on benefits. Models without fixed-effects, since they rely more on permanent cross-state variation in benefit levels to identify benefit effects, would be less sensitive to this problem.

specification.[31]

VI. Conclusions:

In this paper, we have presented and structurally estimated a dynamic programming (DP) model of life-cycle decisions of young women. The model significantly extends earlier work on female labor supply, fertility, marriage, education and welfare participation by treating all five of these important decisions as being made jointly and sequentially within a life-cycle framework. Needless to say, the resulting model is quite complex, and many behavioral and statistical assumptions were needed to make its solution and estimation feasible. Of course, the model is literally false, as our assumptions are designed to abstract from and simplify the full complexity of how people really make life-cycle decisions. Thus, the model is simultaneously both mathematically complex, yet highly stylized as a depiction of actual behavior. Nevertheless, we believe that such models, tightly specified on the basis of very specific theoretical and statistical assumptions, are potentially quite useful for policy analysis. The issue is how to develop faith, or validate, that such a model is indeed useful.

Classical statistical procedures offer limited guidance on how to proceed with validation. Because the model is literally not true, classical specification tests which take as the null hypothesis that the model is the true data generating process will reject the model for a large enough sample size. However, this does not mean that the model is not "useful" in the sense of providing reasonably accurate predictions about the effect of interesting potential policy interventions, or at least predictions that are better than existing models. Analogously, engineering models of mechanical and physical systems are also literally false, but they have proved very useful in predicting how the behavior of such systems would be affected by design changes. But how can we learn whether a model does indeed provide accurate predictions?

One option is to wait for the real world to produce policy interventions (or to produce them ourselves through social experiments), and then check the accuracy of the model's predictions of the impact of the intervention. The problem with this approach is that policy

[31] Effects of the counterfactual experiment for the DP model on additional variables, considered in Keane and Wolpin (2005), are predicted to be quite small. For example, by ages 26-29.5, the marriage rate is predicted to increase from 65.6 to 65.9 percent for whites, from 28.2 to 28.8 percent for blacks and from 55.7 to 56.9 percent for Hispanics.

interventions of this kind don't come along very often and social experimentation is costly. This is presumably (at least in part) why the economics literature contains so few examples where actual or manufactured policy changes have been used to help validate models.

An alternative is to pursue a range of approaches to model validation as we have done in this paper. First, we have examined the fit of our model to the in-sample data that was used in estimation across a range of dimensions of interest. In that context, we also have compared the fit of the DP model to a group of "flexible" models, specified as multinomial logits, for a subset of the choice data that our model describes. Using a RMSE criterion (the number of parameters are similar), there seems to be no clear winner in this cross-model competition. Based on these results, our view is that the DP model fits the in-sample data reasonably well (i.e., after seeing the fit, we continued to view the model as potentially useful for prediction).

Second, as we have emphasized, we have used, as a non-random holdout sample, data from the state of Texas, which had a very different welfare policy regime from the five states that were used in estimation. Based on our own subjective standards, the DP model predicts behavior in Texas acceptably well, as do three of the four MNL models we consider. But one of the models (MNL2-No FE) produced predictions for Texas that are terribly inaccurate by any standard, leaving us with no faith in its usefulness. In terms of the RMSE criterion, the model we called MNL1-FE fits the data from Texas a bit better than the DP model, but, based on this evidence, we continued to view our model and the three remaining MNL models as potentially useful for policy analysis.

Our third method of validation was to use the models to predict the effect of a policy intervention that has no analogue in the historical data, but where we have fairly tight priors, in a qualitative sense, on certain aspects of what might possibly happen. The counterfactual experiment was to give the five estimation states the same welfare rules as Texas. Our strong priors were: (i) that welfare participation should drop, since the Texas benefits are less generous, (ii) that work should increase, but that the decline in welfare places a reasonable upper bound on the increase in work, and (iii) that education should not decrease (since human capital becomes more valuable in an environment with less generous income support). Surprisingly, given their acceptable performance in terms of in-sample fit and prediction for the hold-out sample Texas, all three "surviving" MNL models severely violated one of more of these strong priors. Thus, we came to view all four MNL models as unreliable for policy prediction. In contrast, the predictions of the DP model were consistent with our priors.

In summary, the DP model has, in our judgement, performed well on three different tests of validity. In light of this evidence, we have updated our priors about the potential usefulness of the model (for policy prediction) in a favorable direction. Our research strategy is to continue to look for opportunities to further validate the model, and as these opportunities arise they will either increase or reduce our confidence in the model's usefulness.

One opportunity is presented by the important changes in welfare rules that occurred beginning in the mid-1990s, after our sample period ended. This included EITC expansion, imposition of work requirements for receipt of benefits, and benefit receipt time limits. As discussed in Fang and Keane (2004), there was substantially heterogeneity across states in terms of how exactly these policy changes were structured, and we can use our model to simulate the impact of these changes on a state-by-state basis.

As a final observation, we conjecture that most economists would have professed a greater *a priori* faith in the ability of the MNL models to forecast behavior than in the DP model. That is, they would be concerned that, because the many assumptions invoked in setting up the DP model could all be questioned, it is unlikely such a model could forecast accurately. In contrast, they would view the MNL models, which simply model the value of each alternative as a flexible function of the state variables, as being much less "restrictive." Thus, the poor predictions that the MNL models produce for the counterfactual of giving other states the Texas benefit rules should serve as a cautionary tale, from which we draw two morals.

First, economists should be concerned with model validation regardless of the estimation approach; one needs to hold all models to the same standard. Second, our experience illustrates well the potential strengths of DP models for making policy predictions. It is precisely the economic structure of the model that constrains it to make predictions that are reasonable in certain dimensions. The MNL models' failure is, at least in part, attributable to the fact that they lack sufficient economic structure to impose such reasonable constraints on their predictions. Economics is indeed valuable in econometrics.

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Table 1
Choice Distributions by Age: Estimation Sample of the Combined Five States

	Attending School			Working (PT or FT)			Married			Becomes Pregnant			Receives AFDC		
Age	W	B	H	W	B	H	W	B	H	W	B	H	W	B	H
14	100	93.3	100	14.3	10.5	12.5	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
15	97.7	100	100	11.4	9.9	5.2	0.0	0.0	0.0	1.0	3.4	1.0	1.0	1.3	0.0
16	88.3	87.5	90.3	30.0	14.5	19.3	3.0	1.0	2.9	3.1	3.8	2.1	1.0	1.0	1.0
17	84.6	80.7	79.2	50.0	26.9	32.4	8.7	1.4	6.4	5.6	5.3	2.5	1.3	2.5	2.3
18	42.8	50.9	41.5	63.0	32.6	50.7	16.4	3.7	11.9	3.7	4.5	6.7	2.6	9.0	3.3
19	32.5	32.1	27.1	65.6	43.4	51.2	24.9	7.1	19.9	4.5	8.6	5.6	3.6	15.6	6.8
20	23.8	22.2	18.8	67.5	46.4	52.2	31.5	11.7	27.1	4.3	6.0	4.9	5.4	17.3	10.3
21	19.4	12.3	12.2	69.6	49.2	58.3	37.1	14.4	34.2	6.0	7.9	6.3	5.1	21.2	13.7
22	10.8	8.3	7.7	70.0	52.5	60.6	37.5	20.3	35.9	4.5	5.3	5.7	6.1	25.6	15.1
23	4.2	6.2	3.9	72.0	54.2	58.5	49.1	22.3	39.7	5.9	6.1	5.3	6.2	27.2	15.3
24	3.8	5.4	4.6	72.7	55.4	57.7	54.1	22.8	45.7	6.6	6.9	7.9	7.0	27.8	17.2
25	4.0	5.9	2.9	73.8	62.8	55.6	58.5	20.9	47.2	7.6	7.0	7.2	6.4	26.8	16.0
26-29	3.2	3.6	2.2	71.5	61.1	56.7	63.6	25.6	52.1	5.8	4.4	5.8	5.0	25.7	15.4
30-33	4.5	2.3	2.6	72.6	63.3	64.9	72.8	32.0	56.7	4.3	2.3	5.3	2.6	22.3	14.5

Table 2
Summary Statistics of Total Monthly Benefits By Numbers of Children and Earnings by State: 1967-1990

	Monthly Earnings					
	Zero		$500		$1000	
	One child	Two children	One child	Two children	One child	Two children
CA						
μ	589	724	351	517	87	196
σ	60	67	85	91	89	151
1970	459	568	416	560	297	440
1975	652	794	441	620	132	311
1980	617	757	405	560	156	311
1985	596	730	260	414	0	46
1990	594	728	303	476	0	110
MI						
μ	654	809	429	621	150	304
σ	92	106	161	179	158	215
1970	671	830	585	799	302	516
1975	735	912	551	762	273	483
1980	660	808	424	602	152	330
1985	561	705	235	405	0	58
1990	551	694	293	484	0	156
NY						
μ	574	718	334	514	92	204
σ	52	71	126	152	98	189
1970	562	726	469	685	189	406
1975	635	798	443	643	172	372
1980	552	679	322	473	61	211
1985	524	644	189	334	0	0
1990	528	649	230	393	0	31

Table 2, continued

NC						
μ	480	566	274	384	35	132
σ	48	58	68	82	40	66
1970	455	513	348	432	143	227
1975	570	679	356	502	50	197
1980	462	553	260	364	31	134
1985	454	543	199	295	0	69
1990	438	530	249	367	13	131
OH						
μ	489	607	270	414	87	128
σ	34	43	69	88	36	87
1970	460	565	361	511	106	256
1975	552	688	339	514	27	202
1980	499	619	284	423	11	151
1985	459	570	185	305	0	0
1990	455	566	218	346	0	0
TX						
μ	377	476	217	329	69	106
σ	50	60	51	73	21	43
1970	417	514	297	429	169	201
1975	445	561	253	398	0	117
1980	334	436	198	295	0	96
1985	375	474	170	264	0	52
1990	343	442	181	287	0	101

Table 3 - Actual and Predicted Choice Probabilities by Age for the Estimation Sample: MNL and DP Models

	White			Black			Hispanic		
	Actual	MNL	DP	Actual	MNL	DP	Actual	MNL	DP
Percent Receiving Welfare									
Age 15-17.5	0.9	0.5	1.3	1.9	2.3	4.8	1.3	0.6	4.4
Age 18-21.5	4.3	3.4	4.7	16.9	16.6	15.5	9.2	5.4	11.2
Age 22-25.5	6.4	5.0	7.1	26.9	23.9	24.6	15.0	10.3	15.1
Age 26-29.5	4.7	4.5	7.1	21.6	21.6	28.0	15.2	10.2	15.8
Percent in School									
Age 15-17.5	86.4	81.4	85.3	86.3	82.0	84.2	84.6	84.2	79.2
Age 18-21.5	27.3	28.9	29.8	26.1	25.2	29.5	22.0	29.2	21.4
Age 22-25.5	5.2	5.4	8.3	6.3	6.3	8.0	5.0	5.2	6.0
Age 26-29.5	3.1	2.2	3.5	3.5	2.5	3.5	2.0	2.1	2.8
Percent Working									
Age 15-17.5	35.2	29.7	28.4	19.2	17.6	18.3	22.2	20.1	26.6
Age 18-21.5	66.7	66.3	63.8	44.1	47.9	53.8	52.8	53.0	58.5
Age 22-25.5	72.4	74.9	70.4	56.8	56.0	59.4	58.7	62.2	57.8
Age 26-29.5	71.1	78.7	69.8	61.1	62.1	57.7	56.1	66.8	55.4
Percent Pregnant									
Age 15-17.5	2.5	2.1	1.9	4.6	2.9	3.0	3.2	3.8	3.2
Age 18-21.5	4.4	5.3	4.7	6.7	5.9	6.5	6.9	7.0	6.5
Age 22-25.5	5.5	6.0	5.1	5.8	6.2	7.3	6.7	7.1	7.7
Age 26-29.5	5.5	5.1	4.8	4.2	5.0	6.6	5.9	5.9	6.6
Children Born Before									
Age 20	0.32	0.32	0.31	0.53	0.39	0.47	0.40	0.43	0.48
Age 24	0.72	0.81	0.72	1.05	0.90	1.02	1.00	1.00	1.03
Age 28	1.26	1.24	1.13	1.41	1.20	1.62	1.60	1.49	1.61
Highest Grade Completed									
By Age 24	12.87	13.03	13.08	12.68	12.90	12.97	12.20	12.83	12.38

Table 4 - Actual and Predicted Choice Probabilities for Validation Sample by Age: MNL and DP Models

	White			Black			Hispanic		
	Actual	MNL	DP	Actual	MNL	DP	Actual	MNL	DP
Percent Receiving Welfare									
Age 15-17.5	0.0	0.1	0.1	0.6	0.8	1.3	1.3	0.4	0.5
Age 18-21.5	0.0	0.3	0.7	7.3	7.3	6.4	4.2	3.8	2.3
Age 22-25.5	0.8	0.5	1.6	7.8	9.1	13.0	5.0	4.8	4.9
Age 26-29.5	0.7	0.3	1.9	7.3	8.5	17.7	4.7	4.6	5.9
Percent in School									
Age 15-17.5	93.6	88.5	87.0	87.8	82.0	85.4	80.3	81.0	82.0
Age 18-21.5	36.5	38.4	31.1	27.9	25.2	29.1	29.8	31.4	22.5
Age 22-25.5	6.9	7.7	9.4	3.5	6.3	8.5	4.4	5.7	6.5
Age 26-29.5	4.4	3.7	4.0	1.9	2.5	3.8	4.5	3.4	3.0
Percent Working									
Age 15-17.5	39.3	37.3	38.2	24.7	18.6	24.2	24.1	21.6	33.3
Age 18-21.5	68.9	72.8	75.8	60.5	57.4	64.9	55.0	54.4	64.1
Age 22-25.5	80.0	84.2	82.0	73.1	71.5	70.7	68.1	68.5	64.5
Age 26-29.5	79.6	83.5	82.5	72.8	72.3	69.1	64.9	69.5	63.9
Percent Pregnant									
Age 15-17.5	1.3	2.1	1.7	4.5	2.1	2.9	3.8	4.2	3.3
Age 18-21.5	3.7	5.3	4.8	6.9	4.9	6.7	6.7	6.6	7.1
Age 22-25.5	4.5	6.0	4.9	5.8	5.0	7.4	6.4	6.2	7.5
Age 26-29.5	4.2	5.1	4.8	3.5	3.9	6.6	4.9	5.2	7.0
Children Born Before									
Age 20	0.22	0.18	0.29	0.65	0.58	0.46	0.50	0.50	0.52
Age 24	0.49	0.56	0.68	1.12	0.99	1.03	1.06	1.06	1.11
Age 28	0.86	0.92	1.09	1.71	1.45	1.63	1.54	1.54	1.72
Highest Grade Completed By Age 24	13.27	13.47	13.24	12.81	12.71	13.02	12.21	12.41	12.49

Table 5 - Root Mean Squared Error for Alternative MNL Specifications and for DP Model : Selected Choice Variables

	Estimation Sample					Validation Sample				
	MNL1 FE	MNL1 No FE	MNL2 FE	MNL2 No FE	DP	MNL1 FE	MNL1 No FE	MNL2 FE	MNL2 No FE	DP
Whites										
Welfare	.011	.012	.012	.011	.015	.010	.010	.010	.815	.012
(Mean)			(.043)					(.004)		
Work	.054	.051	.049	.048	.046	.068	.093	.068	.255	.077
(Mean)			(.631)					(.688)		
Pregnancy	.012	.012	.013	.012	.012	.019	.022	.019	.442	.021
(Mean)			(.046)					(.036)		
In School	.045	.044	.045	.047	.027	.046	.086	.045	.138	.054
(Mean)			(.268)					(.315)		
Blacks										
Welfare	.030	.028	.027	.026	.026	.021	.030	.021	.844	.063
(Mean)			(.189)					(.061)		
Work	.035	.030	.034	.032	.064	.059	.054	.058	.215	.065
(Mean)			(.470)					(.600)		
Pregnancy	.015	.015	.016	.016	.021	.034	.037	.033	.490	.036
(Mean)			(.054)					(.052)		
In School	.031	.031	.028	.032	.034	.044	.047	.046	.224	.048
(Mean)			(.269)					(.264)		
Hispanics										
Welfare	.044	.052	.049	.050	.024	.014	.018	.014	.842	.019
(Mean)			(.108)					(.040)		
Work	.067	.071	.059	.064	.048	.050	.062	.048	.169	.092
(Mean)			(.491)					(.550)		
Pregnancy	.015	.015	.015	.015	.019	.022	.025	.022	.487	.030
(Mean)			(.059)					(.056)		
In School	.050	.048	.049	.050	.047	.034	.059	.034	.177	.058
(Mean)			(.246)					(.264)		

Table 6- Counterfactual of Other States with Texas Welfare Benefits: MNL and DP Comparison

	Actual	MNL1 FE Baseline	MNL1 FE With Texas	MNL1 No FE Baseline	MNL1 No FE With Texas	DP Baseline	DP With Texas
Whites							
Percent Receiving Welfare							
Age 15-17.5	0.9	0.5	3.0	0.6	0.2	1.3	0.4
Age 18-21.5	4.3	3.4	19.4	3.6	1.1	4.7	3.0
Age 22-25.5	6.4	5.0	25.9	4.8	1.1	7.1	5.5
Age 26-29.5	4.7	4.5	17.1	4.5	0.7	7.1	5.8
Percent In School							
Age 15-17.5	86.4	81.4	82.6	80.4	78.2	85.3	85.4
Age 18-21.5	27.3	28.9	26.5	27.7	21.1	29.8	29.9
Age 22-25.5	5.2	5.4	4.6	5.3	2.8	8.3	8.3
Age 26-29.5	3.1	2.2	2.0	2.4	1.3	3.5	3.5
Percent Working							
Age 15-17.5	35.2	29.7	15.6	29.8	32.9	28.4	27.8
Age 18-21.5	66.7	66.3	37.0	66.5	77.6	63.8	64.1
Age 22-25.5	72.4	74.9	40.4	74.5	87.1	70.4	71.8
Age 26-29.5	71.1	78.7	48.7	77.9	90.1	69.8	71.1
Percent Pregnant							
Age 15-17.5	2.5	2.1	3.6	2.2	1.5	1.9	1.9
Age 18-21.5	4.4	5.3	15.4	5.4	4.7	4.7	4.8
Age 22-25.5	5.5	6.0	16.9	6.0	5.2	5.1	5.1
Age 26-29.5	5.5	5.1	10.8	5.1	4.6	4.8	4.8
Children Ever Born Before							
Age 20	0.32	0.32	0.67	0.34	0.30	0.31	0.31
Age 24	0.72	0.81	1.85	0.82	0.74	0.72	0.71
Age 28	1.26	1.25	2.76	1.27	1.14	1.13	1.13
Highest Grade Completed							
By Age 25	12.87	13.03	12.93	12.97	12.68	13.08	13.09

Table 6, continued

	Actual	MNL1 FE Baseline	MNL1 FE With Texas	MNL1 No FE Baseline	MNL1 No FE With Texas	DP Baseline	DP With Texas
			Blacks				
Percent Receiving Welfare							
Age 15-17.5	1.9	2.3	7.3	2.5	1.1	4.8	3.1
Age 18-21.5	16.9	16.6	42.3	17.5	8.2	15.5	12.2
Age 22-25.5	26.9	23.9	57.9	24.9	9.6	24.6	20.4
Age 26-29.5	21.6	21.6	53.0	22.1	7.1	28.0	24.3
Percent In School							
Age 15-17.5	86.3	82.0	78.8	81.6	80.6	84.2	84.6
Age 18-21.5	26.1	25.2	18.0	25.7	19.5	29.5	29.9
Age 22-25.5	6.3	6.3	3.0	6.6	3.0	8.0	8.2
Age 26-29.5	3.5	2.5	1.0	2.7	1.3	3.5	3.6
Percent Working							
Age 15-17.5	19.2	17.6	9.6	17.6	20.1	18.3	18.1
Age 18-21.5	44.1	47.9	22.5	46.4	62.3	53.8	54.9
Age 22-25.5	56.8	56.0	23.3	55.3	75.1	59.4	62.9
Age 26-29.5	61.1	62.1	27.2	61.6	80.7	57.7	61.6
Percent Pregnant							
Age 15-17.5	4.6	2.9	5.4	2.9	1.5	3.0	3.0
Age 18-21.5	6.7	5.9	21.4	5.9	5.1	6.5	6.5
Age 22-25.5	5.8	6.2	22.5	6.1	5.7	7.3	7.3
Age 26-29.5	4.2	5.0	14.4	5.0	4.9	6.6	6.6
Children Ever Born Before							
Age 20	0.53	0.39	0.96	0.40	0.34	0.47	0.47
Age 24	1.05	0.90	2.52	0.91	0.82	1.02	1.02
Age 28	1.41	1.30	3.90	1.33	1.21	1.62	1.62
Highest Grade Completed							
By Age 25	12.68	12.90	12.56	12.92	12.62	12.97	13.00

Table 6, continued

	Actual	MNL1 FE Baseline	MNL1 FE With Texas	MNL1 No FE Baseline	MNL1 No FE With Texas	DP Baseline	DP With Texas
			Hispanics				
Percent Receiving Welfare							
Age 15-17.5	1.3	0.6	8.7	0.6	0.1	4.4	1.7
Age 18-21.5	9.2	5.4	49.0	5.1	0.8	11.2	7.0
Age 22-25.5	15.0	10.3	57.5	8.9	1.2	15.1	10.2
Age 26-29.5	15.2	10.2	34.5	9.1	0.9	15.8	11.6
Percent In School							
Age 15-17.5	84.6	84.2	80.9	84.4	82.6	79.2	79.4
Age 18-21.5	22.0	29.2	20.5	28.8	23.3	21.4	21.6
Age 22-25.5	5.0	5.2	4.2	4.9	2.7	6.0	6.1
Age 26-29.5	2.0	2.1	1.3	2.0	1.2	2.8	2.9
Percent Working							
Age 15-17.5	22.2	20.1	8.7	20.2	24.0	26.6	26.4
Age 18-21.5	52.8	53.0	14.6	54.4	70.9	58.5	59.7
Age 22-25.5	58.7	62.2	15.6	63.8	83.6	57.8	61.2
Age 26-29.5	56.1	66.8	31.9	67.2	86.7	55.4	58.9
Percent Pregnant							
Age 15-17.5	3.2	3.8	7.6	3.8	1.5	3.2	3.1
Age 18-21.5	6.9	7.0	30.0	6.9	5.2	6.5	6.6
Age 22-25.5	6.7	7.1	30.2	7.6	6.1	7.1	7.1
Age 26-29.5	5.9	5.9	17.5	5.9	5.4	6.6	6.6
Children Ever Born Before							
Age 20	0.40	0.43	1.29	0.43	0.30	0.48	0.48
Age 24	1.00	1.00	3.35	0.96	0.78	1.03	1.02
Age 28	1.60	1.49	5.06	1.44	1.23	1.61	1.61
Highest Grade Completed							
By Age 25	12.20	12.80	12.50	12.94	12.53	12.38	12.40