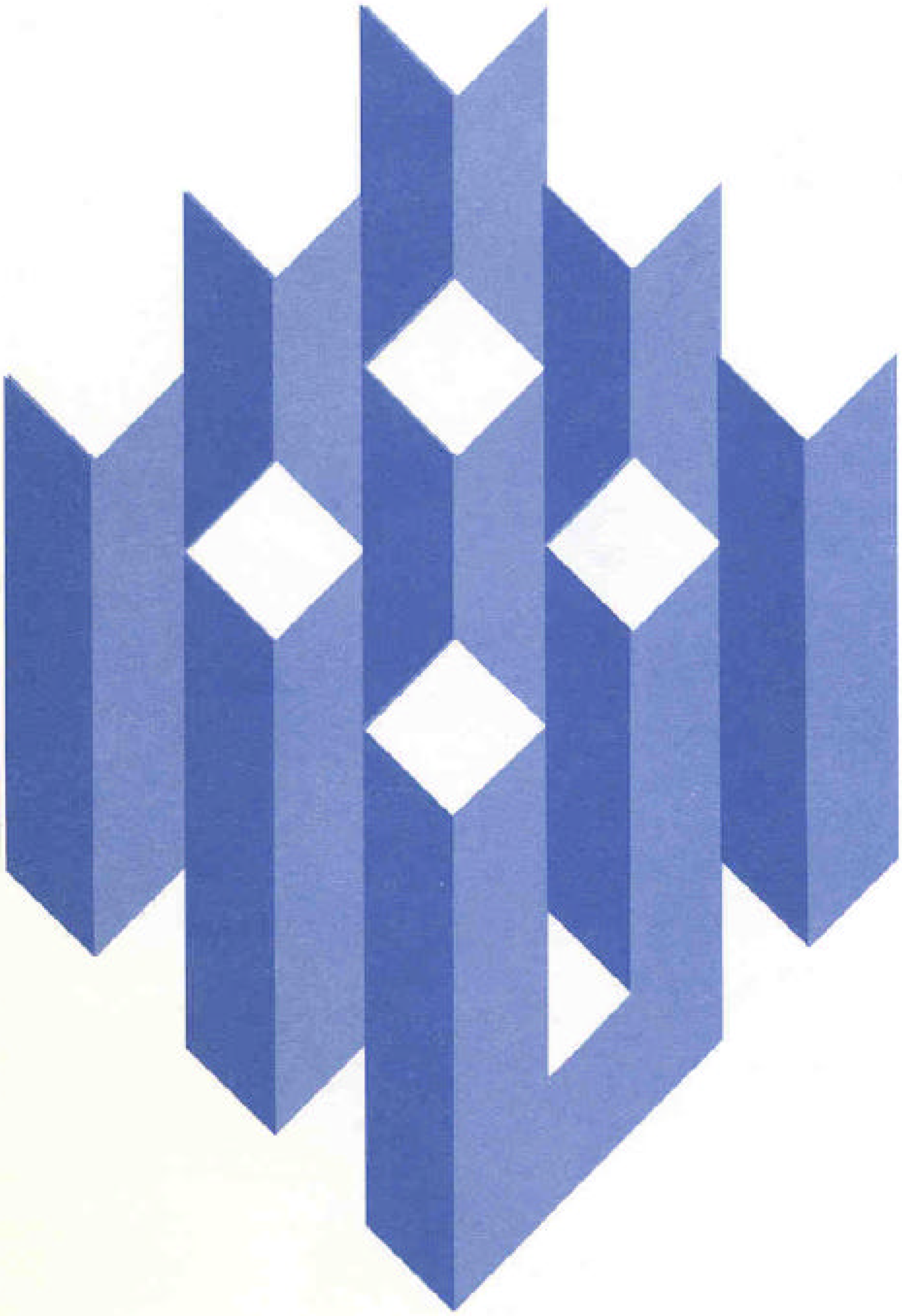
An Analysis of the 1981-82 Changes in the Extended Benefit Program

Unemployment Insurance
Occasional Paper 85-1

Department of Labor
Employment and Training Administration



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U.S. Department of Labor
William E. Brock, Secretary

Employment and Training Administration
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Walter Corson
Walter Nicholson

EXECUTIVE SUMMARY

In 1970 Congress enacted the Extended Unemployment Compensation Act which provided for the payment of additional unemployment benefits during periods when Insured Unemployment Rates (IURs) exceed specific trigger levels. Since then the extended benefits (EB) program has provided a "first line of defense" to workers threatened with lengthening unemployment durations during recessionary downturns. Although some changes were made in the EB program throughout the 1970's, it was not until the early 1980's that a substantial number of major modifications were undertaken. First, the Omnibus Budget Reconciliation Act of 1981 (PL 97-35) instituted several changes in EB triggering formulas, all of which made it more difficult for states to become and remain eligible for the program. Specifically, these trigger changes included: (1) elimination of the national trigger (now only states with the required IUR are eligible for EB); (2) elimination of EB claimants from IUR trigger calculations, thereby mitigating situations where triggering on EB would help to insure the program's continuing in effect; and (3) raising the state EB trigger rate from 4 to 5 percent (or to 6 percent if the requirement that the IUR also exceed 120 percent of the corresponding average IUR in the prior two years is waived). The general goal of all of these changes was to concentrate EB payments in areas and time periods when unemployment rates were highest and to avoid paying EB in relatively strong labor markets.

A second set of changes introduced into the EB program in the early 1980's had the effect of restricting individuals' eligibility for the program. The Omnibus Budget Reconciliation Act of 1980 (PL 96-799) added new "suitable work" provisions to EB that closely paralleled those which had been instituted as part of the Federal Supplemental Benefits (FSB) program of the mid-1970's. These statutory changes required that EB recipients accept jobs that pay an amount equal to either the minimum wage or to their UI benefit, whichever is higher. The new law was somewhat more stringent than existing state laws which often defined "suitable work" in terms of recipients' prior employment history. The EB work test was further tightened by requiring recipients to provide "tangible evidence" of job search activity for continuing eligibility and by making disqualification penalties somewhat more severe. Finally, additional qualifying requirements for EB (requiring the equivalent of 20 weeks of full time work in the base period), were instituted as part of PL 97-35, thereby creating a further divergence between some state UI and EB eligibility provisions. The intent of all of this "tightening-up" of EB was to focus the program even further on workers who have a substantial employment history and who demonstrate clearly that they are actively seeking work.

In this report we investigate the effects that these recent changes have had on the EB program. Not only do we provide quantitative estimates of the degree to which EB claims and costs were reduced below what they would have been in the absence of the changes, but we also offer a number of related analyses that examine whether the program changes seem to have met their intended goals in the way they retargeted EB resources. Our general conclusion is that the recent EB changes had the effect of significantly reducing the size of the program, especially during periods of relatively modest unemployment rates. Changes in EB trigger provisions were primarily responsible for this reduction, with modifications in the EB work test and in the EB eligibility rules having much smaller, though still significant effects. All of the changes seem to have had the intended effect of focusing EB benefits more directly on geographic areas and time periods with high unemployment rates and on workers most firmly attached to the labor force. Whether such changes were "too large" in that too many "deserving" workers were eliminated from the EB program and how the changes interact with other federal extended benefit initiatives (e.g., FSC in the most recent recession) is difficult to judge, given both the limitations on our data and the necessary ambiguities involved in answering these questions.

Research Design

The research design we used to examine recent changes in the EB program had three basic elements. First and most importantly we used quarterly aggregate data on each state's UI system characteristics for the period 1964-1981 to develop a detailed simulation model of EB program operations. Key links in this model were developed in two ways. Behavioral reactions to the EB changes were estimated using standard econometric techniques. On the other hand, definitional links in the model (such as those involving EB triggering) were programmed in a way so as to reflect actual program operations as closely as possible. In its most complete form the only input required for the simulation model is the national unemployment rate—all other state-level predictions are developed internally to the model. In this way the simulation model can easily be integrated with more general macroeconomic models that provide estimates of overall unemployment.

A second part of our research design involved the use of individual data on 7,600 UI recipients in 12 states from the CWBH data system. These data served two general purposes. First, they provided the opportunity to estimate behavioral effects of the EB program changes using a somewhat different approach than that employed with the aggregate data. Hence, the individual data provided a check on our aggregate results. Second, we used the individual data to examine the impact of the new EB eligibility rules since they permitted us to investigate that impact in more detail than would have been possible using only aggregate data.

As a final approach to examining the recent EB program changes, we conducted site visits to six states. There we interviewed state and local UI administrators about the impact that the tightened work test and qualifying wage restrictions for EB had on their operations. Because of the small number of states visited and because the states were chosen primarily in connection with another project, the results of this examination should be regarded as mainly illustrative and may not be representative of all states' administrative experiences under the new EB procedures.

Summary of Results

Our summary of results follows the order of presentation of the four analytical chapters in this report. In Chapter III we describe the results of our econometric examination of EB effects using aggregate data. The principal findings were:

- As has been found in prior research, exhaustion rates for regular UI are significantly affected by EB availability. Other things being equal UI exhaustion rates were about 4 percentage points higher when EB was in effect than when it was not. Availability of benefits beyond EB (such as those provided by FSB or FSC) tended to increase exhaustion rates by an additional 3 percentage points.

- The fraction of UI exhaustees who collected an EB first payment (a figure which averaged 0.92) was significantly affected by economic variables and UI policy parameters. Specifically, availability of benefits beyond EB raised the fraction by about 3 percentage points whereas our aggregate estimates suggested that implementation of the new EB work test may have reduced it by as much as 10 percentage points.

- Our empirical results for EB exhaustion rates (which averaged 0.66 in the aggregate data) closely paralleled those for regular UI exhaustion rates. Most importantly, high wage replacement ratios and availability of benefits beyond EB tended to raise EB exhaustion rates, whereas enforcement of the UI work test (as measured by disqualifications for refusals of suitable work) tended to reduce EB exhaustion rates.

In Chapter IV we present our results of analyzing individual UI claimants' data from the CWBH files. The primary findings from this examination were:

- In many ways the individual results mirrored those from the aggregate data. Availability of benefits beyond EB was found to increase the fraction of UI exhaustees who collect an EB first payment and the fraction of EB recipients who exhaust their entitlements. The change in the EB work test was also found to have reduced the fraction of UI exhaustees who collect EB, but the magnitude of this effect (about 6 percentage points) was somewhat smaller than estimated in the aggregate data. In addition, the change in the EB work test was found to have reduced EB exhaustion rates, a result not found in the aggregate estimate.

- Availability of the individual data permitted us to examine directly the determinants of the number of weeks of EB collected (which averaged 7.4 weeks in our sample). We found that availability of benefits beyond EB increased such weeks substantially (by about 4 weeks) whereas the change in the EB work test reduced these weeks (by about 1.5 weeks).

- The change in the EB work test seems to have increased use of the employment service by EB recipients. Our data (which, unfortunately, were only available for three states) suggested that the likelihood of ES registration increased slightly following the change in the work test and job referrals to EB recipients increased rather sharply following the change. The likelihood of job placements, however, did not increase in the states we examined.

- Our examination of the new EB qualifying wage provisions suggested that overall relatively few (approximately 5 percent) of UI exhaustees were barred from EB eligibility by the changes. But the fraction of ineligible claimants varied widely from state-to-state depending on precisely how existing state laws corresponded to the new EB provisions.

Chapter V briefly describes the results of our analyses of states' administrative operations following the EB policy changes. Our major findings were:

- The new "suitable work" provisions required for continuing EB eligibility tended to be more stringent than prior practices in the majority of the states we visited. For those states the notion that the suitable wage might be adjusted downward as unemployment durations lengthen represented a major innovation in policy.

- The requirement that EB recipients provide "tangible evidence" of their job search efforts also represented a major policy change for some states. This provision was reflected in increased job search reporting requirements and additional eligibility reviews for EB recipients.

- The states generally reported that implementing the new EB provisions required relatively few additional administrative resources. For some states, there was an indication that some modest resource shifts had occurred between administration of eligibility reviews to regular UI and EB recipients and that implementation of the new qualifying wage standards required some extra computer programming. For the most part, however, these resource shifts appear to have been modest.

Finally, Chapter VI presents a comprehensive analysis of the EB program changes using our simulation model. The primary findings derived from using the model were:

- Our detailed examination of the validity of the simulation model suggested that it was relatively accurate in predicting EB caseloads and costs on the basis of the national unemployment rate. It was significantly more accurate than the aggregate regression forecasting techniques we examined, and it provided substantially more information than did these forecasting techniques. Because of its quarterly structure the model did have some difficulty in predicting the precise timing of EB effects—especially those related to triggering. But, overall we have substantial confidence that the model accurately predicts the long run effects of EB program changes.

- The simulation model suggested that if the EB changes had been in effect during 1978-81 (a period of relatively strong labor market activity) the size of the EB program would have been reduced substantially. Most importantly the trigger changes introduced in PL 97-35 would have reduced EB first payments by more than two-thirds had they applied to 1978-81. Changes in the EB work test would have reduced EB first payments by about 6-7 percent during the period whereas the changed EB eligibility rules would have reduced EB first payments by 5 percent.

- The simulation model suggested that the EB program changes had a smaller, though still quite significant impact during the higher unemployment period of 1982.4-1983.3. During that period we estimated that the EB program changes reduced EB first payments by about 24 percent relative to what they would have been had the program remained unchanged. Approximately 60 percent of that decline was attributed to the trigger changes, whereas the remainder resulted from the work test and eligibility modifications.

- Actual EB first payments during 1982.4-1983.3 fell far short of what was predicted by our model or by other methods that used the historical relationship between EB and the overall unemployment rate. In total we estimate that EB first payments were 55 percent below what might have been expected on the basis of historical experience. Our simulation model suggested that less than half of this decline could be attributable to the recent EB policy changes. The remainder primarily reflected the changing relationship between the total and insured unemployment rate that has been noted by many observers and (possibly) other factors that were unique to the 1982-83 recession.

- Our simulation of several hypothesized recessions suggested that the recent EB program changes (especially those related to triggering) had the effect of sharply reducing the size of the EB program during mild recessions. Even during relatively severe recessions the changes had the effect of focusing the EB program on the recessions' low points while cutting back significantly on benefits paid early in the recession and later during the recovery period.

An Overall Evaluation

Our general conclusion then is that the recent changes in the EB program had the effect of significantly reducing its overall size. The simulation results reported here suggested that, during periods of relatively strong labor market activity the reduction in EB caseloads may have been as large as 70 percent. Because most of that reduction came from changes in the EB trigger mechanism, effects during periods of weak labor markets were smaller, but still substantial. Hence, it is clear that the recent changes achieved their primary goals of reducing total EB expenditures and focusing the program more tightly in areas and time periods where labor markets are weakest.

Did the EB cutbacks go "too far?" In order to address that issue one must clearly specify criteria by which the overall adequacy of EB is to be judged. A principal rationale for paying extended benefits during recessions is to provide the same degree of protection enjoyed by regular UI recipients during normal times. Since the risk of experiencing a long spell of unemployment increases during recessions, it might be argued that the protection provided during such recessionary periods should also increase. For example, our study of the 1975-76 recession concluded that the EB program alone would have been sufficient to keep the total exhaustion rate for UI from rising during the period and that payment of FSB essentially amounted to overinsurance against the weak labor market of those years (Corson and Nicholson, 1982).

If the 1982-83 recession were, in this regard, similar to the 1975-76 recession, perhaps an unchanged EB program would also have been sufficient. Compared to 1975-76, the relatively modest temporary extended benefit program (FSC) implemented during 1982-83 suggests that policymakers may have indeed believed that to be the case. Even with the recent EB cutbacks, we found little evidence that total UI exhaustion rates rose rapidly during the 1982-83 recession. So, by that test at least, the recent changes may not have been too severe.

However, this sanguine conclusion may rely too heavily on the exhaustion rate as a measure of UI adequacy. One reason overall exhaustion rates were not increased substantially by the EB cutbacks is that some of these changes had the effect of eliminating many UI claimants before they ever reached EB exhaustion. That fact, combined with the more general decline in UI eligibility of the unemployed during the 1982-83 recession resulted in a very large shortfall in EB program caseloads and costs over what might have been anticipated, given the weakness in the labor market. But, relatively little is known about the characteristics of those individuals who did not collect EB (or, indeed, regular UI) during the recent recession. So, the welfare consequences of the reduced eligibility of the unemployed remain ambiguous.

I. BACKGROUND AND POLICY ISSUES

Beginning in 1970, Congress established a permanent program to extend the duration of unemployment insurance benefits during periods of high unemployment. This program, Extended Benefits (EB), was altered several times during the 1970's, with most changes being temporary. Beginning in 1981, however, several major permanent changes were made. The triggering mechanism by which the EB program becomes operational was changed to require higher levels of unemployment before EB becomes available. In addition, Congress tightened the qualifying and suitable work provisions for individual claimants. The last of these changes went into effect in October 1982, and since that time the number of states on EB and total benefits paid under the program have fallen dramatically despite the high levels of unemployment. This result appears to be due both to the more stringent state and individual eligibility requirements enacted by Congress and to a shift in the long run relationship between the Insured Unemployment Rate (IUR),[1] which is used to trigger on EB benefits, and the total unemployment rate. In the last several years, relatively lower levels of the IUR were associated with a given total unemployment rate than was previously the case.

This report explores the impact of the first of these reasons by attempting to measure the impact of each of the recent triggering and individual eligibility changes on recent EB claims and costs and, more

[1] The Insured Unemployment Rate is defined as the number of regular unemployment insurance claimants divided by the number of individuals employed in jobs covered by the UI System--i.e., it is an unemployment rate for individuals covered by UI.

generally, on EB claims and costs under a variety of economic scenarios. The second reason for the recent declines in EB costs is examined only briefly in conjunction with our main analysis. A detailed analysis of this latter issue is presented in Burtless (1983). A further point to note about the analysis presented here is that it focuses soley on how EB changes have affected the provision benefits through the permanent EB program. The extent to which the temporary Federal Supplemental Compensation (FSC) program has altered this situation for the most recent recession is not addressed. Thus, the analysis concerns only the EB program not the entire federal response to the 1982-83 recession.

In the remainder of this chapter, we discuss the policy background to the permanent extended benefits program, showing how passage of that program represented the culmination of experiences with several earlier programs that extended potential unemployment insurance (UI) durations on a temporary basis. Specific policy questions that have surrounded the EB program since its inception are also identified. Following that discussion, we turn (in Section B) to a brief description of recent legislative changes in the EB program. Key policy questions which concern those changes and which provide the focus for our analysis are then outlined in Section C. The final section outlines the remainder of this report.

A. BACKGROUND

Rising exhaustion rates for regular UI benefits have prompted Congress to provide additional, extended benefits to workers on numerous occasions since the 1950s.[1] Prior to 1970 these programs were adopted on an ad hoc basis as economic conditions warranted, and because these programs sometimes involved extensive Congressional debate, they were not always implemented in either a timely or an efficient manner. The need for a more consistent governmental response to unemployment insurance needs during recessions was explicitly recognized with passage of the Extended Unemployment Compensation Act of 1970. Under this act extended benefits (EB) were payable in a state anytime the state's insured unemployment rate (IUR) was an average of 4 percent or more for a 13 consecutive week period and was at least 120 percent of the average IUR for the corresponding 13 week period in the prior two years.[2] These EB benefits were also available in all states when the seasonally adjusted national insured unemployment rate exceeded 4.5 percent for 13 consecutive weeks.[3] When EB is "triggered on" under these state or national criteria, all UI claimants who have exhausted their regular UI entitlements and who have not ended their benefit years are eligible for up to 13 additional weeks of benefits (or one-half their original UI entitlement if that is less) up to a maximum of

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[1] For a description of those early extended benefit programs, see Corson and Nicholson (1982), Chapter II. The largest of these programs was the Temporary Extended Unemployment Compensation (TEUC) program of 1961 under which approximately 3 million claimants collected nearly $1 billion in benefits.

[2] The 120 percent requirement has, at various times, been waived for states with sufficiently high IURs (4.5 percent in 1973 or 5 percent under 1976 legislation). The requirement was also waived completely for a period of time during the mid-1970s.

[3] The national trigger rate was changed to 4.0 percent during the 1975 recession and the calculation was changed to a 13 week moving average to correspond to the state triggers in 1976, although the state IUR's used for the trigger are not seasonally adjusted.

39 total weeks of benefits.[1] Under the 1970 Act availability for work requirements for continuing EB eligibility were the same as those specified in state laws for regular UI.

Since 1970 the EB program has provided a first line of governmental UI responses to recessionary circumstances. Adoption of the trigger mechanism meant that the program would come into effect more or less automatically whenever economic conditions worsened, and in fact, some first payments have been made under EB in every year since the program's inception. During the 1970s two additional discretionary programs were also adopted that extended benefits beyond those provided by EB. The "temporary compensation" program of 1971 extended benefits to a maximum of 52 weeks.[2] That program was phased out in early 1973 only to be replaced by the much larger Federal Supplemental Benefits (FSB) program, which was adopted in late 1974 to address the severe recession of the mid-1970s. This program increased maximum UI durations, in two tiers, up to a total of 65 weeks of benefits. In all, more than 5 million individuals collected more than $6 billion under FSB which, after several modifications that

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[1] Individuals who are determined eligible for UI establish a benefit year beginning with the date of their initial claim. They can collect their regular UI entitlement during this year period until all potential benefits are collected or the year period ends.

[2] Because the trigger formula for this program differed from the one used for EB, it was possible for states to have no extended benefits, to have EB only, to have temporary compensation only, or to have both extended benefit programs.

restricted the program, expired in 1978. The FSB program and its subsequent modifications have been extensively analyzed.[1]

A new program of temporary extended benefits, Federal Supplemental Compensation (FSC), has also been enacted for the latest recession. This program began in September 1982, and it was initially expected to last through March 1983. Through subsequent extensions, it is now currently scheduled to expire at the end of March 1985. The FSC program provides additional weeks of benefits both in states with EB and states without EB with the additional amount varying with the IUR. Several changes have also been made in the range within which potential duration can vary. The minimum available in any one state has been 6 weeks and the maximum 16.

The attention given by policymakers and researchers to the FSB program has not been matched by similar attention to EB. Other than a modest amount of technical analysis of triggering issues (see below), the program has been mainly studied in relationship to other programs.[2] This is surprising since EB is second only to the regular UI program in terms of total benefits paid and UI recipients served. During the 1970s, for example, more than 12 million first payments were made under the EB program, and benefits totaled nearly $10 billion. During the 1981-82 recession, benefits payable under EB have continued at high levels in most

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[1] See Corson and Nicholson (1982), Corson et al. (1977), Brewster et al. (1978), Felder and Pozdena (1978), and Felder and West (1978).

[2] For example, the 15 state survey of FSB recipients included a small comparison sample of individuals who collected EB only (Corson et al., 1977). Nicholson (1981) and Hight (1975) both analyzed the suitability of EB durations during recessions and concluded that such extensions provided approximately the amount of benefits required to keep total UI exhaustion rates from rising during recessions.

states. Since EB benefits are half federally financed, whereas regular UI benefits are not, EB operations have become of increasing concern to federal policymakers.

Two issues have characterized debate over the EB program since its inception. First are questions relating to the EB trigger mechanism. State trigger requirements have been changed many times since 1971 in response to perceived problems with the original formula. Specific problem areas have included (1) dissatisfaction with the 120 percent requirement--especially when states experience high unemployment for long periods of time, (2) questions about the appropriate trigger level for state IUR's, (3) questions about the appropriate level (if any) for a national trigger, (4) questions about the proper definition of the trigger indicator (the IUR)--for example, whether it should be seasonally adjusted or whether it should include UI exhaustees or EB recipients in its numerator, and (5) questions about the speed with which and the length of time over which EB benefits are triggered on and off.[1] More generally, questions such as the desirability of local area triggers or of a more graduated approach to EB implementation (i.e., a few weeks at a time in response to labor market conditions) have also been widely discussed. Choices made with regard to any of these questions can have major implications for the timing of EB claims and costs. Despite the attention devoted to the triggering question, however, there is as yet no agreement on what the effects of

[1] For example, Michigan, despite its high total unemployment rate, triggered off EB during November 1981 when the state 13 week average IUR dipped below the trigger point. Then, even though the IUR rose immediately, the state was required to suspend EB for 13 weeks before the benefits were triggered on again.

specific triggering formulas are nor about explicit policy criteria that should guide trigger selection. Furthermore, evaluation of new procedures have tended to adopt largely descriptive methodologies that simply illustrate for policymakers the cost implications of the trigger choice studied.

A second general area of concern about the EB program relates to eligibility requirements. Under the original 1970 legislation, EB had the same eligibility provisions as those for the state's regular UI benefits. Once an individual exhausted UI, he or she simply began collecting EB under the same provisions. The philosophy implicit in such an approach is that EB recipients are essentially identical to regular UI recipients except that they have had the misfortunate of experiencing a longer spell of unemployment. Those who take the contrary view point to two issues. First, it is argued that, because EB is in part federally financed, it should have more consistent eligibility criteria than the large variation that results from relying on state laws. More uniform procedures would reduce some of the peculiarities (such as treatment of seasonal workers who file repeated UI claims) that are built into the state laws. Second, those who favor more uniform national criteria also tend to argue for restricting EB to workers who can show a "strong" labor market attachment. In practical terms, this has meant focusing either on requiring greater employment experience in the UI base period than is required under many state laws, or on requiring more stringent job search or job suitability standards than prevail under state laws. Such stiffer job suitability standards were incorporated into the FSB program (as part of PL95-19) in

1977, and these provisions have been found to have had a significant impact on disqualifications under the program (see Felder and Pozdena, 1978).

B. RECENT EB CHANGES

Two major areas of changes in the EB program were enacted during 1980 and 1981 in response to the longstanding concerns discussed above. First, with respect to EB triggering, the Omnibus Budget Reconciliation Act of 1981 (PL97-35) instituted several changes, all of which made it more difficult for states to become and remain eligible for the program. These included (1) elimination of the national trigger (now only states with the required IUR are eligible for EB); (2) elimination of EB claimants from IUR calculations (thereby mitigating situations where the triggering on of EB would raise the IUR and raise the likelihood of the program's continuing in effect); and (3) raising the state trigger rate to 5 percent (or to 6 percent if the 120 percent rule is waived). The first two of these changes went into effect in September 1981 and the third in September 1982.

These changes were proposed as part of the Reagan Administration's overall program to reduce government expenditures. In conjunction with this general goal, the trigger changes were intended to concentrate the reduced EB benefits in the areas and time periods with the highest unemployment. As stated by the Administration, eliminating the national trigger was designed to "prevent paying benefits in states with low unemployment where it is not needed." Changing the IUR calculation was designed to "remove several anomalies in the current program," whereby payment of EB can be delayed when unemployment rises,[1] payment can continue when unemployment drops below the level needed to start payment, and one state can be on EB while another state with an identical overall unemployment rate can be off EB. And, finally, the increase in the state trigger was intended to "redirect benefits to areas where they are needed, while removing incentives for prolonging unemployment in growth areas of the nation where job opportunities are available."[2]

A second set of recent changes modified EB eligibility provisions. The Omnibus Budget Reconciliation Act of 1980 (PL96-799) added suitable work provisions to EB that closely paralled those adopted several years earlier for FSB. Specifically, the new statutes required, effective April 1981, that EB recipients accept jobs that pay an amount equal either to the minimum wage or to their UI benefit, whichever is higher. This provision is more stringent than many states' job suitability criteria which usually tie suitable work criteria to recipients' prior employment history. In addition, EB claimants were required to provide "tangible evidence" of work search activity for continuing eligibility and disqualification penalties were made more severe. This change in the work test was expected to "help workers who have little chance to return to their old occupation face the reality of the changing economy and begin new careers" (OMB, 1981). Finally, additional qualifying provisions for EB, requiring 20 weeks of full-time work or the equivalent in the base period

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[1] This could occur because a prior period of EB would raise the IUR used in the 120 percent calculation, thus requiring an even higher level of unemployment to trigger-on a new period of EB.

[2] A statement of these goals for the EB changes is found in OMB (1981), 243-244.

were added in 1981 as part of PL97-35, creating a further divergence between some state and EB eligibility provisions. This last change became effective at the end of September 1982. It was expected to "minimize the occasions when workers receive benefits for longer periods of time than they were employed" (OMB, 1981).

In all then, the 1980 and 1981 changes in EB resulted in making it more difficult for states to be eligible to pay benefits under the program and for individuals to be eligible to receive benefits under the program.

C. POLICY QUESTIONS

The purpose of this report is to examine the operational and behavioral significance of the recent changes made in the EB program. Specifically, the principal question that is addressed is:

- How did the 1980 and 1981 changes in EB triggering and eligibility provisions affect claims and costs of the EB program relative to a situation under which the program had remained unchanged?

There are two ways in which these program changes might have affected EB claims. First, the changes, by affecting the eligibility of particular UI exhaustees for EB, can be expected to have had a direct, definitional effect on some persons who have exhausted their UI benefits in the period since the changes. Second, the programmatic changes may also have affected behavior of potential EB recipients, either by inducing them to find jobs and thus deterring them from collecting regular UI to the point of benefit exhaustion or by deterring them from applying for EB once they had exhausted. In this report we estimate both of these kinds of effects using a methdology that combines aggregate UI statistics and data on individual UI recipients. Such information should be useful to both federal and state policymakers, especially those interested in EB budgetary costs.

In addition to reducing costs, the EB changes were intended to concentrate benefits on areas and time periods with the highest unemployment. Thus, it is important to ask:

- How did the changes in EB triggering and eligibility provisions affect the distribution of EB benefits over the business cycle? How did they affect the distribution by state? And, more generally, how did the changes in EB affect the distribution of unemployment insurance coverage (i.e., regular UI and EB) between recessionary and nonrecessionary periods?

These questions are intended to help us evaluate whether the reductions in EB claims and costs were merely a way of reducing government expenditures or did they make sense in terms of equity considerations. One way of addressing this issue is to examine the distribution of the reductions in EB by the severity of unemployment, as the first two questions do. One can also ask, as the third question does, whether individuals who are laid-off during recessionary periods have more or less UI coverage of their unemployment spells than individuals laid-off during nonrecessionary periods. To help answer this question, we examine the impact of the changes on the probability of exhausting all available benefits (ie., regular UI and EB).

A number of issues about effects of the specific EB changes are also examined in this study. For the three changes in the trigger formulas (that is, elimination of the national trigger, elimination of EB recipients from IUR computations and raising of the state trigger rates) these issues include:

- What effect did each component of the trigger formula changes have in isolation? Was the overall effect a sum of these or were there important offsetting or compounding influences?
- What effects might the changes have had during periods when labor markets may have followed a variety of different potential patterns?

Our research permits us to provide reasonably reliable answers to all of these questions about triggering, and the answers should be of considerable interest to those involved with designing future changes in the EB trigger mechanism.

In addition to the basic issue of how many UI exhaustees failed to qualify for EB under the new (20 weeks of work) requirement we also examine a number of related questions including:

- What are the demographic and labor market characteristics of those excluded under the new provisions? Were the individuals excluded from EB more likely to collect UI for longer periods of time than they were employed than those not excluded? Did the provisions have a very different impact depending on the original state qualifying requirements? Would the alternative criteria for implementation have similar outcomes?

Answers to such questions help provide an assessment of whether the new qualifying requirements achieved their intended goal of focusing benefits on workers with more significant labor market attachments.

Finally, our examination of the new suitable work provisions specified for EB addresses issues of how the new statutes were implemented and of what effect they may have had on recipients' job search activities and likelihood of disqualification. Issues considered include:

- How different are the new provisions from exisiting state practices? How stringently are the new provisions enforced? How have state actions affected actual disqualifications? Has the new work test had any effect on the probability of collecting EB and on the duration of EB receipt?

Answers to such questions should not only illustrate how the new EB standards worked in practice but should also provide input into the continuing policy debate over how the UI "work test" should be enforced.

D. OUTLINE OF THE REPORT

The remainder of this report is divided into five chapters. In Chapter II we describe the simulation model that we used to analyze the effects that the recent program changes have had on EB caseloads and costs. This description identifies several key behavioral parameters that must be estimated in order to implement the model, such as the proportion of regular UI exhaustees who collect EB. Estimates of these parameters are then presented in the next two chapters. In the first of these chapters, Chapter III, we report estimates derived from aggregate, state level data on regular UI and EB caseloads and costs. An additional set of estimates of the same parameters is then made in Chapter IV using individual claimant data. This analysis permits us to assess the reliability of the results obtained from the state level data and it provides a richer descriptive treatment of the effects on claimants of the changes in the EB program. Further description of the impacts of the EB changes is then provided in

Chapter V through an analysis of administrative impacts. This analysis is based on information collected in visits to a sample of six states.

In Chapter VI, we then show how the empirical estimates presented in Chapters III and IV are used in the simulation model, and we assess the reliability of the model. Finally, we report the results of our simulations of the effects of the recent changes in EB policy. Estimates are reported for both actual historical periods and hypothetical economic circumstances. This analysis is then used to draw general conclusions about the effects of the programmatic changes.

II. A SIMULATION MODEL OF THE EB PROGRAM

In this chapter we describe the model that we used to analyze the effects that recent program changes have had on EB caseloads and costs in each state and in the nation as a whole. After a few brief introductory comments about why we chose to employ a simulation modeling approach to these questions, we then (in Section A) examine the structure of this model in detail. That discussion pinpoints a few key parameters that require statistical estimation, and in Section B we describe the theory which lies behind that estimation. In Section C, we then show how our model of the EB program can be integrated with more general macroeconomic models. Specifically, we show how most of the crucial inputs for the model can be related to the national unemployment rate so that predictions of that variable can be used to generate EB predictions as well. Our attempt to link basic UI variables to the national unemployment rate is of interest in its own right because of the insights it provides into current controversies over the relationship between the total and insured unemployment rates. A discussion of that relationship is also provided in Section C. Finally, in Section D, we summarize our model and indicate briefly how it can be used.

Before starting our detailed discussion of model specification, it may be helpful to describe briefly why we chose to adopt a simulation approach. There are three methodologies that might have been used to examine impacts of the recent EB changes: (1) a qualitative study based on interviews with state officials responsible for implementing the changes; (2) a statistical study based on aggregate EB data; and (3) construction of a detailed model of the EB system. Although we do report (in Chapter V) results of some on-site interview with state officials, we believe these do not provide the types of quantitative information needed for policy planning. Statistical modeling of aggregate EB data might have offered some prospect of being able to identify quantitative program impacts, but we believed that such a study would run serious risks of confounding programmatic changes with other events occurring in the economy at large. It also might not have yielded sufficient details on why various changes occurred. Hence, we were led to adopt a simulation methodology for most of our analysis. We believe that approach offered both the possibility of obtaining detailed quantitative estimates and the opportunity to experiment with the precise structure assumed for operation of the EB system. Thus, the simulation approach seemed the best one to take for the questions we wished to answer. Now we will examine the basic elements of the model.

A. BASIC STRUCTURE OF THE SIMULATION MODEL

Our simulation model was constructed to reflect the flow of workers through the UI-EB system at the state level. The model is based on four fundamental identities that seek to replicate this flow for periods when EB is in effect in a state:

$$(1) \quad UIEXH \equiv (EXR)(UIFIRST)$$

$$(2) \quad EBFIRST \equiv (EBRATIO)(UIEXH)$$

$$(3) \quad EBDOLLARS \equiv (AVEPAY)(EBFIRST)$$

$$(4) \quad EBEXH \equiv (EBEXR)(EBFIRST)$$

where

UIEXH	=	total exhaustions of regular UI in the state
EXR	=	exhaustion rate for regular UI in the state
UIFIRST	=	first payments for regular UI in the state
EBFIRST	=	first payments under EB in the state
EBRATIO	=	ratio of EB first payments to UI exhaustions
EBDOLLARS	=	total dollars of EB benefits in the state
AVEPAY	=	average dollars of EB benefits paid to each recipient
EBEXH	=	total EB exhaustions in the state
EBEXR	=	state exhaustion rate for EB

To convert these basic identities into a structural model three decisions had to be made:

1. Which of the variables should be generated within the model and which should be considered exogenous, outside the model?

2. Which of the variables in the model should be viewed as "behavioral" and therefore subject to statistical analysis?

3. How should the time periods associated with the variables be defined so as to be consistent with the available data?

In this section we describe our solutions to these questions.

Because of their direct policy relevance, estimates of the four outcome variables UIEXH, EBFIRST, EBDOLLARS, and EBEXH are generated directly in the model. UI first payments are taken as exogenous--that is, they are an input into the model. Although we provide a rough way of predicting UI first payments from the national unemployment rate (see

Section C) our research did not stress those estimates and other methods of predicting UI first payments at the state level might be used.

We also assumed that availability of EB itself was exogenous to the flow Equations (1)-(4). That is, for each state, we developed a variable indicating whether or not the state was paying EB in a particular quarter. For some of our simulations this information was truly exogenous--being taken from actual experiences. In other cases, we attempted to predict each state's EB status using a "trigger" link to our model. The structure of that link is described in Section C also.

The other four variables in Equations (1)-(4) are taken as behavioral parameters. That is, since we believed that EXR, EBRATIO, AVEPAY, and EBEXR might be affected by the economic environment and by UI and EB policies, we decided that these variables should be subject to a statistical analysis using aggregate state data. The final three were also analyzed using individual data (see Chapter IV). The basic theories behind our examination of these relationships are discussed in the next section. In the actual operation of the simulation model several different variants of equations predicting these behavioral variables were employed. Reasons for adopting these variants are briefly described in the conclusion to this chapter and in Chapter VI.

A final series of choices required for construction of the simulation model concerned temporal aggregation of the data. Our basic decision was to adopt a quarterly framework. A finer disaggregation into monthly data might have been possible, at least for some of our model links. But we believed that the complexities that would have been introduced by the need to model flows very accurately so as to avoid large unexplained variations made a monthly approach prohibitive. On the other hand, aggregation of the available data into annual figures would have obscured intra-year variations which may provide the best indications of how the EB changes worked (e.g., with respect to triggering). Consequently, adoption of a quarterly approach to the data was chosen as being the most appropriate for the questions we were investigating.

Given this quarterly focus, there remained the issue of how time dimensions for the variables in Equations (1)-(4) were to be defined. Although we examined a number of alternatives, in the end we opted for a simplified approach under which all variables in Equations (2), (3), and (4) were regarded as referring to the same quarter, whereas the regular UI first payment variable in Equation (1) was lagged two quarters. Implicitly, we adopted a view of the flow of workers through UI and EB that had all workers receiving a first payment at the start of a quarter (say t), collecting UI for up to two quarters--26 weeks--and then, if still unemployed, exhausting their UI entitlements, collecting EB, and, if necessary, exhausting EB all in the same quarter (t+2). Of course, there are many reasons why this assumed pattern may not accurately reflect the experiences of particular workers. Workers may have gaps in their periods of unemployment thereby leading them to collect regular UI over more than two quarters. Similar possibilities exist for EB collection and for possible gaps between UI exhaustion and receipt of an EB first payment. Perhaps most important, adoption of an artificial aggregation of the flow data into quarterly categories may introduce considerable inaccuracies for individuals who start their periods of benefit collection later in a calendar quarter. In such cases, benefit collection experiences will in all likelihood span more than the assumed number of quarters and the aggregate data will not precisely correspond to actual worker flows. Despite these difficulties, we believed that our quarterly aggregation represented the patterns for many workers as they flow through the UI system, and we decided to adopt it as the best of the schemes available. Since the model seems to track actual experiences fairly well, our choice may not have been too bad. But, given the degree of temporal aggregation, it will always be the case that the model will exhibit some inaccuracies in predicting precise quarterly magnitudes.[1]

B. BEHAVIORAL PARAMETERS

Our model treated EXR, EBRATIO, AVEPAY, and EBEXR as behavioral outcomes. That is, we believed that changes in the economic environment or in UI and (more importantly) EB might affect these magnitudes and we wished to estimate these effects as one part of our overall evaluation. Two different methods were used to make such estimates: (1) statistical analyses of aggregate cross section time series data from all states; and (2) statistical analyses of individual data from a subsample of 12 states. Although the precise details of each of these analyses are presented in Chapters III and IV respectively, here we will briefly describe the theory that underlies both of them. Each behavioral relationship will be discussed separately.

[1] Further complications are also raised by our specification that EB is either "on" or "off" in a quarter, whereas in reality, the program can be "on" for only part of a quarter. Hence, the model predicted even less well in transition quarters when a state's EB status changed.

1. EXR - Regular UI Exhaustion Rate

Our behavioral theory of regular UI exhaustions was taken largely from prior research (Nicholson and Corson, 1978; Nicholson, 1981). Based on these previous results, the natural log of the exhaustion rate (LNEXR) was assumed to be a function of worker's personal characteristics (X), labor market conditions (L), and UI policy parameters (G):

$$\text{(5)} \qquad LNEXR = \alpha + \beta X + \gamma L + \delta G + u$$

Although we will delay a discussion of the specific entries in the vectors X, L, and G until Chapter III, here we can indicate the kind of theory that guided our selection. Our primary goal was to examine factors that might either increase the length of individual's unemployment spells (thereby raising EXR) or that might increase their weeks of regular UI entitlement (thereby lowering EXR). For our current purposes, two factors were of principal importance. First, we wished accurately to model the effect of weakening labor market conditions on the exhaustion rate. Since a principal goal of this study was to examine the cyclical performance of the EB program, obtaining an accurate way of predicting how many individuals who received a UI first payment might eventually be eligible for EB (by exhausting UI) had high priority. Second, we wished to investigate whether the recent changes in EB legislation had any effect on UI exhaustions. For example, if these changes made EB less attractive than had previously been the case, workers may have searched harder for work as their UI exhaustion dates neared and exhaustion rates may therefore have fallen a bit. Some of our attempts to capture this "deterrence" effect are described in Chapter III. Most of the other variables included in Equation (5), though of some interest in their own right, were not directly relevant to our assessment of EB. Their effects will be only briefly mentioned in connection with our statistical analysis.

2. EBRATIO - Fraction of UI Exhaustees Who Collect EB

The parameter EBRATIO was used to measure the likelihood that a UI exhaustee will go on to collect EB (assuming EB is available). This parameter was assumed to be determined in part by EB policy. For example, the new eligibility rules for EB probably reduced the average value of EBRATIO and, as suggested in Chapter I, the size of this effect varied from state to state depending on existing provisions for regular UI eligibility. EBRATIO may also have a behavioral component since the fraction of exhaustees going on to EB may be influenced by the relative attractiveness of continued UI receipt versus other options (e.g., employment). In Chapter III we will be primarily concerned with examining these latter behavioral effects using aggregate data. Because our aggregate data did not cover the period when the EB eligibility rules changed (September 1982) we will use individual data to examine these effects (in Chapter IV). There we also provide estimates of behavioral impacts on EBRATIO that tend to parallel those obtained from the aggregate data.

3. AVEPAY - Average EB Payment

We defined AVEPAY to be the average amount of EB benefits paid to each EB recipient. Specifically, the variable was defined as the ratio of the total dollar amount of EB paid in a quarter to the number of EB first payments in that quarter. Two major factors accounted for the variance in this parameter: (1) the average weekly UI benefit in a state and (2) the average number of weeks for which EB was collected. Our approach to this distinction was somewhat different in the aggregate and in the individual data analysis. For the aggregate analysis we used AVEPAY directly. Hence, any effects estimated there reflected a combination of effects on the average weekly benefit level of EB recipients and on the number of weeks collected. Since AVEPAY provided us with a direct way of estimating changes in EB costs, this combining of effects may be appropriate. In our analysis of the individual data, however, we took a more behavioral approach and examined determinants of weeks of EB collected independently. Results of this examination were relatively similar to those from the aggregate data, however.

4. EBEXR - EB Exhaustion Rate

Our modeling of the EB exhaustion rate closely followed the approach taken to model the exhaustion rate for regular UI. That is, we sought to identify how personal characteristics, the economic environment, and UI and EB regulations affect EBEXR. The most difficult problem faced in this analysis arose in defining EBEXR in our aggregate data. Because of repeated triggering on and triggering off of the EB program, it was very difficult to obtain a meaningful way to define exhaustions. Ultimately, our solution was to focus on a subset of quarters in which these problems were, to an extent, mitigated. The details of this approach are reported in Chapter III. Because of the potential shortcomings in this analysis, we also examined determinants of EB exhaustions using individual data and the results of that examination are reported in Chapter IV.

Overall then, our treatment of the parameters EXR, EBRATIO, AVEPAY, and EBEXR provided a number of avenues through which economic factors and behavioral responses might have affected performance of the EB program. Our approach to estimating such effects was two-fold. First, as reported in Chapter III and IV, we developed econometric estimates based on rather detailed statistical models. Then, as shown in Chapter VI, we conducted a series of "sensitivity" exercises with our simulation model to determine the relative magnitudes of such possible effects. Consequently, we believe that we have spanned the likely size of the effects of the EB policy changes in our estimates.

C. EXOGENOUS INPUTS TO THE MODEL

As discussed briefly in Section A, our model operated from two fundamental exogenous inputs: (1) a trigger indicator specifying whether EB was in effect in a quarter and (2) UI first payments. In this section we will describe these sections of the simulation model in more detail.

One of our primary goals in developing methods for inserting exogenous inputs into the simulation model was to make it possible to tie the simulation foreeasts closely to those from more general economic models. For that purpose, we decided to use the seasonally adjusted national unemployment rate (SAUR) to predict both the state's IUR (for use in determining state's EB status) and state first payments under the regular UI program. For both of those variables similar time series regressions were run for each state for the 1964-81 time period. Each regression included the SAUR, seasonal dummies, and a time trend. Results of those regressions are reported in Tables II.1 and II.2. Because the results for the IUR regressions (Table II.1) are relevant to many other

TABLE II.1

EQUATIONS USED TO PREDICT STATE IURs

State	SAUR	Q1	Q2	Q3	Trend	Constant
1. Alabama	0.7220	1.0430	0.0761	0.0042	0.0016	-0.9525
2. Alaska	-0.0241	5.7470	1.0800	-3.0330	0.0516	6.8470
3. Arizona	1.0480	1.0580	0.1830	0.1834	-0.0496	-1.3010
4. Arkansas	0.9330	2.3510	0.1182	-0.3863	-0.0145	-1.1600
5. California	0.6923	1.4380	0.3820	-0.0959	-0.0307	1.6630
6. Colorado	0.4988	1.1650	0.1868	-0.0789	-0.0055	-1.0570
7. Connecticut	1.1170	1.5290	0.3725	0.9206	-0.0431	-1.6790
8. Delaware	0.7107	2.0280	0.1053	1.2940	-0.0029	-1.5910
9. District of Columbia	0.4525	0.5327	-0.0100	0.2472	0.0003	-0.6108
10. Florida	0.7642	0.2576	-0.1128	0.4852	-0.0247	-1.1870
11. Georgia	0.8240	0.8992	0.1941	0.3477	-0.0148	-2.1240
12. Hawaii	0.5714	-0.0665	-0.5234	-0.6055	-0.0073	0.6516
13. Idaho	0.4949	3.4030	0.3217	0.3690	0.0148	-0.0791
14. Illinois	0.8117	1.4680	0.4068	0.1483	0.0141	-2.4330
15. Indiana	0.6809	1.5320	0.2651	-0.0295	-0.0026	-1.7560
16. Iowa	0.4502	1.8810	0.3276	0.2520	0.0109	-1.2640
17. Kansas	0.4832	1.6300	0.2030	0.3817	-0.0108	-0.3546
18. Kentucky	0.7372	2.5810	0.5792	0.2726	-0.0029	-1.2020
19. Louisiana	0.5642	1.5880	0.8259	0.4052	-0.0130	0.0019
20. Maine	0.9529	2.4360	0.4881	-0.3539	-0.0163	-0.7720
21. Maryland	0.7484	1.5090	0.3290	0.1226	-0.0144	-1.1710
22. Massachusetts	0.9696	1.7760	0.2143	0.1427	-0.0370	-0.3503
23. Michigan	0.8806	2.3730	0.0963	1.5470	0.0317	-2.2530
24. Minnesota	0.7840	2.4050	0.3800	-0.2432	-0.0262	-1.2540
25. Mississippi	0.7596	1.5650	0.2902	0.0228	-0.0036	-1.5900
26. Missouri	0.7165	2.1300	0.3601	0.3628	-0.0062	-0.9851
27. Montana	0.5482	3.8840	0.2993	-0.5000	0.0103	0.0189
28. Nebraska	0.5210	2.0260	0.0702	-0.0120	-0.0173	-0.8186
29. Nevada	0.7596	1.6900	-0.1745	-0.6206	-0.0360	1.3940
30. New Hampshire	1.1400	1.2290	0.4129	0.0133	-0.0458	-2.8070
31. New Jersey	0.8071	2.2650	0.6508	0.3342	-0.0054	-0.2369
32. New Mexico	0.6848	1.5980	0.2373	-0.0794	-0.0250	0.2774
33. New York	0.8143	1.5480	0.2489	0.0916	-0.0204	-0.2338
34. North Carolina	0.9793	1.6030	0.4324	-0.0093	-0.0338	-2.2650
35. North Dakota	0.2633	4.8500	0.8234	-0.5299	-0.0132	1.0470
36. Ohio	0.7602	1.4690	0.0836	-0.1110	0.0014	-1.9950
37. Oklahoma	0.6265	0.9756	0.1782	-0.0263	-0.0471	0.6289
38. Oregon	0.7819	2.3790	0.3664	-0.8666	0.0004	-0.1760
39. Pennsylvania	1.0530	1.8540	0.1768	-0.0029	-0.0028	-2.2700
40. Rhode Island	0.9424	2.632	0.4557	1.0740	0.0012	-1.4870
41. South Carolina	1.0180	0.8175	0.0601	-0.0950	-0.0212	-2.2960
42. South Dakota	0.3900	2.5150	0.1528	-0.1277	-0.0163	-0.1449
43. Tennessee	0.7471	2.0890	0.3692	-0.0156	-0.0137	-0.9353
44. Texas	0.3754	0.5704	0.1325	0.1092	-0.0177	-0.1381
45. Utah	0.4956	2.3850	0.1746	-0.1295	-0.0226	0.8725
46. Vermont	1.0710	2.4500	0.4719	-0.4191	-0.0197	-1.4970
47. Virginia	0.6119	1.2860	0.4721	0.2510	-0.0081	-2.0900
48. Washington	1.2350	2.3450	-0.1532	-0.1604	-0.0402	0.0732
49. West Virginia	0.5616	3.3900	0.5457	-0.0794	0.0192	-0.4964
50. Wisconsin	0.7434	2.3050	0.2283	0.0250	0.0104	-1.9520
51. Wyoming	0.3390	2.3060	0.3417	-0.3421	-0.0231	0.2824

TABLE II.2

EQUATIONS USED TO PREDICT STATE FIRST PAYMENTS UNDER REGULAR UI

State	SAUR	Q1	Q2	Q3	Trend	Constant
1. Alabama	4487	16100	-2363	4101	401	-17040
2. Alaska	512	2494	-1440	-2843	111	-593
3. Arizona	3245	2562	-822	-75	-19	-6702
4. Arkansas	2790	6864	-3896	-4646	125	-4085
5. California	24900	82210	-15830	-15480	379	58500
6. Colorado	1836	6118	-91	2272	121	-5738
7. Connecticut	5885	21950	-2521	15480	-39	-3058
8. Delaware	534	5002	-687	2749	30	-16
9. District of Columbia	979	1439	-142	1174	20	-1372
10. Florida	11130	1455	601	12430	14	-33880
11. Georgia	10080	18340	-2811	2826	263	-36960
12. Hawaii	1078	375	-1137	-1003	37	-420
13. Idaho	522	4707	-1269	-213	91	-198
14. Illinois	16310	44430	-2706	-4607	405	-32290
15. Indiana	6148	25830	-2236	-810	241	-10560
16. Iowa	2317	9979	-321	-207	226	-8791
17. Kansas	1868	7511	-406	1874	63	-3237
18. Kentucky	3574	20710	-1735	45	274	-10750
19. Louisiana	1861	10830	2837	-96	181	1553
20. Maine	1997	5372	871	1942	79	-3946
21. Maryland	4362	18500	-2495	3913	55	-4984
22. Massachusetts	8636	33570	-6849	1430	-221	11820
23. Michigan	11700	43090	-13510	50110	902	-19760
24. Minnesota	5760	17840	-4888	-7881	62	-10500
25. Mississippi	2361	8231	743	-540	112	-7482
26. Missouri	4614	24220	-484	2985	246	-3857
27. Montana	557	4212	-706	-1842	46	295
28. Nebraska	1579	5754	-1332	-1287	13	-3173
29. Nevada	1117	1607	-2061	-1995	30	748
30. New Hampshire	2304	2110	6430	2537	-1	-8803
31. New Jersey	8121	36170	-2719	2792	362	14400
32. New Mexico	769	2453	-367	-759	0	598
33. New York	20630	93320	-5094	7894	-1213	68810
34. North Carolina	17160	36690	-776	-1467	-150	-55620
35. North Dakota	277	3332	-622	-1378	38	-102
36. Ohio	15710	44990	-8675	-354	357	-38430
37. Oklahoma	2738	4087	-23	-596	-38	-3799
38. Oregon	3211	11270	-4052	-6524	198	-2241
39. Pennsylvania	18730	- 80330	-7293	3353	612	-28890
40. Rhode Island	958	8378	1293	3023	101	1406
41. South Carolina	7959	12280	188	2662	-2	-28500
42. South Dakota	419	2237	-266	-470	17	-1065
43. Tennessee	5618	25330	-3499	-1640	230	-12030
44. Texas	8114	10100	-999	402	43	-12100
45. Utah	1114	5267	-1189	-1184	36	-433
46. Vermont	577	2457	-177	-930	34	-704
47. Virginia	6342	15760	-36	-676	118	-24810
48. Washington	3877	9041	-4100	96	93	11680
49. West Virginia	1079	15240	-1943	870	205	-81
50. Wisconsin	4851	22110	-7132	-4069	434	-10250
51. Wyoming	279	1345	-381	-796	9	-270

research issues, we will describe them in some detail here. After discussing the regressions we will also provide a brief description of how we predicted EB triggering in each state.

The most general finding of our regressions on state IUR's was that the results were qualitatively similar across states. The equations all had fairly high R^2's (above .6), the SAUR variables always had a statistically significant coefficient, quarterly patterns were quite similar, and many states had statistically significant negative time trends in the relationship between the two measures of unemployment. Hence, there appeared to be enough consistency to warrant our using the coefficients in Table II.1 to predict IURs.

As we discussed briefly in Chapter I, the negative trends reported in Table II.1 have recently been the subject of controversy and the reasons for the secular decline in IURs are not well understood (see Burtless, 1983). The state-by-state results reported in Table II.1 may provide some insights into this question. First, they showed that the existence of a negative trend is widespread. Thirty-eight of the 51 jurisdictions examined had negative trends and those trends were, in most cases, statistically significant. Second, a few very large states (notably California and New York) had relatively large negative trends--an estimated decline of at least 0.02 per quarter or about 0.1 percent per year--so it is not surprising that aggregate national data also show a negative trend. Interestingly, none of the states in the "industrial heartland" had such significant negative time trends, however. The trends estimated for Illinois, Michigan, Ohio, and Wisconsin were all positive, and those for Indiana and Pennsylvania were essentially zero over our period of estimation. Indeed, with the exception of Alaska, Michigan had the largest positive trend among those estimated.[1] Consequently, the long run decline in the ratio of insured to total unemployment may be in part related to the declining importance of employment in these industrial states as a fraction of the national total. Whether the differential trends could be explained by differences in the industries located in these states and how frequently their employees collect UI, or whether the differential arose from conscious policy decisions in the states was difficult to determine within this limited part of our study, but the state differences did suggest the desirability of taking a state-by-state approach to the data.

The other coefficients in Table II.1 warrant less comment. Those for the SAUR were about as expected. That is, states with highly cyclical industrial structures (Connecticut, Pennsylvania, Michigan, New Jersey, and so forth) tended to have higher coefficients for the SAUR than did those states that seem more immune to the national business cycle (e.g., Texas). The size of the quarterly dummies were both quite large and varied greatly from state to state. As a generalization it might be noted that those states with the largest coefficients for Quarter 1 tended to be those with the harshest winters (Alaska, Idaho, Montana, and North Dakota). But, even in other states, quarterly patterns were quite pronounced. Although we did not investigate in detail the reasons for these strong seasonal patterns, it is clear that these have important effects both on the performance of the EB program (indeed, seasonality is a principal reason

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[1] For the period after 1981, however, the IUR in Michigan declined significantly—especially so in light of the poor labor market conditions there. The presence of many exhaustees in the Michigan labor force who had not reestablished UI eligibility may, in part, explain this recent decline.

for the "120 percent" provision in the EB trigger mechanism), and on many other aspects of the operations of the states' regular UI programs.

Because of the vastly different sizes of states' labor forces, it is less easy to generalize about the regressions on first payments (Table II.2). Again, all of the states were characterized by equations that fit the data rather well. And, the SAUR was statistically significant in virtually all of the runs made, as were many of the seasonal dummies. The equations therefore seemed to track first payments rather well and provided a suitable starting point for our simulation runs.

It should be stressed that the use of the equations in Tables II.1 and II.2 to predict IURs and regular UI first payments was not required by our model. These data can instead be inserted for some actual historical period, or values can simply be assumed. But tying the IUR and first payments values explicitly to the SAUR seemed to us a natural way to tie the simulation model to the larger national economy. So our simulations were primarily based on this approach.

One particularly important use of our IUR predictions should be explicitly highlighted--their use to derive an indicator for each state's EB trigger status. The first step in the creation of this link of our model was to assume that states were either "on" EB in a quarter or "off" EB in that quarter and that such trigger decisions were based on the quarterly IUR. These assumptions were at variance with actual procedures for two reasons. First, EB may be on for only part of a quarter. And, second, EB triggering is based on a moving average of weekly unemployment rates. Because incorporation of either of these complications into our model would have been extremely difficult, we decided to opt for the simpler, quarterly-based approach. While this probably meant that our EB trigger predictions were sometimes wide of the mark, we did not believe that such inaccuracies would interfere with our basic goal of estimating the long-run effects of recent EB program changes.

Determination of a state's EB trigger status was not accomplished by simply comparing the predicted value of the IUR to the required trigger rate, however. Three other aspects of the trigger mechanism were also taken into account: (1) possible existence of a national trigger; (2) consideration of the "120 percent" rule; and (3) allowance for inclusion of EB recipients in the trigger indicator (as was the case prior to the 1981 changes). The first of these was the simplest to implement. A national prediction of the IUR was constructed as a weighted average of the predicted state rates and was then seasonally adjusted using a least squares adjustment technique. For periods when the national trigger existed as a policy option, then, we simply compared our seasonally adjusted estimate of the national IUR to the required trigger rate. When the national requirement was met in the immediately preceding quarter, every state was assigned an "on" trigger indicator.

Implementation of the 120 percent rule (including the waiver option) and the inclusion of EB recipients in trigger indicators required considerably more programming. In the final version of the model, the 120 percent rule was approximated by using the appropriate predicted quarterly IUR from the two preceeding years. The 120 percent waiver option (at a 1 percentage point higher IUR) was also used for those states with such an option. If the state IUR exceeded the required rate and met the 120 percent rule (or its option), the state was assigned an "on" EB trigger indicator for the next quarter.

Although we examined a number of complex options for including EB recipients in IURs for trigger purposes, endogenous use of EB caseload predictions proved to be too cumbersome and inaccurate for implementation. As an alternative, we opted for the simpler approach of increasing predicted IURs by 0.5 percentage points during periods when EB was in effect in the prior quarter. This increase in the IUR trigger indicator was consistent with our aggregate data on the size of EB relative to UI caseloads, and the correction factor showed relatively little variance both across states and over time (except in transition quarters). Hence, the 0.5 percentage point estimate provided us with a simple, but fairly accurate way of estimating the effects of elimination of EB recipients from the IUR trigger indicator.

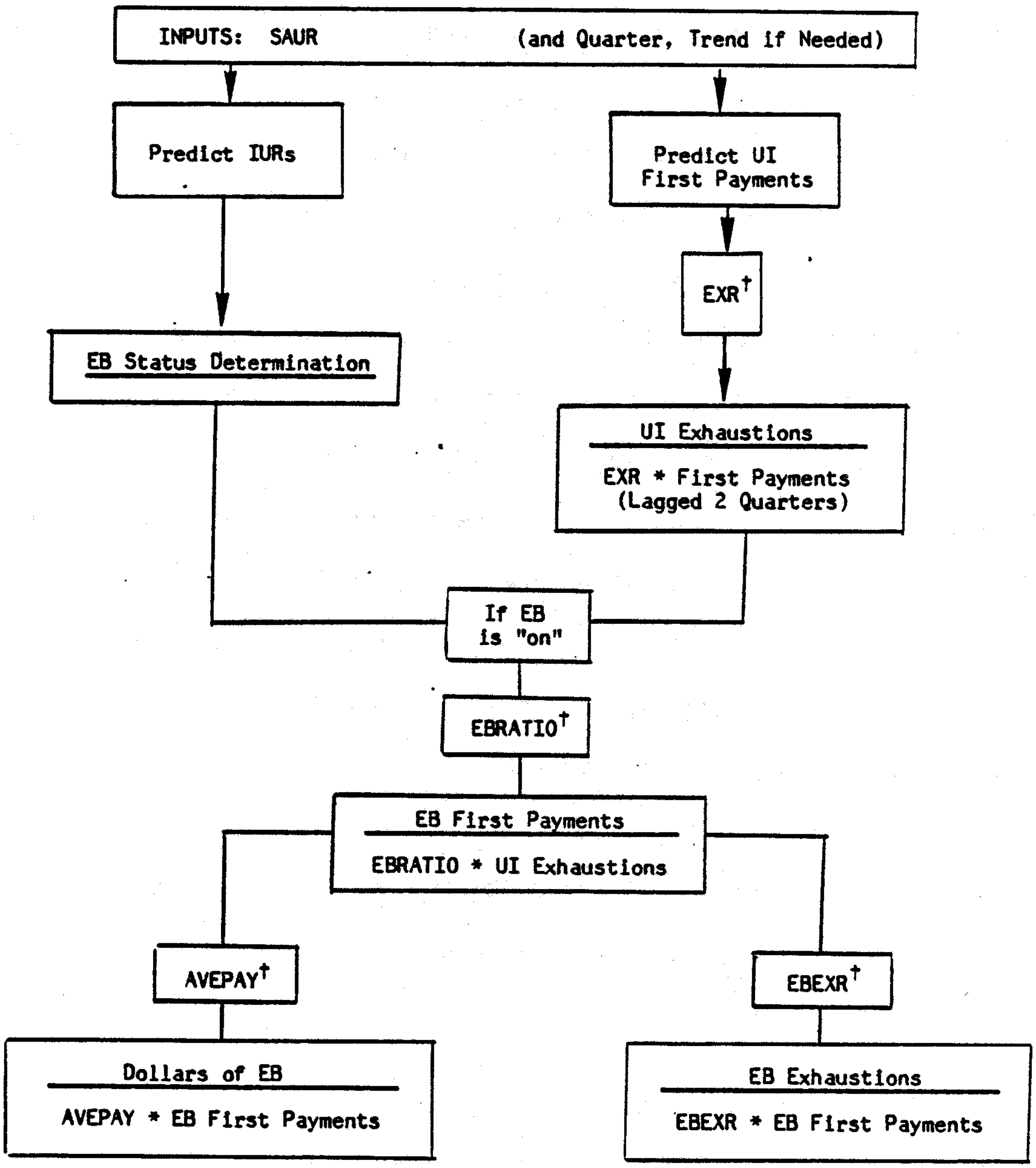
In all then, our development of a trigger link involved a number of assumptions and these may have prevented us from predicting EB status with complete accuracy (for a complete analysis see Chapter VI). But, for historical simulations, we were still able to insert the actual EB status (if desired). And the link provided us with a way of simulating EB performance over hypothetical periods in a way that we believed closely approximated actual trigger operations.

D. SUMMARY OF THE MODEL

A summary of the simulation model is provided by the flowchart in Figure II.1. That chart indicates how the model seeks to represent the actual flow of workers through both regular UI and EB. The model starts with a specification of the SAUR for either an actual or hypothetical

FIGURE II.1

FLOW CHART OF EB MODEL



† Important behavioral parameters to be estimated (see Chapters III and IV).

NOTE: Underlined variables represent principal model outputs. All outputs are provided by state and quarter and are also aggregated nationally.

period. This figure is then used to predict UI first payments and an EB trigger status indicator (which is based on the IUR) for each state. Exhaustion rates for regular UI are then predicted (again using predicted IURs as one determinant) and these are applied to the first payment figures to estimate the number of exhaustions. First payments under EB--a principal output of our model--are then estimated by multiplying UI exhaustions times our EBRATIO prediction for those states in which EB is predicted to be triggered "on." These estimated EB first payments then generate estimates of dollars paid under EB and total EB exhaustions. All of these outputs are provided on a state specific basis, and they are aggregated for the nation as a whole.

In Chapter VI we will provide a detailed discussion of the accuracy with which our model predicted actual EB performance over an historical period as a way of indicating the validity of our estimates of EB program changes. Here we can preface that discussion a bit by briefly pinpointing what appear to be the model's most crucial links with respect to determining its overall predictive accuracy. Most important is our determination of each state's EB status. Making that determination is quite difficult in our model because of the differences in timing between our data (quarterly) and the true EB trigger indicator (weekly). This problem is especially severe during periods of moderately above average unemployment when IURs cluster around the trigger rates specified by law. A partial solution is to use the actual EB trigger status during a particular historical quarter, but even that solution may be inaccurate during transition quarters when EB is partly "on" and partly "off."

A second crucial link in the simulation model is the initial prediction of UI first payments since all other outcomes are based directly on those predictions. Although the equations used for these predictions have high R^2s, standard errors are still relatively large ranging between one-fourth and one-half of mean first payments. Consequently, predictions can be quite inaccurate and, from our modeling experiences that seemed to be especially a problem when the unemployment rate was changing rapidly. An alternative solution would be to use actual first payments during an historical period or to use first payments predictions made by the UI Service for its own purposes. Our model permits such estimates to be inserted directly but, for the most part, we use predictions generated by the equations specified in Table II.2

Use of a predicted exhaustion rate (combined with the first payments figures) provides a final source of potentially major prediction errors in the simulation model. Again, most of the problems with this link of the model relate to matters of timing. In periods when first payments and exhaustion rates are changing rapidly, our quarterly model may miss important turning points or may assign data to the wrong quarter. A solution would be to input total exhaustions directly into the model, but we have not done so in the belief that the model would then lose much of its utility. Instead, we have chosen to aggregate the model's predictions over several quarters (usually over an economic cycle) in the belief that this may mitigate some of the particular timing problems raised by our data.

It should be stressed that, although the three problem areas in our model indicated above may affect its predictive ability for specific time periods, these difficulties may have only minor implications for our ability to estimate effects of the EB changes. That is, there may be only minor interactions between the errors in our predictions and the components of the EB changes we are examining. In that case, comparison of EB simulations made with the policy changes to a "base case" simulation made without the changes will accurately measure the policy effects even though the base case predictions themselves may be quite inaccurate. Such issues of model validity are taken up in detail in Chapter VI where we offer support for our belief that our estimated policy effects are fairly accurate. First, however, we turn to examine the results of our attempts to estimate the various links of the simulation model empirically.

III. EMPIRICAL ESTIMATES USING AGGREGATE DATA

In this chapter we present our aggregate estimates for the determinants of the "behavioral" parameters EXR (regular UI exhaustion rate), EBRATIO (the fraction of UI exhaustees who collect an EB first payment), AVEPAY (the average total EB benefits received by each individual receiving a first payment), and EBEXR (the EB exhaustion rate). The estimates were made using pooled time series-cross section data for 50 UI jurisdictions for the period 1964.3 to 1981.4.[1] Results of these estimates were generally quite statistically significant and they illustrated a number of possible behavioral effects of both the EB program and of more general UI system characteristics. Our discussion of these results is divided into four sections. In Section A we describe the aggregate data used for the analysis. Then, in Section B, we present our econometric estimates for the regular UI exhaustion rate (EXR). This is the most extensive analysis we undertook both because the results were the most significant and because the behavioral effects of EB were most apparent in the regular UI exhaustion data. The three other parameters that apply specifically to the EB program (EBRATIO, AVEPAY, and EBEXR) are investigated in Section C. Although this examination yielded a number of important results, the analysis was not quite so detailed as for the EXR variable.

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[1] UI jurisdictions included 50 states and the District of Columbia. However, the state of New Hampshire was not included in our sample because of significant nonsimilarities between its UI system and those of other states. We therefore used data for 49 states and the District of Columbia. These are sometimes referred to as 50 "states" in the text. Data are also available for 1964.1 and 1964.2, but these were omitted because of the need for a two quarter lag in our specification for the EXR equation (see below).

Finally, in Section D, we present our conclusions and show briefly below how the econometric results were used in the simulation modeling.

A. DESCRIPTION OF THE DATA

Table III.1 describes the basic variables that were used in our analysis together with the mean and standard deviation for each variable across the 3,500 observations (50 states times 70 quarters) in the complete data set. The variables in the table have been grouped into four categories: (1) economic and other background characteristics; (2) regular UI characteristics; (3) extended benefits characteristics; and (4) the four behavioral parameters that provided the dependent variables for our analyses. Because most of these variables will be discussed in detail in following sections, our description of them here will be brief.

The variables in the first section of Table III.1 represent exogenous factors that were expected to affect UI outcomes. Most important is the insured unemployment rate (IUR). That variable provided our primary measure of labor market conditions in each state. Also, as described in the previous chapter, this variable provided our primary way of simulating recessionary circumstances since we predicted its level for each state based on actual and hypothesized patterns in the total national unemployment rate. The other variables in Section 1 of the table (MALE, MANF, and CON) measure characteristics of each state's pool of UI recipients and these were the only such data available for our entire sample.[1] These were used throughout our analysis to control for differences in labor force and employment characteristics among the states.

[1] Data on UI recipients' ages were also available for some periods but were not sufficiently complete to be included in most of our analysis.

TABLE III.1

BASIC VARIABLES USED IN THE ANALYSIS
(3500 observations)

Variable Name	Description	Mean	Standard Deviation
1. Economic and Background Characteristics			
IUR	Insured unemployment rate for second month of quarter.	3.47	2.06
MALE	Percent of UI recipients who are male.	59.34	10.83
MANF	Percent of UI recipients from manufacturing industries.	36.46	16.10
CON	Percent of UI recipients from construction.	18.38	10.02
2. Regular UI Characteristics			
APD	Average potential duration for regular UI (weeks).	23.86	2.69
UNIF	Equals 1 for uniform duration states.	0.143	0.351
DISP	Dispersion of potential durations. = $(\text{maximum duration} - \text{APD})^2$	16.59	26.32
DISNABL	Disqualifications for not able or available for work (per 1000 claimant contacts).	8.14	5.33
DISREF	Disqualifications for refusal of suitable work (per 1000 claimant contacts).	0.78	0.64
WRR	Wage replacement ratio (average UI benefit divided by average weekly wage).	0.345	0.051
3. Extended Benefits Characteristics			
EB	=1 if EB in effect.	0.337	0.473
TCFSB	=1 if benefits available beyond EB (from TC or FSB programs).	0.144	0.351
EBWORKTEST	=1 after April 1981 for change in job suitability definition.	0.043	0.203
4. Basic Dependent Variables			
EXR	Exhaustion rate for regular UI.	0.296	0.142
EBRATIO*	Ratio of UI exhaustees to EB exhaustees first payments.	0.919	0.261
AVEPAY*	Average benefits paid to EB recipients.	704.0	501.2
EBEXR*	EB exhaustion rate.	0.656	0.149

*Samples used for these variables were a subset of the full 3500 observations. See text, Section C.

Variables in Category 2 of Table III.1 measure features of states' UI programs--especially those features that were expected to affect the lengths of UI recipients' unemployment spells (and, hence, their exhaustion rates). Variables measuring the duration of UI benefits (APD and UNIF) and the generosity of UI benefits (WRR) were expected to have positive effects on spell lengths, whereas measures of the degree of enforcement of the UI "work test," DISNABL and DISREF, were expected to have negative effects on those lengths.[1] All of these variables have been found to be significant in other studies related to individuals' unemployment durations (for a summary, see Corson and Nicholson, 1982), and we found that such results continued to hold here.

Means and standard deviations for the basic four behavioral parameters of our simulation model are shown in Part 4 of Table III.1. These show that regular UI exhaustion rates averaged 29.6 percent over our period of observation--a figure consistent with more direct measures of exhaustion. That figure was somewhat above the 25 percent "norm" usually mentioned for the UI exhaustion rate--perhaps because of the presence of several recessions during our sample period. For the three parameters that refer to EB (that is, EBRATIO, AVERAY, and EBEXR), it was necessary to limit the sample to quarters in which EB was in effect and where the transition from an "off" to an "on" status (or vice versa) was unimportant. Only by proceeding in this way could reasonable values for the three variables be obtained. Although the precise details of this

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[1] The variable DISP was created to measure the variance in individuals' potential durations in a state. Although that variable was used in most of our analysis, we will not focus much attention on it.

sample selection are given in Section C, here we can briefly describe the results. Overall, the mean for EBRATIO was 0.92--more than 90 percent of UI exhaustees go on to EB during quarters in which the program is available. Although this mean value was consistent with our data on individuals (a mean value for EBRATIO of 0.88 is reported in Chapter IV), perhaps the most intriguing feature of the statistic was the relatively large standard deviation about this mean. Of course, in part, this variability arose from the inability of our aggregate data adequately to capture the true probability that an exhaustee will collect EB--primarily because quarterly data on exhaustees do not represent exactly the same individuals who collect an EB first payment in the same quarter. Still, we believed that at least part of the variance in EBRATIO was systematic, being influenced by economic conditions and UI system parameters. Hence, an empirical investigation seemed warranted.

For AVEPAY and EBEXR the aggregate mean values were also broadly consistent with the micro data. The $704 mean for AVEPAY implied that the typical EB recipient collected for about 9 weeks ($704 divided by a mean weekly benefit amount in the sample of $78) and that figure compares fairly well to an average of 7.4 weeks for the 12 states in our individual data set. Similarly, our estimated EB exhaustion rate of approximately two-thirds is also consistent with the individual data and with national aggregate data for the 1974-77 period (see Corson and Nicholson 1982, page 75). These variables also continued to exhibit considerable variability, however, so an empirical investigation of their possible determinants seemed warranted.

B. MODELING THE UI EXHAUSTION RATE

Our econometric estimates for the regular UI exhaustion rate were largely based on a model developed elsewhere (Nicholson, 1981). That model used the natural logarithm of EXR as a dependent variable with most of the variables in Sections 1-3 of Table III.1 as independent variables. The model was estimated using pooled data for the period 1965.2 to 1974.4. As a beginning to our examination, Table III.2 reports the result from a similar regression run over all of the observations in our larger data set.[1] In that regression, as for most of the others we ran on the aggregate data, a set of quarterly dummy variables was added to the list of independent variables in order to control for seasonality.

The ordinary least squares results reported in Table III.2 were quite similar to those obtained in the earlier study. All of the coefficients were significantly different from zero at the .05 level with many of those coefficients being more than ten times their respective standard errors. As before, the IUR had a large positive effect on exhaustion rates whereas the effects of average potential duration (APD) and uniform duration policies were negative. The UI enforcement parameters (DISNABL and DISREF) both had significantly negative effects on exhaustion rates and the quantitative size of these effects was quite similar to that found in the earlier study. The wage replacement coefficient in Table III.2 was very significant. This coefficient implied that the effect of each 10 percentage point increase in WRR on the exhaustion rate can be

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[1] In these estimates, the variables MALE, MANF, CON, APD, UNIF, and DIS were lagged two quarters to reflect more accurately the characteristics of claimants exhausting benefits.

TABLE III.2

DETERMINANTS OF REGULAR UI EXHAUSTION RATES:
1964.3 - 1981.4
(Dependent Variable: LNEXR)

Independent Variable	Coefficient	Standard Error
IUR	0.0458*	0.0038
MALE	-0.0061*	0.0008
MANF	-0.0089*	0.0004
CON	-0.0082*	0.0010
APD	-0.0399*	0.0024
UNIF	-0.1412*	0.0185
DISP	0.0011*	0.0002
DISNABL	-0.0089*	0.0012
DISREF	-0.1282*	0.0105
WRR	0.7301*	0.1194
EB	0.1441*	0.0160
TCFSB	0.1032*	0.0199
Q1	-0.0272	0.0180
Q2	0.1878*	0.0164
Q3	-0.4513*	0.0181
Constant	0.2257*	0.0853
R^2	0.605	
F-statistic	356.0	
Degrees of Freedom	(15,3484)	

*Coefficient significantly different from zero at the .05 level on a one-tail test.

ȯfset by an increase in APD of about 1.8 weeks. Looked at in a slightly different way these figures imply that increases in WRR increase the average length of UI recipients' unemployment spells by about this magnitude. Although the 1.8 week figure was close to that obtained in prior research on aggregate data, it exceeded by a fairly large factor the 0.5-1.0 week estimate typically found in individual data sets (see Hamermesh, 1977).

Effects of extended benefits policy were clearly visible in the exhaustion rate equation. Our estimates implied that, other things being equal, exhaustion rates were increased by a factor of 14 percent (that is, they would be, say, 34.2 percent instead of 30 percent) when EB was in effect compared to periods when it was not in effect. Clearly, availability of EB seemed to have provided an incentive to collect regular UI through exhaustion. Availability of further benefits beyond EB (under the TC or FSB programs) seemed to have increased exhaustion rates by an additional 10 percent. We also tested for possible effects of the April 1981 changes in the EB work test, but, contrary to the results to be reported in Section C for the EB program, we found no significant impact on regular UI exhaustion rates.

Although we regarded the regression results of Table III.2 as being very satisfactory for our simulation model, a number of additional runs were made to test for the robustness of the estimates. In Table III.3 we report the effects of running the exhaustion rate regression over two periods 1964.3 - 1974.4 and 1975.1 - 1981.4. Results for these two periods were generally similar to those obtained for the entire period but a few differences might be highlighted. First, the cyclical sensitivity of

TABLE III.3

EXHAUSTION RATE EQUATION FOR TWO PERIODS
(Dependent Variable: LNEXR)

Independent Variable	1964.3 - 1974.4 Coefficient	Standard Error	1975.1-1981.4 Coefficient	Standard Error
IUR	0.0677*	0.0056	0.0208*	0.0048
MALE	-0.0078*	0.0010	-0.0011	0.0012
MANF	-0.0111*	0.0007	-0.0069*	0.0007
CON	-0.0112*	0.0015	-0.0051*	0.0012
APD	-0.0534*	0.0034	-0.0285*	0.0034
UNIF	-0.1397*	0.0263	-0.1441*	0.0242
DISP	0.0023*	0.0003	-0.0004*	0.0003
DISNABL	-0.01182*	0.0017	-0.0040*	0.0017
DISREF	-0.1220*	0.0129	-0.1453*	0.0187
WRR	0.8155*	0.1594	0.6656*	0.1735
EB	0.1449*	0.0276	0.1583*	0.0200
TCFSB	0.0282	0.0473	0.1472*	0.0206
Q1	-0.0473*	0.0247	0.0114	0.0254
Q2	0.2233*	0.0221	0.1469*	0.0225
Q3	-0.4363*	0.0252	-0.4450*	0.0206
CONSTANT	0.7074*	0.1204	-0.3784*	0.1188
R^2	0.625		0.551	
F-statistic	232.0		113.0	
Degrees of Freedom	(15,2084)		(15,1384)	

*Coefficient significantly different from zero at .05 level on one-tail test.

regular UI exhaustion rates seems to have declined between the two periods--the coefficient of the IUR in the latter period was less than one-third of its value in the early period. Second, the relationship between the APD and WRR coefficients implied a somewhat larger UI disincentive in the latter period (nearly 2.6 weeks of extra unemployment for each .10 rise in WRR) than in the earlier period (1.5 weeks). Finally, although the coefficient of the EB variable was roughly the same in the two periods, the coefficient of TCFSB was much larger in the second period. That is, effects of the TC program on regular UI exhaustions in the early period appeared to be much smaller than the effects of the FSB program on exhaustions in the mid-1970s. The longer period for which FSB was available and the longer potential duration of those benefits relative to the TC program make this result plausible. The FSB estimate may have, however, been overstated by uncontrolled effects of the very poor labor market conditions in the 1975-76 period; so the results reported in Table III.3--which imply that availability of FSB had about the same effect on regular UI exhaustion rates as did EB--may be subject to question.

In order to examine this possibility and other issues related to the stability of our estimates, we employed an estimation procedure recently proposed by Hausman and Taylor (1981) to examine pooled time series, cross-section regressions. The results of using this technique for the exhaustion rate regression over our whole period of estimation are reported in Table III.4. On the whole, our results were only slightly changed by this procedure. The coefficient for the IUR was significantly higher under the Hausman-Taylor method and the coefficients for the EB and TCFSB dummies were somewhat lower than our OLS estimates (Table III.2).

TABLE III.4

EXHAUSTION RATE EQUATION USING HAUSMAN-TAYLOR ESTIMATION:
1964.3 - 1981.4
(Dependent Variable: LNEXR)

Independent Variable	Coefficient	Standard Error
IUR	0.0972*	0.0045
MALE	-0.0072*	0.0008
MANF	-0.0066*	0.0006
CON	-0.0086*	0.0009
APD	-0.0282*	0.0037
UNIF	-0.0890*	0.0309
DISP	0.0002	0.0003
DISNABL	-0.0133*	0.0015
DISREF	-0.1071*	0.0114
WRR	0.9096*	0.1679
EB	0.1090*	0.0142
TCFSB	0.0843*	0.0172
Q1	-0.1352*	0.0164
Q2	0.1759*	0.0141
Q3	-0.4192*	0.0167
CONSTANT	-0.2515*	0.1139
R^2	0.715	
F-statistic	574.0	
Degrees of Freedom	(15,3435)	

*Coefficient significantly different from zero at the .05 level on a one-tail test.

Qualitatively, however, there were no major differences. Indeed, results--not reported here--for running Hausman-Taylor estimates over the more recent (1975.1 - 1981.4) period were quite similar to our OLS regression results for the whole period. For that reason, and because integration of the Hausman-Taylor estimates into our simulation model would have been quite difficult, we decided to use the ordinary least squares parameter estimates in Table II.2 for our simulation exercises.

C. RESULTS FOR EB PARAMETERS

In this section we report the results of our econometric examinations of the EB outcomes EBRATIO, AVEPAY, and EBEXR. Before discussing these results we should describe the sample on which they were based. Obviously, it was not possible to include periods for which EB was not in effect in that sample since the basic dependent variables were not defined then. Less obvious, however, are problems posed by transition quarters--that is, by quarters when a state's EB status changes. Some preliminary analysis suggested that inclusion of these quarters in our sample yielded many spurious and inexplicable results, so these quarters were dropped for our subsequent investigations. That is, our final sample included only quarters in which the EB program was in effect, had been in effect in the prior quarter, and continued in effect into the next quarter. Use of this screening resulted in relatively sensible mean values for the main EB parameters (see Table III.1) and regression results which, while not as satisfactory as those for the regular UI exhaustion, nevertheless provided considerable insight into operation of the EB program. In all, our analysis sample had 778 state-quarter observations. Our general strategy was to run regressions on each of the EB outcomes that included all of the independent variables in Table III.1.[1] Since these results were not as statistically significant as were those for the regular UI exhaustion rate we then further modified the specification to include only those independent variables that had statistically significant coefficients and the seasonal dummies. These modified results were then used in the simulation model. For the outcomes EBRATIO, AVEPAY, and EBEXR, results are reported for the complete and modified regressions in Tables III.5 and III.6 respectively. Here we will discuss the results for each outcome separately.

The variable EBRATIO measured the fraction of UI exhaustees who collect an EB first payment. The variables MANF, APD, and TCFSB all had significant positive effects on EBRATIO. The latter two might be interpreted as disincentive effects—especially the TCFSB coefficient which implied that, other things being equal, 3 percent more exhaustees collect EB when further extensions (such as FSB or, in the 1980s, FSC) are available. Perhaps most interesting, the result in Tables III.5 and III.6 imply that EBRATIO fell 10 percent in response to implementation of the new EB work test in April 1981. This figure, together with our more detailed results from the examination of individual data (Chapter IV) was used as part of our simulation modeling of the effects of EB program changes.

Our estimation of EB benefit payments was perhaps the least successful of all our econometric analyses. Our first approach attempted

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[1] The variable DISP was not included, however, because it contained a few extremely large "outlier" values in the sample. A variable was added, however, to reflect the EB work test. That binary variable, EBWORKTEST, took the value 1.0 for the quarters 1981.2, 1981.3, and 1981.4—the three-quarters in our sample which followed the implementation of the new work test rules.

TABLE III.5

REGRESSIONS ON EB OUTCOMES

Independent Variable	EBRATIO Coefficient	EBRATIO Standard Deviation	AVEPAY Coefficient	AVEPAY Standard Deviation	LNEBEXR Coefficient	LNEBEXR Standard Deviation
IUR	-0.0036	0.0050	13.48*	7.49	-0.0158*	0.0048
MALE	-0.0004	0.0015	-3.67	3.06	0.0008	0.0014
MANF	0.0028*	0.0008	1.26	1.53	-0.0001	0.0007
CON	0.0011	0.0014	-3.81	2.74	-0.0042*	0.0014
APD	0.0092*	0.0040	0.23	7.48	0.0096	0.0088
UNIF	0.1374	0.2661	76.58	49.42	-0.1393	0.1258
DISNABL	-0.0012	0.0021	5.54	3.98	-0.0006	0.0021
DISREF	0.0049	0.0211	-45.01	39.47	-0.0494*	0.0213
WRR	-0.3426	0.2829	-454.90	350.41	0.4463*	0.1746
TCFSB	0.0302*	0.0123	42.99	40.36	0.0756*	0.1926
EBWORKTEST	-0.1035*	0.0533	497.21*	105.83	0.0066	0.0504
AWBA	--	--	8.17*	1.42	--	--
Q1	-0.0061	0.0306	-18.25	57.52	0.0132	0.0296
Q2	-0.0179	0.0255	-68.49	48.00	-0.0389	0.0250
Q3	-0.0709*	0.0260	-19.38	48.48	-0.1402*	0.0250
CONSTANT	0.7732*	0.1405	359.41	261.40	-0.5701*	0.1360
R^2	0.091		0.150		0.146	
F-statistic	5.54		8.98		8.44	
Degrees of Freedom	(14,763)		(15,762)		(14,690)	

*Coefficient significantly different from zero at .05 level on a one-tail test.

TABLE III.6

EB OUTCOME REGRESSIONS USED IN SIMULATION MODEL

Independent Variable	EBRATIO Coefficient	EBRATIO Standard Deviation	AVEPAY Coefficient	AVEPAY Standard Deviation	EBEXR Coefficient	EBEXR Standard Deviation
IUR	--	--	13.96*	8.03	-0.0137*	0.0046
MANF	0.0021*	0.0007	--	--	--	--
CON	--	--	--	--	-0.0047	0.0012
APD	0.0147*	0.0033	--	--	--	--
DISREF	--	--	--	--	-0.0618*	0.0185
WRR	--	--	--	--	0.4152*	0.1698
TCFSB	0.0263	0.0192	--	--	0.0640*	0.0189
EBWORKTEST	-0.1057*	0.0529	496.51*	103.11	--	--
AWBA	--	--	7.38*	1.15	--	--
Q1	-0.0137	0.0267	-78.51	52.12	0.071	0.0289
Q2	-0.0196	0.0254	-80.57*	46.91	-0.0372	0.0249
Q3	-0.0765*	0.0253	12.48	46.66	-0.1315*	0.0251
CONSTANT	0.5187*	0.0875	75.60	97.94	-0.3996*	0.0728
R^2	0.053		0.135		0.108	
F-statistic	6.24		20.17		9.36	
Degrees of Freedom	(7,770)		(6,771)		(9,695)	

(Outcome Measure: EBRATIO, AVEPAY, EBEXR)

*Coefficient significantly different from zero at .05 level on a one-tail test.

to calculate the number of weeks of EB benefits collected to use as a dependent variable. But our calculations (which divided total EB benefits by the product of EB first payments times the average weekly benefit amount, AWBA) produced meaningless results. Hence, we decided to analyze weeks of benefit collection with our individual data and instead use AVEPAY in our aggregate analysis. The results for that variable were also largely uninterpretable until we included AWBA as an independent regressor. In those cases (which are reported in Tables III.5 and III.6) the coefficient of AWBA implied that the typical claimant collects 7-8 weeks of EB benefits and that somewhat higher amounts are collected during periods of high unemployment. The large estimated positive effect for the change in the EB work test is problematical. On the one hand, this may have reflected our inability to control adequately for rising benefit levels in our pooled regressions. Or it may have reflected the true effect of more carefully limiting EB to workers in the labor force. In general, we believed that such issues were better examined with our individual data, so we shall delay a more extensive discussion until Chapter IV.

For the EB exhaustion rate (EBEXR) our results were considerably better than were those for AVEPAY. In some ways, these results mirrored those for regular UI exhaustions: enforcement of the work test (as measured by DISREF)[1] had a negative effect on EBEXR and, as before, WRR had a strong positive effect. Also, as for regular UI exhaustions, existence of benefits beyond EB had a positive effect on exhaustions. However, the

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[1] We did have some very partial data on EB enforcement. Specifically, we had information on total EB determinations and denials for some states and for a few quarters in 1980 and 1981. Regressions on these observations did not, however, yield any statistically significant results.

EB exhaustion rate equation did not fit the data as well as did the UI exhaustion rate equation[1] and the IUR had an (unexpected) negative coefficient. From the aggregate data, it appeared that the changed EB work test did not affect EB exhaustion rates, but a significant negative effect was obtained with the individual data. In all, however, the EB exhaustion equation appeared to be suitable for our purposes although in some simulations we preferred to hold the EB exhaustion rate constant at its mean level (approximately 0.67).

D. CONCLUSION

In conclusion then, the regressions reported in Tables III.2 and III.6 provided one primary input into the prediction of EXR, EBRATIO, AVEPAY, and EBEXR in our simulation model. In some cases, however, we believed that it was more appropriate to simulate effects of the EB policy changes using results from our individual data analysis or by simply assuming values for certain key parameters. Hence, these econometric estimates provided only a starting place for our model construction. Details on how all of our analysis was brought together in the construction of the final model are provided in Section A of Chapter VI.

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[1] Because we defined EBEXR as the ratio of final EB payments to first EB payments in a quarter, a lack of correspondence between the actual individuals represented in these two data series probably added substantially to the variance.

IV. ANALYSIS OF INDIVIDUAL DATA

In the previous chapter we used state level data to examine the impacts of the changes in the EB program and to estimate equations describing the flow of claimants through the EB program for use in our simulation model. In this chapter we reexamine these issues using individual claimant data in order to assess the reliability of the results obtained from the state level data. In addition, the individual level data permit us to provide a richer descriptive treatment of the effects of the changes in the EB program, and for the qualifying wage change, the individual level data provide our only source of information concerning the effects of this change in the program.

The remainder of this chapter contains five sections. The first describes the sample used for the analysis and its limitations. The second reports the results of estimating equations describing the probability of EB receipt, the duration of EB receipt, and the probability of EB exhaustion, and compares these estimates to those obtained with the state level aggregate data. The third section then examines the effect of the work test change on the use of the Job Service and the fourth section examines the effects of the new qualifying wage requirements for EB. A final section provides a summary.

A. DESCRIPTION OF THE DATA AND ITS LIMITATIONS

The individual level data used for our analysis come from the CWBH data system, a longitudinal sample of UI claimants collected from fourteen states. For our analysis we selected a subsample of claimants from this system who had exhausted regular UI during a period in which EB was available. We focused on this sample because we were interested in examining the probability of EB collection and subsequent experience with the program. Although individuals who exhaust regular UI during non-EB periods may collect EB if an EB period triggers-on, we restricted our analysis to individuals who could go directly onto EB after exhausting regular UI.[1] In addition to these restrictions, the resulting sample was further subsampled to obtain a manageable sized analysis sample. In selecting this analysis sample we oversampled states with low CWBH sampling rates and undersampled those with high CWBH sampling rates such that the resulting sample was self-weighting.

The sample sizes by state and the months in which regular UI final payments were received are reported in Table IV.1. In all, twelves states were included in the analysis. One of the other states was dropped because it was judged to have severe data problems, and the other CWBH state had not submitted data at the time we received the CWBH extract file used to select our analysis sample. The total analysis sample had 7,600 records. The sample size varied considerably by state for three reasons. First, the states with larger UI caseloads had larger samples because of their greater importance. Second, states that experienced frequent periods of EB availability had larger samples (controlling for state size) than did states with fewer periods of EB. For example, Pennsylvania had a much larger sample than New York for this reason. And, finally, the CWBH data did not cover the same time period in each state. All states except

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[1] We excluded individuals who exhausted regular UI during the last week of an EB period since they could not collect EB during that period.

TABLE IV.1

EB ANALYSIS SAMPLE

State	Sample Size	EB Periods of Sample
Georgia	101	8/80[a]-1/81
Idaho	154	6/81[a]-7/81, 10/81-4/83[b]
Missouri	801	6/80-7/81, 3/82-6/82
Nevada	93	7/80-7/81, 2/82-10/82, 1/82-4/83[b]
New Mexico	6	9/82[a]-11/82
New York	1030	7/80-1/81
North Carolina	688	7/80-1/81, 2/82-10/82, 2/83-4/83[b]
Pennsylvania	3375	3/80[a]-6/81, 1/82-4/83[b]
South Carolina	459	9/80[a]-6/81, 1/82-2/83
Utah	92	3/82-10/82, 1/83-4/83[b]
Washington	648	5/81[a]-4/83[b]
Wisconsin	153	2/82[a]-12/82
Total	7600	

[a] An EB period in the state began prior to this month, but the CWBH sample used for the analysis did not have observations prior to this date.

[b] April '83 is the final month for which data were available.

South Carolina had CWBH data through April 1983, but they had different starting dates.[1] Thus, in some states, data were available for part of 1980 (e.g., Pennsylvania), while in others, the earliest data were for 1981 or 1982 (e.g., Washington and Wisconsin) even though the states had experienced earlier EB periods. We did not restrict the samples to cover a uniform period since we wanted to have some records for EB receipt prior to the EB work test change in April 1981.

The sample described above has some limitations for analysis. The CWBH states were selected for a pilot test of the CWBH data system and not as a national probability sample. Hence, the states, as is apparent, overrepresent some regions of the country and underrepresent others. And, as we stated above, the time period covered for the sample is not uniform across states. For these reasons, we cannot consider our results to represent the national population of potential EB recipients. The sample includes potential EB recipients in six of the states (Georgia, Missouri, Nevada, New York, North Carolina, Pennsylvania) for the March 1980 to April 1983 period. Of the remaining six states, four (Idaho, New Mexico, South Carolina, Utah) are quite small or are missing only a small number of months of data for the 1980-83 time period, and these omissions are not significant. The other two (Washington and Wisconsin) are missing a number of months of potential EB periods during this time period, and thus the overall sample underrepresents these two states for this time period. Despite these limitations, we think that the sample is useful for our analysis in that there are EB periods before and after the work test

[1] South Carolina had data through February, 1983.

change, and there are states represented with differing UI eligibility rules. Moreover, our analysis is mainly intended to provide a comparison to the results obtained with the aggregate data that represents all states.

B. DETERMINANTS OF EB RECEIPT

In this section we report regression results using CWBH data that describe the probability of EB receipt and the duration of EB benefits collected. The impact of the work test change on EB is examined and the results are compared to those presented in Chapter III using state level data.

Three dependent variables were used in this analysis to describe EB experiences. The first, an EB participation dummy variable, described the probability of EB receipt for claimants who exhausted regular UI during periods in which EB was in effect. As is reported in Table IV.2, this variable had a mean of .88 for our sample. In other words, it suggested that 88 percent of all regular UI exhaustees collect EB during EB periods. This number is equivalent in concept to the EBRATIO variable presented in Chapter III which had a slightly higher mean value of .92. The difference between these two measures occurred for several reasons. First, the individual data did not cover all states, and there may be cross state variation which affects the comparison. Second, the aggregate data measure was only an approximation to the true probability because of lags in EB receipt that were not taken account of by this measure. And third, the two measures covered different time periods relative to the EB work test change, and this change appeared to have lowered the probability of EB receipt (see more below). Nevertheless, measures of the probability of EB receipt showed that all regular UI exhaustees do not go on to collect EB

TABLE IV.2

DESCRIPTION OF VARIABLES USED IN THE ANALYSIS

Variables	Mean	Standard Deviation
Dependent Variables		
EB Participation:	0.88	0.33
1=yes		
0=no		
EB Weeks Collected[a]	7.41	3.33
EB Exhaustees:[a]	0.52	0.50
1=yes		
0=no		
Independent Variables		
EB Potential Duration	12.00	2.18
Wage Replacement Rate	0.50	0.41
Work Test Change Dummy:	0.64	0.48
1=after change		
0=before change		
FSC:	0.58	0.49
1=FSC available		
0=FSC not available		
Total Unemployment Rate	9.31	1.78
Manufacturing:	0.38	0.48
1=pre-UI employer in manufacturing		
0=other		
Sex:	0.35	0.48
1=female		
0=male		
Race:	0.21	0.41
1=non-white		
0=white		
Marital Status:	0.54	0.50
1=not married		
0=married		
Spouse Work:	0.24	0.43
1=yes		
0=no or no work		
Age	35.87	13.23
Years Education	11.74	2.32

Sample Size =7076.

[a] Computed over sample that collected EB.

during periods of EB availability. This, of course, is expected since some individuals leave the UI rolls each week as they become reemployed. The typical rate at which individuals leave the rolls each week is, however, somewhat less than the 8-12 percent figure found for the regular UI to EB transition.[1] This larger drop off rate may occur because claimants must come into the UI office to establish EB eligibility as opposed to just sending in a claim card as they do for most regular UI weeks claimed. In addition, the work test requirements of EB also seem to be a factor.

The other two dependent variables used for our analysis are weeks of EB collected and an EB exhaustion dummy variable. The analysis with these variables was done only for individuals who began collecting EB. The means for these variables were 7.4 weeks and 0.52--i.e., a 52 percent exhaustion rate. For the aggregate data set, a dollars collected variable was used for the analysis. Its mean divided by the mean weekly benefit amount implied that about 9 weeks of EB were collected for each first payment. A correspondingly higher exhaustion rate (about two-thirds) was also found using the aggregate data. As we stated above, some differences can be expected to arise between the two data sets because of differences in variable definition and the observation period.

To examine the determinants of each of these variables we used two basic sets of independent variables in our regressions. The first, more limited set, corresponded to the set of variables used in the aggregate data analysis. The second larger set added additional demographic

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[1] The rate of leaving the UI rolls is about 6 percent per week. This rate was computed using a sample of UI recipients from the CWBH sample.

variables. The means and standard deviations of these variables are reported in Table IV.2. Quarterly dummy variables were also used in the analysis. The independent variables included four UI related variables. EB potential duration and the wage replacement rate (the weekly benefit amount divided by the usual weekly wage) were used to account for possible disincentive effects of the EB program. The two additional UI variables were also intended to account for these effects. We expected the work test change variable to show that this stiffening of the EB work test reduced the probability of receipt, EB duration, and the probability of exhaustion. The FSC dummy variable equalled one if the benefit extensions under the FSC program were available, and it was expected that the presence of FSC would increase EB participation and EB duration.

The other variables in the basic model included the total unemployment rate (the aggregate analysis used the IUR), a manufacturing industry dummy to correspond to the aggregate variable on the proportion of insured unemployment in manufacturing, and a sex dummy variable. Additional demographic variables on race, age, marital status, education, and spouse's work status were also examined.

The results of estimating these models for the probability of EB receipt are reported in Table IV.3. The results were the same for the two specifications--i.e., the additional independent variables did not change the estimated effects for the other variables. The estimates showed that longer EB potential durations increased participation, as expected, and that the presence of additional extended benefits (FSC) had a similar positive and statistically significant effect. The wage replacement rate was not statistically significant. The results were generally similar to

TABLE IV.3

DETERMINANTS OF EB PARTICIPATION: REGRESSION RESULTS
(standard error in parentheses)

Independent Variables	Regression 1	Regression 2
EB Potential Duration	0.027* (0.002)	0.025* (0.002)
Wage Replacement Rate	0.009 (0.009)	0.012 (0.009)
Work Test Change Dummy	-0.060* (0.024)	-0.051* (0.024)
FSC	0.088* (0.037)	0.104* (0.037)
Total Unemployment Rate	-0.018 (0.013)	-0.027* (0.013)
Manufacturing Dummy	0.055* (0.008)	0.057* (0.008)
Sex	0.005 (0.008)	0.003 (0.008)
Race	—	-0.050* (0.010)
Marital Status	—	-0.007 (0.009)
Spouse Work	—	0.002 (0.010)
Age	—	0.001* (0.000)
Years Education	—	0.005* (0.002)
Quarter 1	-0.076* (0.014)	-0.072* (0.014)
Quarter 2	-0.033* (0.014)	-0.036* (0.014)
Quarter 3	-0.038* (0.011)	-0.035* (0.011)
Constant	0.713* (0.093)	0.711* (0.096)
R^2 statistic	0.056	0.063
F statistic	42.287	31.850
Degrees of freedom	(10,7066)	(15,7061)

*Coefficient significantly different from zero at .05 level on a one-tail test.

the aggregate data results reported earlier. In particular, the EB potential duration coefficient of .025 was quite similar in magnitude to the aggegate estimate of .009 for average potential duration (APD), since APD was defined for the regular UI program and it was thus approximately twice EB average duration.

The work test change dummy also had the expected negative and significant sign showing that this change in the EB program reduced participation by about 6 percentage points. The aggregate data estimate was higher (about 10 percentage points). Despite this difference in magnitude, the two estimates confirmed our hypothesis that this programmatic change deterred EB participation.

The other independent variables provide further insights into the determinants of EB participation. The unemployment rate had an unexpected negative sign which was significant in one regression for a two tailed test. This perverse result also occurred with the aggregate data, although the estimate was insignificant. The other variables showed that age and education increased EB participation, while non-whites had lower participation rates than whites. Sex, marital status, and the spouse's work status appeared to have no effect, although when we interacted the spouse's work status with the work test change variable we found that the presence of a working spouse reduced the negative impact of the work test change by about half its overall impact.

The regressions for EB weeks collected and EB exhaustion are reported in Tables IV.4 and IV.5. These regressions were similar to those for participation. Potential duration increased weeks collected and reduced exhaustion as it should by definition. The presence of FSC had a

TABLE IV.4

DETERMINANTS OF EB WEEKS COLLECTED: REGRESSION RESULTS
(standard error in parentheses)

Independent Variables	Regression 1	Regression 2
EB Potential Duration	0.323* (0.023)	0.335* (0.023)
Wage Replacement Rate	0.054 (0.460)	0.038 (0.116)
Work Test Change Dummy	-1.540* (0.259)	-1.600* (0.259)
FSC	4.232* (0.400)	4.090* (0.401)
Total Unemployment Rate	-0.890* (0.139)	-0.828* (0.140)
Manufacturing Dummy	0.082 (0.086)	0.109 (0.087)
Sex	0.213* (0.087)	0.226* (0.089)
Race	—	0.396* (0.111)
Marital Status	—	0.203* (0.103)
Spouse Work	—	-0.048 (0.109)
Age	—	-0.006* (0.003)
Years Education	—	0.029 (0.019)
Quarter 1	-0.442* (0.147)	-0.473* (0.147)
Quarter 2	-0.318* (0.143)	-0.298* (0.143)
Quarter 3	0.961* (0.117)	0.929* (0.117)
Constant	10.138* (0.995)	9.292* (1.026)
R^2 statistic	0.098	0.103
F statistic	66.543	46.800
Degrees of freedom	(10,6117)	(15,6112)

*Coefficient significantly different from zero at .05 level on a one-tail test.

TABLE IV.5

DETERMINANTS OF EB EXHAUSTIONS: REGRESSION RESULTS
(standard error in parentheses)

Independent Variables	Regression 1	Regression 2
EB Potential Duration	-0.005* (0.003)	-0.006* (0.003)
Wage Replacement Rate	0.047* (0.017)	0.048* (0.017)
Work Test Change Dummy	-0.454* (0.039)	-0.448* (0.039)
FSC	0.401* (0.060)	0.411* (0.060)
Total Unemployment Rate	0.018 (0.021)	0.013 (0.021)
Manufacturing Dummy	0.042* (0.013)	0.047* (0.013)
Sex	0.003 (0.013)	-0.001 (0.013)
Race	—	-0.018 (0.017)
Marital Status	—	0.032* (0.015)
Spouse Work	—	0.009 (0.017)
Age	—	0.001 (0.001)
Years Education	—	0.005* (0.003)
Quarter 1	-0.096* (0.022)	-0.093* (0.022)
Quarter 2	0.115* (0.021)	0.113* (0.021)
Quarter 3	0.134* (0.018)	0.134* (0.018)
Constant	0.402* (0.149)	0.344* (0.154)
R^2 statistic	0.056	0.058
F statistic	36.531	25.045
Degrees of freedom	(10,6117)	(15,6112)

*Coefficient significantly different from zero at .05 level on a one-tail test.

positive effect which can be interpreted as a disincentive effect. And the change in the EB work test reduced weeks collected and the probability of exhaustion as hypothesized. The magnitude of these latter effects appeared to be quite large and they were highly significant. Our point estimates were that the change in the work test reduced weeks collected by 1.6 and the exhaustion rate by 0.45. The aggregate data results, however, were either of the wrong sign or insignificant.

The evidence obtained in the analysis of individual level data confirmed our earlier findings with the aggregate data that the change in the EB work test had a negative impact on participation in the program among regular UI exhaustees. It also appeared to have substantially reduced benefits collected for those who began collecting EB. We, of course, cannot say whether these impacts occurred because claimants find work more quickly under the new work test or because they find the new search requirements onerous and drop off the UI rolls.

C. USE OF THE JOB SERVICE

The new EB work test requires EB claimants to report job search activity each week and to accept job referrals and job offers at all wage levels above the minimum wage or the weekly benefit amount, whichever is higher. These requirements are more stringent than those generally imposed prior to the change, and it was expected that the new work test would lead to more intensive search activity both by claimants themselves and through the state Job Service. In this section we examine use of the Job Service both before and after the EB work test change to see if increased use occurred.

We examined this issue by selecting samples of EB recipients before and after the work test change from three states included in the CWBH analysis sample described above. We then obtained data on Job Service use from the ESARS data system from these three states, and we constructed several measures describing the use of the Job Service by EB recipients. These measures are reported separately in Table IV.6 for EB recipients who collected benefits before and after the work test change. The data show generally that larger proportions of EB recipients used the Job Service after the work test change than before the change, as we expected, and these recipients received more services. This outcome occurred for most of the use measures for states 1 and 2 and many of the differences were statistically significant. For example, referrals to jobs increased from 4.6 to 6.4 percent of the recipients in state 1 and 5.1 to 10.8 percent in state 2. One exception, however, was for placements which showed no increase after the change, in any state. Results for the third state were not as consistent as those for states 1 and 2. In that state, job referrals were the only use measure to show a significant difference in the expected direction. In that state, overall registration with the Job Service dropped after the work test change and other differences were insignificant. In general, however, the results provide some evidence that the work test change stimulated increased use of the Job Service but not that this use led to increased job placements.

D. IMPACT OF THE UNIFORM QUALIFYING WAGE REQUIREMENT

One of the last EB changes to be instituted was the requirement that all recipients have 20 weeks of work or more in the base period to be eligible for EB benefits. In states that define eligibility in terms of

TABLE IV.6

USE OF THE JOB SERVICE BY EB RECIPIENTS

Service	Percent of EB Recipients Receiving Service					
	Before Work Test Change			After Work Test Change		
	State 1	State 2	State 3	State 1	State 2	State 3
Registered	86.3	88.6	63.5	92.5	92.8	41.6
Referral to a Job	4.6	5.1	3.6	6.4	10.8	5.8
Placement in a Job	1.8	2.7	1.2	1.5	3.3	0.5
Counseling, Testing, Referral to Supportive Services	4.1	6.9	2.5	4.8	11.0	3.9
Local Office Contact	14.6	5.5	4.1	19.9	22.4	4.0
Sample Size	919	959	887	1368	2137	1391

base period earnings that must exceed specified multiples of the high quarter wage (hqw) or the weekly benefit amount (wba), the equivalent requirements were to have a minimum of 1.5 hqw or 40 wba of earnings in the base period. This change became effective at the end of September 1982. It was intended to focus the program on individuals with a relatively strong labor force attachment as evidenced by their base period work history by making individuals with relatively short base period work histories ineligible. In addition, the uniform minimum requirement reduced some of the cross state variation in EB eligibility in which identical individuals might receive benefits in one state but not another. It has been argued that this is a worthwhile objective because the EB program is, in part, federally financed.

In this section we examine the impact of this change on the proportion of regular UI exhaustees who are eligible for EB. We also ask how potential eligibility is affected by differing state laws, and we compare the characteristics of potential eligibles and ineligibles to examine the distributional impact of this program change. Because this change occurred relatively recently, we have not examined its impact on behavior (e.g., are individuals who are ineligible for EB under this law less likely to exhaust regular UI than they were before the change in the law?) Instead, our analysis is purely descriptive because we assume that no change occurs in the pool of UI exhaustees in response to this change.

The percent of regular UI exhaustees who would have been made ineligible by this change in EB qualifying wage requirements are reported in Table IV.7 for the 11 states for which we had enough exhaustees to report the data by state. A twelfth state, New Mexico, is included in the

TABLE IV.7

PERCENT OF UI EXHAUSTEES ELIGIBLE FOR EB
BY QUALIFYING WAGE RULE[a]

State	State Law	Alternative Qualifying Wage Rule: Weekly Benefit Amount	Alternative Qualifying Wage Rule: High Quarter Wage	Alternative Qualifying Wage Rule: Weeks Worked	Applicable State Law
Georgia	1.5 hqw	13.9	0.0	n.a.	0.0
Idaho	1.25 hqw	13.4	11.3	n.a.	11.3
Missouri	30 wba	15.8	7.3	n.a.	7.3
Nevada	1.5 hqw	3.2	0.0	n.a.	0.0
New York	20 wks wk	3.7	n.a.	2.2	2.2
North Carolina	1.5 hqw	5.8	4.0	n.a.	4.0
Pennsylvania	37-40 wba	3.9	2.0	n.a.	2.0
South Carolina	1.5 hqw	3.3	1.1	n.a.	1.1
Utah	19 wks wk	3.3	3.3	1.5	1.5
Washington	680 hours	0.0	3.3	4.9	4.9
Wisconsin	15 wks wk	22.2	n.a.	24.2	24.2
Total[b]		5.6	3.1	4.8	3.5
Sample Size		7536	6357	1865	7487

n.a. = data not available.

[a] Sample is restricted to cases that exhausted regular UI prior to the qualifying wage change.

[b] Total includes New Mexico which had too small a sample to be reported separately.

total figure. The data in the table showed that the overall impact of the new law in states in our sample was small; 3.5 percent would have been ineligible. This percentage, however, varied considerably by state, from zero in states with regular eligibility requirements that are equivalent to the new EB requirement (e.g., Georgia) to 24 percent in Wisconsin, a state that requires 15 weeks of work in the base period for regular UI eligibility. Thus, in order to assess the likely impact of this change over all states, we considered the eligibility requirements of each state. Information on these requirements indicated that almost half of the states have regular UI requirements as stringent as those now used for EB.[1] To determine the likely impact for the remaining states, we used the ineligibility rates in Table IV.7 as a guide to make an estimate for each state. These estimates may have substantial errors because of the small number of states in our sample that use each of the three equivalent methods to determine eligibility and that also use rules less stringent than those for EB. However, no other information was available, and since few states have regular UI eligibility requirements that differ substantially from those for EB, the error over all states is probably small. After making an estimate of the ineligibility rate for each state, we used the resulting estimates in our simulation model to examine the impact of this change. As we report in Chapter VI, the overall impact on EB first payments and costs was relatively modest (approximately 5

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[1] See the information published in Significant Provisions of State Unemployment Insurance Laws, July 4, 1982, Employment and Training Administration, Unemployment Insurance Service, 1982.

percent), because many states have either a very small impact or no impact from this programmatic change.

In addition to the above findings, the data in Table IV.7 also showed that the method used to determine eligibility can have a subtantial impact on the resulting ineligibility rate of the EB qualifying wage rule. For example, Georgia uses a high quarter wage rule and under this rule no one would be ineligible for EB. Yet, under the supposedly equivalent weekly benefit amount rule, 14 percent of UI exhaustees in Georgia would not be eligible for EB. Similar, although smaller differences, existed for some of the other states in our sample. In addition, we found that states whose qualifying wage rule appeared equivalent to the EB rule had some cases who appeared ineligible. For example, North Carolina appeared to have 4 percent ineligible even though they have an eligibility rule of 1.5 hqw. Whether this apparent discrepancy arose because of poor CWBH data or the fact that the North Carolina eligibility rule is not really 1.5 hqw as reported by the UIS, we do not know. Since, however, the apparent discrepancies were small, we have ignored them in our analysis.

A final issue that we examined regarding the change in the EB eligibility requirement was the impact of this change on the characteristics of potential EB recipients. The only statistically significant demographic differences we found were that EB eligibles were more likely to be female than ineligibles and to be older, but the magnitudes of these differences were quite small. Eligibles also collected slightly more EB benefits when compared to individuals who would have been ineligible after the change in the law. This difference was also small and we did not take account of it in the simulation model. In addition, we found that individuals excluded by the qualifying wage change were almost twice as likely to collect UI for more weeks than they worked in the base period than individuals not excluded. Thus, the goal (see Chapter I) of reducing the number of these cases was achieved.

E. SUMMARY

The individual data estimates reported in this chapter generally supported the Chapter III findings concerning the determinants of EB participation, weeks collected, and exhaustion. In particular, the results showed that the change in the EB work test had a negative impact on participation in the program among UI exhaustees. It also appeared to have reduced benefits collected and to have reduced EB exhaustion rates by substantial amounts. And there was some evidence that use of the Job Service but not placements increased among EB recipients.

Our analysis of data on qualifying wages showed that the uniform eligibility rule for EB reduced EB eligibility among UI exhaustees by about 5 percent. The impact of this new rule, however, varied considerably by state from no impact to up to a 24 percent impact in states with relatively generous regular UI eligibility requirements. The form in which the rule was applied (i.e., as a multiple of high quarter wages, the weekly benefit amount or weeks worked) also appeared to affect the impact within an individual state.

V. ADMINISTRATIVE ISSUES

Two of the recent EB changes, the tightened work test and the qualifying wage restrictions, required the states to change their rules and procedures for administering the EB program. These changes had both short and long run implications for the states' allocation of administrative resources, as well as an impact on recipients' eligibility for EB benefits through the change in procedures. To complement the statistical results presented in the two previous chapters and explore the impacts of these two changes further, we interviewed state and local UI administrators in six states and collected information on how procedures changed and on the resources devoted to administration. The states we visited were selected for another project that examined reasons for the wide cross state variation in nonmonetary issue denial rates, and the questions on the EB changes were included in that projects' site visits for efficiency reasons. Because of this selection procedure and because of the relatively small number of states we visited, the findings discussed here should not be viewed as representative of all states' experiences. Rather, they should be viewed as illustrating some of the procedural changes that were made and their likely impact on administrative resource allocation.

We now turn to a discussion of the findings of the site visits. The discussion is presented in two sections, the first on the work test change and the second on the qualifying wage change. The names of the states we visited are not reported to preserve the confidentiality of our respondents.

A. WORK TEST ADMINISTRATIVE ISSUES

The change in the EB work test, which became effective with respect to weeks of unemployment beginning after March 31, 1981 is generally more stringent than the work test provisions for regular UI which applied to EB claimants prior to that date. The work test changes can be described in three parts. First, EB claimants are disqualified for failure to apply for or to accept suitable work. Under this provision suitable work is defined as a minimum wage job or if the weekly benefit amount exceeds the minimum wage, this larger amount is used. The idea behind this provision is that UC recipients should adjust their wage demands downward as unemployment spells lengthen. The only exceptions permitted are if the claimant has a specific starting date for a new job or is expected to find work within four weeks.[1] To be a suitable job offer, it must be in writing unless it is listed with the ES.

The second part of the new EB work test is that claimants are required to actively seek work and provide "tangible evidence" to the state agency that they are seeking work. The intention is that EB claimants, because of their long unemployment duration, should seek work more diligently than regular UI claimants. Federal regulations permit the states to define active search requirements, in general, except that a written report is required as "tangible evidence" and no exceptions are permitted (e.g., for illness, jury duty, etc.) as in some state laws.

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[1] The four week restriction is recommended to the states but it is not an explicit part of the law. Also, state suitable work provisions regarding non-wage issues (safety, health, etc.) continue to apply to EB.

The final part of the work test change is that any disqualification is for the duration of unemployment until the individual works for four weeks and earns four times the weekly benefit amount. This disqualification differs from the regular UI program, since under regular UI programs disqualifications for not searching for work are on a week to week basis until the claimant begins looking for work again. A related requirement is that under the new EB law any claimant who was disqualified under regular UI for voluntary leaving, misconduct, or refusal of suitable work and who was not disqualified for the duration of unemployment is ineligible for EB. This requirement only affects states which do not disqualify claimants for the duration of the spell for these issues. As of September 1983, this was true for only 7 states for voluntary leaving, 17 for misconduct, and 16 for refusal of suitable work.[1]

The changes in EB work test requirements that occurred because of these provisions varied in the six states we visited (see Table V.1). In two of the states, the minimum wage definition of suitable work represented little change from regular UI or from previous EB practice, since in both these states claimants are expected to adjust their wage demands downward as unemployment spells lengthen. In both states, UI administrators thought that for most claimants minimum wage jobs would be considered as suitable work by the time claimants transitioned from regular UI to EB. In the other states, however, the change in the suitable work definition represented a major shift since claimants are not expected to adjust wage

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[1] See the relevant tables in Comparison of State Unemployment Issuance Laws, Unemployment Insurance Service, Employment and Training Administration, U.S. Department of Labor.

TABLE V.1

COMPARISON OF REGULAR UI AND NEW EB WORK TEST REQUIREMENTS

State	Suitable Work	Work Search
1	Suitable work wages are adjusted downwards as regular UI weeks collected increase. EB minimum wage rule is not a major change.	Requires 2 contacts per week on separate days for EB. Regular UI rules are similar, although requirements are slightly more flexible.
2	Suitable work wages not adjusted for regular UI. Permits refusal of jobs not in usual occupation for first four weeks of EB. Then minimum wage rule is applied.	Requires 3 contacts per week on separate days for EB. Regular UI requires 2 contacts.
3	Major shift to EB minimum wage rule. No adjustments in suitable work wages are made for regular UI.	Requires 3 contacts per week on separate days for EB. Regular UI requires 2 contacts.
4	Suitable work wages adjusted downwards for regular UI, so little change for EB rule.	Requires 3 contacts per week on separate days for EB. Regular UI requires 2 contacts.
5	Major shift to EB minimum wage rule. No adjustments in suitable work wage are made for regular UI.	Requires 3 contacts per week on separate days for EB. No requirements for regular UI.
6	Major shift to EB minimum wage rule. No adjustments in suitable work wage are made for regular UI.	Requires 3 contacts per week for EB, although relaxed in some offices to 2 contacts. No requirements for regular UI.

demands as regular UI spells lengthen.[1] Thus, in these states, the change in suitable work definition might have a significant impact on claimant behavior. In one of these states, however, UI administrators thought that the requirement that job offers, to be considered suitable, must be in writing or the job listed with the ES substantially weakened the potential impact of the minimum wage definition.

Work search requirements for EB claimants in the six states also changed as a result of the new EB law as shown in Table V.1. Five of the six states require the claimant to report three employer contacts on three separate days each week to be considered eligible, while the sixth state requires two contacts on separate days. Two of the states also have requirements that the same employer cannot be visited more than once during an EB benefit collection period. In all cases, these rules are more stringent than those for regular UI as intended by the law, although the degree to which they are more stringent varies considerably by state. Four of the states require reporting of employer contacts (usually two per week) for regular UI although more exceptions seem to be permitted in geographic areas with very high unemployment or for occupations which require a different type of work search. In the remaining two states this reporting requirement represents a major shift from regular UI since reporting of search activity is not required at all. Cross state differences were also noted in the extent to which the report forms are checked and reported job contacts are validated, with most states doing very little or no validation.

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[1] One of these states permits refusal of jobs not in the usual occupation for the first four weeks of EB.

Another search related activity that all states reported was the use of eligibility reviews (ERPs) for EB claimants.[1] Relative to regular UI, ERPs are done more frequently for EB claimants. Several states reported doing an ERP at the start of EB and several scheduled further reviews every sixth week. This activity should also represent a more stringent application of work search requirements for EB recipients relative to regular UI and relative to the situation prior to the 1981 change in EB. Under regular UI these reviews are generally held about every 10 to 12 weeks, although there is substantial variation by state.

Finally, one of the states we visited was affected by the requirement to disqualify EB claimants who had been denied regular UI for separation reasons but whose separation disqualification had not been for the duration of unemployment. Because of the relatively small number of states that do not disqualify for the duration, this requirement should have only a small impact.

We also asked the states how implementation of these search requirements had affected their use of administrative resources. Three of the states said that there had been no impact in that the time allocated by the federal government to administer the EB program was sufficient for their procedures. The other states, however, indicated that some resources are diverted from regular UI administration to EB for administering ERPs to EB claimants. This probably arises from the fact that ERP activity is not budgeted as a separate activity but is included in the time allocated to

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[1] Regular UI and EB claimants are periodically called into the UI office for an in-person discussion of their work search activity and prospects for obtaining work. The frequency and the content of these reviews varies substantially by state.

continued claims and the fact that more ERPs are administered per week claimed to EB claimants than to regular UI claimants.

B. QUALIFYING WAGE ADMINISTRATIVE ISSUES

Effective September 25, 1982 EB claimants were required to have at least 20 weeks of full time insured employment (or its equivalent) to be eligible for EB.[1] This standard requirement affected only states for which this requirement was more stringent than the current state law. Administratively, it required the affected states to reprogram their calculation of EB monetary eligibility and benefits. Since the Federal Supplemental Compensation program was implemented at roughly the same time as this EB change and since it adopted the same qualifying requirement, any necessary programming was presumably of use for both programs.

In the states we visited, implementing the qualifying wage change appeared to have only a minor impact on administrative resources. Three of the states reported that no changes were needed since their qualifying wage requirements were equivalent to the EB requirements. Two of the states that did make changes said that the necessary programming was minor and took little time. The final state did report that substantial programming was done and their estimate was that it took six programmers about two months to accomplish the task. This state was also the only state for which the qualifying wage change made a substantial difference in EB eligibility.[2] Given our estimates in the previous chapter that this change

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[1] The equivalencies are 40 times the weekly benefit amount or 1.5 times the high quarter wage in the base period.

[2] This state predicted that 15-20 percent of potential EB eligibles were made ineligible by the change in qualifying wage requirements.

affected a relatively small percentage or none of the potential caseload in most states, we can conclude that this change had only a minor impact administratively.

VI. POLICY SIMULATIONS

In this chapter we report the results of using our simulation model to estimate the effects of recent changes in EB policy. Generally, we found that these effects were quite large, especially those that resulted from changes in EB trigger policy. Before describing these results in detail we first (in Section A) summarize the final form of the simulation model and show how the empirical estimates presented in Chapters III and IV were incorporated in it. Section B then briefly examines the predictive accuracy of the EB model during the 1975-80 period. There we find that the model tracks the data fairly well and hence conclude that the model provides a reasonably accurate way of measuring the effects of EB program changes. Following this discussion of model validity we then turn to examine our results. Section C reports the estimated effects of the EB policy changes for two actual historical periods. For the years 1978-81 we simulate what EB first payments would have been if the 1981-82 program changes had been in effect and show that they would have been much smaller than they actually were. Then we examine the post-change period, 1982.4 to 1983.3, and show that the actual drop in EB first payments was even larger than the substantial decline predicted by our model. After this analysis of actual EB experiences, in Section D we report some of the simulation exercises that we performed for hypothetical economic circumstances. In particular, we focus on how the 81-82 program changes affected the potential operations of the EB program during "mild" and "severe" recessions and whether the changes targeted program benefits to specific phases of the recessions. Finally, in Section E, we summarize all of our simulations and draw some general conclusions about the effects of EB policy.

A. FINAL SPECIFICATION OF THE EB SIMULATION MODEL

In Chapter II we described the general structure of our simulation model and showed how some parts of it (most notably, operation of the EB trigger) operate. Here we briefly report on how the empirical estimates developed in Chapters III and IV were actually incorporated into the model's "EB link" which was used to generate our estimates for EB first payments, EB benefit costs, and EB final payments. An understanding of this process is important for appraising the strengths and weaknesses of the results generated by the model.

The EB link of the simulation model is used for any quarter in which a state's EB trigger indicator is "on." First payments under the EB program are then calculated by multiplying the number of regular UI exhaustees (this number is itself predicted elsewhere in the model) times EBRATIO, the fraction of UI exhaustees who go on to collect EB. Two alternative approaches to the specification of EBRATIO are available in the model: (1) it may be inserted as a fixed number; or (2) it can be estimated by the econometric equation presented in Chapter III. Preliminary analysis with the simulation model suggested that these approaches yielded very similar results so, for simplicity, we adopted the first for the analysis reported here. Specifically, we assumed that EB ratio took its historical mean value (0.92) for periods prior to the change in the EB work test and that, consistent with our empirical estimates, this value declined by 0.06 (to 0.86) for periods when the new job suitability standards were in effect. During appropriate periods, EBRATIO was also adjusted in each state for the new 1982 EB eligibility standards using the state specific ineligibility rates.[1]

EB benefit costs are calculated in the simulation model by multiplying predicted EB first payments times AVEPAY, our estimate of the average dollars in benefits collected by each EB recipient. Again, the simulation model permits this figure to be an input or to be predicted by an econometric equation. Our early simulations found that the econometric equation developed in Chapter III did not predict average EB payments very accurately, so we again opted for using input values for our reported results. Specifically, we assumed (based on our empirical information) that the typical EB recipient collects benefits for 8.5 weeks. For each state, total weeks of benefits paid to EB recipients were then multiplied by the average weekly benefit amount (WBA) to derive total benefit costs. For the present results, the average value of WBA used for this purpose was calculated over the 1977-81 period. Hence, our results reflected payments at approximately 1979 benefit levels. Since no adjustments were made for inflation, the model may not therefore predict nominal payments in years far from 1979 very well. Relative magnitudes should not, however, be affected very much by our decision to use constant dollar amounts. It should also be pointed out that, contrary to the first payments estimates, our calculation of benefit amounts made no allowance for the effects of

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[1] As described in Chapter IV, these ineligibility rates were first calculated for each CWBH state using the CWBH claimant sample. Then, an estimate of the rate was made for each additional state using the estimates obtained for the CWBH states and information on each state's qualifying wage rules.

changes in the EB work test--primarily because we had conflicting empirical estimates of that effect.

The final outcome predicted by the simulation model is the total number of exhaustions of EB. This is calculated by multiplying EB first payments times EBEXR, the EB exhaustion rate. Again, the simulation model permits this value to be input as a parameter or to be estimated econometrically. Although, in this case, our econometric estimates performed quite well, we decided here also to input a fixed parameter for the results to be reported in order to be consistent with the simple approaches taken for EBRATIO and AVEPAY. For this purpose we chose a uniform exhaustion rate of 0.67, a figure consistent both with the data reported in Chapters III and IV and with aggregate historical data on the EB program (see Corson and Nicholson, 1982). This uniform exhaustion rate made, as we did for AVEPAY, no allowance for the effects of the change in the EB work test, again because of conflicting evidence as to its impact.

In all then, the EB link actually used in this chapter to appraise policy effects had a very simple structure. Of course, the model in its full generality permits a much richer incorporation of behavioral effects. For example, use of the econometric equations allows the model to capture effects of changes in the EB work test or of the availability of benefits beyond EB (e.g., FSB or FSC) in several different ways. But for our present purposes we believed it appropriate to utilize a simpler structure in the belief that this would provide a more direct and easily interpreted series of simulation exercises. Because of the structure chosen, we also believe that the simulations of EB first payments provide a better measure of the impact of the EB changes than our simulations of EB benefits and final payments. For this reason, most of our impact estimates are reported solely in terms of EB first payments.

B. VALIDITY OF THE MODEL

Before reporting these simulation exercises we will, in this section, investigate whether our model appears adequately to mirror EB operations. To examine that question we ran the model over the 1975-1980 period and compared its results to actual data for that period. National quarterly aggregate figures are reported in Table VI.1. In addition to reporting EB first and final payments and EB costs the table also reports the number of states that the model predicts to have an EB "on" trigger indicator and compares that figure to the actual number paying EB benefits during the quarter. A cursory comparison of these two series suggested that, although on average predictions of states' EB trigger status were accurate, the estimates were very wide of the mark in a few cases. Such discrepancies were especially pronounced in quarters when EB was on in all states because of the national trigger provision and the simulation model estimated a national IUR that fell below the national trigger rate. In order to assess the importance of the model's trigger predictions for its overall accuracy we simulated the 1975-80 results with both the "full" model and with an abridged version that used states' actual trigger experiences. Results for both of these simulation exercises are reported in Table VI.1. Fortunately, the results are quite similar (in terms of their root mean square prediction errors) since evaluation of the impacts of the changes in trigger policy requires us to predict states' EB status and thus use the full simulation model.

TABLE VI.1

PREDICTIVE ACCURACY OF EB SIMULATIONS

Time Period		First Payments (1,000s)			Benefits ($1,000,000)			Final Payments (1,000s)			Number of States on EB	
		Actual	Full Prediction	EB Status Prediction	Actual	Full Prediction	EB Status Prediction	Actual	Full Prediction	EB Status Prediction	Actual	Predicted
1975	I	612	298	544	$275	$235	$425	203	199	364	51	17
	II	1011	748	748	560	582	582	577	501	501	51	51
	III	1190	676	676	728	527	527	742	453	453	51	51
	IV	959	770	771	689	597	597	816	516	516	51	51
1976	I	901	835	835	587	650	650	796	559	559	51	51
	II	822	855	928	516	665	721	623	573	621	51	41
	III	709	631	631	481	493	493	517	423	423	51	51
	IV	658	646	646	457	502	502	521	433	433	51	51
1977	I	833	764	764	474	596	596	507	512	512	51	51
	II	669	706	861	474	556	670	492	473	577	51	32
	III	513	293	605	334	230	473	311	196	405	51	15
	IV	504	591	591	381	460	460	395	396	396	51	51
1978	I	385	251	653	270	201	510	238	168	437	51	10
	II	302	400	364	207	312	292	141	268	244	13	18
	III	159	93	257	138	78	206	106	63	172	12	7
	IV	55	79	68	54	67	53	49	53	46	4	5
1979	I	65	70	82	43	61	68	30	47	55	6	3
	II	64	280	89	75	222	75	60	187	60	6	14
	III	41	80	68	42	68	57	34	54	46	4	5
	IV	85	49	68	67	42	56	35	33	45	4	4
1980	I	149	72	193	94	63	165	48	48	129	11	3
	II	281	415	329	264	333	276	159	278	220	19	18
	III	735	403	559	558	318	439	265	270	375	51	26
	IV	761	650	650	734	507	507	515	435	435	51	51
Root Mean Square Error		--	179	156	--	107	119	--	117	130	--	--

The estimates in Table VI.1 were used in three ways to appraise the validity of the simulations. First, and most simply, we "eyeballed" the estimates to see whether they followed the actual data fairly closely and whether there were obvious reasons for any large prediction errors observed. In general, this examination suggested the model performed fairly well. Major turning points seemed to have been captured by the model and many of the quarterly estimates were quite close—a fact that is remarkable given the disaggregated, state-by-state nature of the model. The year 1975 appeared to have been the most difficult for the model to predict. The major under-prediction in 1975.1 mainly resulted from the failure to predict implementation of the national EB trigger (by Congressional action) in that quarter and, in the case of benefits, from the fact that EB was in effect for only part of the quarter. The major under-predictions for 1975.2 and 1975.3 were more difficult to explain. These may have in part resulted from our failure to include explicit behavioral effects of the FSB program in this version of the simulation model or they may have reflected the model's inability to capture the very deep recession of those quarters. We did not examine these issues in detail, however. Other than for the 1975 period, then, a cursory examination of the model suggested that it performed fairly well.

As a more formal means of assessing the model's validity, we calculated root mean square errors (RMSE) for each of the predictions listed in Table VI.1. Although these numbers were, in absolute terms rather high, relative to the variation in the actual series themselves they were small. For example, the standard deviation of the first payments series was 352 (thousand), whereas the RMSE from the full simulation model was 179. Indeed, if 1975 is omitted from these calculations the figures become 309 for the actual series and 123 for the full simulation model. For total benefit payments the actual standard deviation for the entire period was 230 (that is, $230,000,000) compared to 107 in the full simulation model. And, for EB exhaustions, the figures were 262 (thousand) and 117, respectively. Hence, the model appeared to be explaining a substantial portion of the variation in the actual data, and this provided some confirmation as to its overall validity.

As a final check on the model's predictive ability we ran simple least square estimates on national aggregate totals for the EB outcome measures using the seasonally adjusted unemployment rate (SAUR) and three seasonal dummies as independent variables. These regressions are reported in Table VI.2. For EB benefits and EB exhaustions the standard error from the regressions for the 1975-80 period exceeded by about 20 percent the RMSE's from the full simulation.[1] For EB first payments, however, the regression standard error was lower. But, as Table VI.2 shows, that resulted primarily from the inclusion of 1975 data in these comparisons. Once the regressions were run over the 1976-80 period the regression standard error for first payments (141) also exceeded the RMSE from the full simulation model for the same period (123) by a substantial margin. Although it might of course have been possible to improve somewhat upon the fit of the regressions in Table VI.2 by using more elaborate

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[1] The regression standard error is in fact an underestimate of the mean square prediction error in a regression equation, so comparison of the simulation model's RMSEs to the standard errors represents a more severe test than would comparing these to a set of regression-generated predictions.

TABLE VI.2

AGGREGATE QUARTERLY REGRESSIONS ON EB OUTCOMES
1975-1980

Independent Variable	First Payments (1,000's)		Benefit Payments ($100,000)		Total Exhaustion (1,000's)	
	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
			1975-80			
SAUR	327.13*	31.66	198.96*	26.07	218.24*	33.60
Q1	31.32	86.74	-79.64	71.44	-55.37	92.08
Q2	10.26	86.63	-54.29	71.36	-53.77	91.97
Q3	30.72	86.66	-31.09	71.38	-74.97	92.00
CONSTANT	-1797.17*	228.88	-1002.42*	188.53	-1146.43*	242.97
R^2	0.85		0.76		0.69	
Standard Error	150		124		159	
			1976-80			
SAUR	351.84*	40.28	242.04*	30:13	255.19*	37.55
Q1	87.07	88.96	-22.25	66.54	44.79	82.93
Q2	19.22	88.88	-28.49	66.48	-4.94	82.85
Q3	-2.31	88.92	-42.52	66.51	-71.71	82.88
CONSTANT	-1960.89*	277.16	1294.23*	207.31	-1418.53*	258.36
R^2	0.84		0.81		0.76	
Standard Error	141		105		131	

*Coefficient significantly different from 0 at .05 level on a one tail test.

specifications, they nevertheless indicated that, by a fairly severe test, the simulation model seemed to perform well.

Our conclusion from these examinations then was that the simulation model appeared to be a reasonably accurate tool with which to assess the EB policy changes. The model is not, of course, a perfect predictor of EB outcomes and there may indeed exist superior methods for estimating EB figures for purposes of budgetary planning. But as a structural model for appraising alternative policy impacts, the model appeared to be quite suitable.

C. ESTIMATED IMPACTS FOR THE 1978-83 PERIOD

In this section we use our simulation model and the pattern of actual unemployment rates for the 1978 to 1983 period to examine the impacts of the recent changes in the EB program. The discussion is organized into two sections. In the first part we use the 1978 to 1981 period, and ask what the impacts of these EB changes would have been if they were in effect over this entire period. This period was chosen because it is within the estimation period of our model, and it included both low and high unemployment periods. Then, in the second section, we examine the last year (1982.4-1983.3) for which unemployment rate data are available. This time period was chosen because it occurred after all the EB changes were implemented, and it thus provided a period in which to compare simulated benefits with actual benefits.

1. Impacts for 1978-81

During the first two years of the 1978-81 period, the U.S. economy experienced relatively low unemployment rates (approximately 6 percent) as economic growth picked up after the recession of the mid-seventies. In the latter two years of this period, the economy worsened and the unemployment rate rose to 7 to 8 percent. This period was then followed by the current recession in which the unemployment rate rose above 10 percent. This pattern of unemployment rates leads us to characterize the 78-79 period as representing a nonrecessionary period and the 80-81 period as representing the beginning phase of a major recession. For this reason, we have reported the results of our simulations separately for these two periods.

The results of our simulations of the EB changes are reported in Table VI.3 for each trigger change, for all trigger changes, and all changes as a group. First payments were used as a measure of the impacts since results for dollars of benefits would be quite similar. Examination of the table leads to several interesting observations. First, the impacts of the individual eligibility changes would have been considerably smaller than those for the trigger changes. Each of these changes would have reduced first payments by about 5 to 6 percent for a combined impact of about 12 percent. In addition, the impact of these changes would have varied little with the time period, since the work test change is not dependent on the time period. Only the eligibility change is affected by time because the impact differs by state and different states are on EB over time.

Second, each of the trigger rate changes would have had substantial impacts on EB first payments during the 1978-81 period if they had been in effect. Removal of the national trigger would have reduced the number of states on EB by about 5 per quarter, thus reducing first payments by about 15 percent. Raising the state trigger from 4 to 5 percent would have also

TABLE VI.3

SIMULATION OF EB CHANGES
1978-81[a]

EB Change	Percent Reduction in Base Case, First Payments			Number of States on EB Per Quarter		
	1978-79	1980-81	Total Period	1978-79	1980-81	Total Period
Base Case, No Change	142,000[b]	530,700[b]	336,300[b]	7.5	34.1	20.8
National Trigger Change	0.0	19.2	15.1	7.5	23.7	15.6
State Trigger Change	67.6	20.2	30.2	3.6	28.6	16.1
IUR Definition Change	25.6	9.6	13.0	5.6	30.0	17.8
All Trigger Changes	67.6	68.6	68.4	3.6	8.9	6.3
Work Test Change	6.5	6.5	6.5	7.5	34.1	20.8
Eligibility Change	5.7	4.9	5.3	7.5	34.1	20.8
All Changes	71.1	72.0	71.8	3.6	8.9	6.3

[a] Unemployment experience prior to 1977 is used to initialize the model. The 1978-79 period was a nonrecessionary period and the 1980-81 period was a period of rising unemployment leading to the current recession.

[b] Quarterly average EB first payments.

reduced the number of states on EB by about 5 per quarter, but it would have had an even greater impact on first payments than removal of the national trigger. Overall, first payments would have been reduced by about 30 percent. This larger impact arises because the state trigger change occurs in states with relatively high unemployment while the national trigger change affects many states with quite low unemployment rates and, hence, low potential caseloads. The change in the IUR definition (i.e., removing EB claimants from the calculation) would have reduced the number of states triggered on by 3 per quarter and first payments by 13 percent.

Third, the impact of these trigger changes would have varied by time period. Removal of the national trigger only would have had an impact in the higher unemployment period because that was the only period in which the national trigger was on. The state trigger change, on the other hand, would have had its largest relative impact in the lower unemployment period because during that period more states had IURs in the 4 to 5 percent range than in the latter time period. In fact, the overall impact of the state trigger change in the early time period would have been quite large (68 percent). The IUR definition change would have had a larger impact in the early period because a number of states were still on EB at the start of this period because the 1975-77 recession did not end until early 1978.

A final observation is that the combined impact of the trigger changes would have been greater, for the entire time period, than the sum of each individual change. This would have occurred because removal of the national trigger would have led to additional situations in which the increase in the state trigger would have triggered states off EB. This result would have occurred only for the 1980-81 period, since the national trigger was not on in the 1978-79 period. During this earlier period the combined impact of the IUR definition change and the state trigger change would have been the same as that for the state trigger increase alone because the same states would have been affected by both changes.

These observations lead to two general conclusions regarding the EB changes. First, these changes potentially reduce EB caseloads and costs substantially. For the 1978-81 period, our estimate was that caseloads and costs would have been reduced by as much as 70 percent. Second, the impacts of the individual changes can vary substantially over time, depending on the level and pattern of unemployment rates. Because the 1978-81 period did not include a complete business cycle, these varying impacts are not clearly shown by the above analysis. To examine this issue further we use hypothetical business cycles in Section D.

2. Impacts for 1982.4 - 1983.3

The 1982.4 - 1983.3 period represents the first full year of experience under the changed EB program--i.e., all changes were instituted by October 1982. A comparison of actual EB experience with our simulated results for this year permits us to assess the extent to which the recent decline in EB claims and costs is attributable to the changes in the program.

The results of this exercise are reported in Table IV.4. The results indicate that our simulation model predicted that there would have been 4,200 thousand EB first payments for 1982.4 - 1983.3 in the absence of program changes. The model also predicted that the EB changes, as a whole, would have reduced first payments by about 25 percent. This reduction was considerably lower than the simulated impacts for 1978-81 because of the

TABLE VI.4

SIMULATION OF EB CHANGES
1982.4 - 1983.3

	EB First Payments (1,000s)	Percent Reduction Relative to Simulation Model Base Case
Simulation Model		
Base Case, No Change	4,191	—
All Trigger Changes	3,584	14.5
All Changes	3,183	24.1
Actual Experience	1,869	55.4

double digit unemployment rate in effect for much of this period. The high national unemployment rate was predicted to lead to state IURs generally above the rates affected by the changes in EB.

The data in the table also show, however, that actual EB first payments were considerably below the amount predicted for all EB changes. Actual first payments were 45 percent of our simulated, no change amount instead of the predicted 76 percent. Thus, the actual reduction in first payments was over twice the expected one. Another way to consider this reduction is to compare actual EB benefits in 1980 (a period before the changes) with those in 1982.4 - 1983.3. Unemployment rates in the latter period were almost 3 points higher than in the earlier period, and our model predicted that first payments would have been twice as high in the latter period with the EB changes and 2.7 times as high without changes. Actual first payments were instead approximately the same in the two periods. Thus, the EB changes alone explain at most half the recent drop in EB benefits and other factors must also contribute substantially to this outcome.

This result, of course, may have occurred solely because the simulation model was a poor predictor of EB, but we do not think this is the case. The simple regression models of EB discussed in Section B predicted even larger EB first payments for the 1982.4 - 1983.3 period than the simulation model, under the no change assumption. A likely factor contributing to our finding is the fact that currently a given total unemployment rate (TUR) appears to be associated with lower IURs than was the case in the past (see Burtless, 1983). The simulation model accounted for this situation to some degree through the time trend used in the state

IUR equations, but these trends probably did not take full account of the recent IUR/TUR shift. The net result of this situation was that the high total unemployment rate of 1982.4 - 1983.3 was associated with a number of state IURs in the 4-5 percent range affected by the EB changes. Thus, while historic experience suggested that the EB changes would have reduced EB first payments by 25 percent given the recent high unemployment rate, the actual impact was considerably larger because of the divergence of total and insured unemployment rates.

The data in Table VI.5 on the actual EB status of states during the 1982.4 - 1983.3 period provide some evidence of this situation. For example, in the first two quarters of this period an additional 10 and 11 states would have been on EB if the trigger rate had remained at 4.0.[1] That is, a number of states had IURs in the critical range despite total unemployment rates of over 10 percent. However, in the latter two quarters fewer states would have been on EB even with the lower trigger rate. In those quarters, the 120 percent criterion would have kept a number of states off EB even if the trigger rate was 4.0 percent.

D. IMPACTS OF EB CHANGES FOR HYPOTHETICAL RECESSIONS

In the previous section we examined the impact of the changes in the EB program using recent unemployment rate patterns. In this section we continue our analysis by examining impacts over two hypothetical recessions, a mild and a severe recession. This analysis shows more

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[1] Some of these states had an IUR greater than 5.0, but were not eligible because the 120 percent rule applies to rates betwen 5.0 and 6.0 under the increased state trigger.

TABLE VI.5

EB STATUS OF STATES UNDER ALTERNATIVE TRIGGER RULES[a]

Time Period[b]	Number of States on EB	Number of States with IUR > 4.0 and Not on EB[c]		
		On if Trigger Was 4.0	On if Trigger Was 4.0 and No 120 Percent Rule	On if No 120 Percent Rule[d]
1982.4	13	10	2	0
1983.1	24	11	4	0
1983.2	21	5	8	1
1983.3	2	2	12	0

[a] All rules assume no national trigger and IUR does not count EB claimants. Puerto Rico and the Virgin Islands are excluded from the figures. The 13 week waiting period is ignored.

[b] The mid-point of each quarter is used to compute the status.

[c] These categories are mutually exclusive.

[d] The state reported in this column had an IUR greater than 6.0, but the state did not waive the 120 percent rule.

clearly than the prior analysis how the changes affect EB caseloads over an entire business cycle.

The two hypothetical recessions were both started with a total unemployment rate of 5.5. For the mild recession this rate was increased by 0.5 percentage points each quarter to a rate of 7.0. This high rate was maintained for one quarter and then the unemployment rate was reduced by 0.25 each quarter until the initial rate of 5.5 was reached. The same pattern was used for the severe recession with the initial rise being 1.0 percentage points each quarter to a high point of 8.5. The unemployment rate was then reduced by 0.5 a quarter to the initial level. Both recessions lasted for 11 quarters. In our tables we have characterized the first three quarters as the "beginning" of the recession when the unemployment rate rises, the next three as the "middle" when the unemployment rate is at its peak, and the last five quarters as the "end" when the unemployment rate falls to its initial level.

Data describing the impact of the EB changes for the mild and severe recessions are reported in Tables VI.6 to VI.8. In the first table, if we examine the all changes category, we can observe that the reduction in EB first payments was greater in percentage terms for the mild recession (74 percent) than for the severe recession (47 percent), although the reduction in both cases was substantial. In both situations the reduction in first payments was lowest during the middle of the recession when unemployment was at its peak. This was particularly true for the severe recession in which the reduction during the middle of the recession was 39 percent and above 60 percent in both the beginning and the end of the recession. This pattern also occurred for the individual trigger changes

TABLE VI.6

PERCENT REDUCTIONS IN EB FIRST PAYMENTS RELATIVE TO BASE CASE

	Recessionary Phase			
	Beginning	Middle	End	Total
Mild Recession				
Base Case, No Change[a]	67.7	380.4	252.2	236.9
National Trigger Change	0.0	0.0	0.0	0.0
State Trigger Change	74.1	65.5	66.1	66.4
IUR Definition Change	0.0	0.0	39.2	19.0
All Trigger Changes	74.1	65.5	75.8	71.1
All Changes	77.3	68.9	78.6	74.2
Severe Recession				
Base Case, No Change[a]	119.8	714.9	417.9	417.6
National Trigger Change	0.0	8.5	4.9	6.3
State Trigger Change	80.9	9.0	24.8	21.8
IUR Definition Change	0.0	0.0	14.9	6.8
All Trigger Changes	80.9	31.3	57.7	47.2
All Changes	83.3	39.4	62.6	46.6

[a] Quarterly average, thousands of first payments.

TABLE VI.7

DISTRIBUTION OF EB FIRST PAYMENTS BY RECESSIONARY PHASE

	Recessionary Phase			
	Beginning	Middle	End	Total
Mild Recession				
Base Case, No Change	7.8%	43.8%	48.4%	100.0%
National Trigger Change	7.8	43.8	48.4	100.0
State Trigger Change	6.0	45.1	48.9	100.0
IUR Definition Change	9.6	54.1	36.3	100.0
All Trigger Changes	7.0	52.4	40.6	100.0
All Changes	6.9	52.9	40.2	100.0
Severe Recession				
Base Case, No Change	7.8%	46.7%	45.5%	100.0%
National Trigger Change	8.3	45.6	46.1	100.0
State Trigger Change	1.9	54.3	43.8	100.0
IUR Definition Change	8.4	50.1	41.5	100.0
All Trigger Changes	2.8	60.8	36.4	100.0
All Changes	2.8	60.7	36.5	100.0

TABLE VI.8

MEAN NUMBER OF STATES ON EB PER QUARTER

	Recessionary Phase			
	Beginning	Middle	End	Total
Mild Recession				
Base Case, No Change	3.6	20.3	14.0	12.9
National Trigger Change	3.6	20.3	14.0	12.9
State Trigger Change	1.3	7.0	4.6	4.7
IUR Definition Change	3.6	20.3	9.2	10.7
All Trigger Changes	1.3	7.0	2.8	3.5
All Changes	1.3	7.0	2.8	3.5
Severe Recession				
Base Case, No Change	7.0	44.3	23.8	24.8
National Trigger Change	7.0	35.7	21.0	21.2
State Trigger Change	1.7	39.3	17.2	11.2
IUR Definition Change	7.0	44.3	20.0	23.1
All Trigger Changes	1.7	23.3	7.8	10.4
All Changes	1.7	23.3	7.8	10.4

but not the eligibility and work test changes. These latter changes, as we observed in the previous section, reduced first payments, after accounting for the trigger changes, by about 12 percent across the board with only slight variation depending on which states were on EB.[1] As measured against the base case, these individual eligibility changes had a larger impact when the reduction in first payments from the trigger changes was small (e.g., the middle of the severe recession).

The data on the individual trigger changes showed that the state trigger change had its largest impact at low unemployment rates when there were many states with IURs in the 4.0 to 5.0 range. For example, the reduction in the beginning of the severe recession was 81 percent from this change alone, while it was 9 percent in the middle of the severe recession. Because unemployment rates were lower in the mild recession, the impact of the state trigger change was larger in all phases of that recession than for the severe recession except for the beginning phase. The national trigger change showed a different pattern. It only had an impact during the middle and end of the severe recession since unemployment rates were never high enough in the mild recession to be affected by this change. Finally, the impact of the IUR definition change only occurred at the end of a recession because states had to have been on EB to be affected. This change in particular had no impact in the heart of a recession.

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[1] The figures reported in Tables VI.6 show these EB changes as the difference between the all change category and the all trigger change category.

A final point that can be made with these simulations is that the cumulative impact of the state and national trigger changes is probably greater than the sum of the individual changes, as we observed for the 1978-81 period. This occurred because when the national trigger was eliminated more states were affected by the state trigger change than when only the state trigger was raised. The combination of the state trigger change and the IUR definition change did not seem to have this effect. Instead, the overall impact was less than the sum of the two independent impacts.

The data in Table VI.7 show the same impacts described above in a slightly different way, by reporting the distribution of benefits across the three phases of the recession. The overall impact of the changes was to concentrate EB benefits during the middle or most severe part of a recession. For example, in a severe recession 47 percent of the first payments occurred in the middle of the recession with no changes while 61 percent occurred in the middle after all changes were instituted. A similar although smaller distributional change occurred for the mild recession.

The final table, Table VI.8, reports how the number of states on EB was affected by the EB changes. The numbers show that the average number of states on EB per quarter was reduced substantially by the changes in the EB program, with this reduction occurring, of course, only for the trigger changes. As above, the reduction was smallest, as a percent of the base case, during the middle of a recession and largest at the beginning and end.

Thus, the overall result of the EB changes was not only to reduce EB claims and costs but to concentrate the reduction in periods with relatively moderate unemployment. That is, periods with very high unemployment rates (e.g., the middle of severe recessions) were the least affected by these changes.

E. SUMMARY AND EVALUATION

Our analysis of the recent changes in the EB program employed a model that simulates quarterly EB caseloads and costs using the national seasonally adjusted unemployment rate as the principal exogenous input. The model permits the simulation of alternative EB policies. Our analysis of the predictive accuracy of the model lead us to conclude that it is a reasonably accurate tool to assess the relative impacts of alternative EB policy changes.

In our analysis of the impacts of recent changes in EB we found that, generally, these effects were quite large, especially those that resulted from changes in EB trigger policy. For example, we estimated that if all changes had been in effect for the 1978-81 period EB caseloads and costs would have been as much as 70 percent lower than they were without any programmatic changes. We also found that the results varied considerably with the unemployment rate. Reductions in EB caseloads and costs arising from the program changes were largest in moderate unemployment periods and considerably less during deep recessionary periods, although reductions of 25 percent or more were still predicted for these periods. The net result of the changes was to concentrate benefits on severe recessionary periods more than was the case under the prior program rules.

Our analysis of the low level of EB caseloads and costs that occurred during 1983, despite high total unemployment rates, suggested that the EB changes alone explained at most half the decline from expected levels. The recent divergence between total and insured unemployment rates appeared to interact with the EB trigger changes to produce the larger than expected decline in EB benefits.

In all then, our simulation analyses suggested that the recent EB policy changes achieved their intended purposes of reducing expenditures under the program and concentrating remaining resources on time periods and locations in which labor markets are weakest. In order to evaluate whether the policy changes went "too far" in this regard, it is first necessary to develop criteria by which the adequacy of the protection that EB provides to workers might be assessed. Although no set of criteria has won widespread acceptance for this purpose, one commonly employed approach focuses on the total exhaustion rate being experienced by unemployed workers under various circumstances (see, for example, Corson and Nicholson, 1982). The view implicit in this criterion is that during normal times UI exhaustion rates average about 25-27 percent and that a primary goal of extended benefits policy is to prevent this rate from rising very much during periods of weak labor markets. In that way, the degree of protection provided to unemployed workers by the UI system as a whole will be about the same at various stages of the business cycle.

In order to examine this simplified "equal protection" criterion we again used our simulation model to predict exhaustion rates under various policy regimes for the periods 1978-81 and 1982.4-1983.3. Results of these exercises are reported in Table VI.9. For 1978-81, the results are

TABLE VI.9

SIMULATED AND ACTUAL EXHAUSTION RATES FOR REGULAR UI AND FOR UI SYSTEM, INCLUDING EB

	Regular UI Exhaustion Rate	Exhaustion Rate Including EB
1978-81		
No Policy Change	30.6	23.8
All Policy Changes[a]	29.0	26.8
1982.4-1983.3		
No Policy Change	37.0	22.8
All Policy Changes[a]	36.2	23.5[b]

[a] The regular UI exhaustion rate falls under the policy change scenario because of the effects of reduced EB availability on UI exhaustions.

[b] This figure would be 30.6 if only EB trigger changes had been simulated (see discussion in the text).

straightforward. Regular UI exhaustion rates during those years averaged 30.6 percent, a figure slightly above the normal range. Under the EB program, as it actually operated, total UI exhaustion rates (that is, the fraction of claimants who went on to exhaust their entire UI and EB entitlements) averaged 23.8 percent. Adoption of the EB policy changes would have raised that rate to 26.8 percent, a figure within the normal range. Hence, in terms of this very specific criterion, the EB policy changes would not have severely compromised the equal protection goal during 1978-81.

Results for simulating 1982.4-1983.3 posed more difficult problems of interpretation, however. During those quarters, regular UI exhaustion rates were high (37 percent) and some type of benefit extensions were clearly mandated. In the absence of any policy change our simulations suggested that the EB program would have clearly met this need by holding the overall exhaustion rate to 22.8 percent. Surprisingly, even with the major restrictions placed on the EB program by the 1980-81 policy changes, our simulations still suggested that overall exhaustion rates were substantially reduced (to 23.5 percent) by the program.[1] Other simulation runs (not reported in Table VI.9) suggested that this overall finding actually masked two divergent trends. Looking only at the recent trigger changes in EB, the simulations suggested that the total exhaustion rate

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[1] These estimates ignore, on the one hand, the fact that actual EB payments were lower than the level predicted by the model, and, thus, overall exhaustion rates may have been higher. On the other hand, a further temporary extension (FSC) was provided during the time period, in part to offset the EB trigger changes. This program had the effect of decreasing overall exhaustion rates, and the net result of these two factors is uncertain. See Corson, 1984 for a discussion of the FSC program.

would have been much higher during 1982.4-1983.3 (about 30 percent) if only trigger policy had been changed. But our simulations suggested that the changes in the EB work test that accompanied the trigger changes also had the effect of reducing overall exhaustion rates by deterring from EB participation a substantial number of individuals who would eventually have exhausted EB. That is, the low simulated exhaustion rate in Table VI.9 comes about because the effects of the trigger changes on raising total exhaustion rates were negated by the reduction in the number of EB recipients.[1] Hence, evaluating whether the EB changes went "too far" during these quarters requires that an explicit judgment be made about whether those individuals who did not collect and exhaust EB under the new statutes but would have done so under the old statutes were especially "deserving" of help. If so, one might conclude, based on the trigger change only simulations, that the recent EB program changes were a bit too severe. But, if policymakers are satisfied that these individuals were rightly deterred from EB collection and exhaustion, then the recent EB changes did not go too far in restricting and refocusing the program. It should be emphasized, of course, that this discussion is based on a rather narrow criterion for EB performance--the overall UI exhaustion rate.

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[1] In making the exhaustion rate calculation, individuals actually barred from EB by the new eligibility restrictions were counted as "exhaustees" so the simulated decline in exhaustions reflected only the more-or-less voluntary deterrence effects of the EB work test change. The effect of the trigger changes was also not solely in one direction. While the reduced availability of EB, occurring because of the trigger changes, raised total exhaustion rates directly, this effect was partially mitigated by reduced disincentive effects. That is, the reduced availability of EB reduced the number of regular UI exhaustees.

Adoption of more broad-based criteria (which might include the overall distributional impact of the EB policy changes) could lead to a different assessment.

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