# TESTING HYPOTHESES ABOUT RENT CONTROLS

## FINAL REPORT & FOLLOW-UP WORK ASSESSMENT

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Final Report

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## RÉSUMÉ

## ESSAI D'HYPOTHÈSES DE CONTRÔLE DES LOYERS

Frank T. Denton, Christine H. Feaver, R. Andrew Muller

A. Leslie Robb et Byron G. Spencer

La présente étude comportait l'essai statistique formel de diverses hypothèses touchant les effets des contrôles sur le marché locatif. Une base de données a été constituée à partir de diverses sources, des modèles économétriques adaptés aux différents types d'hypothèse ont été élaborés et évalués en utilisant des séries chronologiques groupées d'une région métropolitaine ou des séries chronologiques provinciales, et de nombreux d'essais ont été exécutés. Voici les principales conclusions tirées des résultats de l'essai : 1) rien ne prouve que les contrôles influent sur le taux à long terme d'augmentation des loyers, 2) rien ne prouve de façon concluante que les contrôles rendent les mises en chantiers d'immeubles d'appartements sensibles aux taux d'inoccupation ou aux loyers; 3) en général, il semble que les contrôles n'ont pas tendance à réduire les taux d'inoccupation, bien que les preuves soient quelque peu ambiguës à cet égard, 4) les preuves indiquent une tendance à associer les contrôles à des proportions plus élevées de ménages de locataires, 5) rien ne prouve que les contrôles influent sur le taux de conversion des logements individuels en

logements collectifs, et 6) rien ne prouve que les contrôles font augmenter la proportion de logements locatifs occupés qui demandent des réparations importantes.

# ESSAI D'HYPOTHÈSES DE CONTRÔLE DES LOYERS

Résumé

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A. Leslie Robb et Byron G. Spencer

Octobre 1993

Ce projet a été exécuté selon les modalités d'un marché de recherche conclu avec la Société canadienne d'hypothèques et de logement. Il comportait l'essai statistique formel de diverses hypothèses touchant les effets des contrôles des loyers sur le marché locatif. Voici l'énoncé initial de ces hypothèses tel que préparé par la SCHL :

1) Loyers

À long terme, la réglementation des loyers n'influe pas sur les loyers. La réglementation des loyers empêche les loyers d'augmenter de façon marquée en période de croissance économique forte, mais elle empêche également les fortes diminutions de loyers en période de récession.

2) Mises en chantier

Il n'y a pas de différence importante entre les incidences des taux d'inoccupation et des loyers sur l'offre de logements locatifs et les incidences des fluctuations des taux d'inoccupation et des loyers.

3) Taux d'inoccupation

La réglementation des loyers est associée à des taux d'inoccupation plus bas - les autres paramètres étant identiques.

4) Valeurs immobilières

La réglementation des loyers diminue l'intérêt relatif de l'investissement dans le logement locatif. Cette situation est illustrée par le fait que les changements de pourcentage dans les valeurs des immeubles locatifs sont plus petits dans les marchés réglementés que dans les marchés libres.

5) Type d'occupation préféré

La réglementation des loyers réduit les avantages financiers relatifs de la propriété, ce qui accroît la préférence pour la location.

6) Conversion

La réglementation des loyers encourage les conversions de logements locatifs en logements de propriétaires-occupants (en particulier les logements en copropriété).

7) Entretien et réparations

La réglementation des loyers décourage l'entretien et les réparations et réduit les services (par exemple le nettoyage) que les propriétaires-bailleurs fournissent à leurs locataires.

Nous avons entrepris une recherche méticuleuse des données qui conviendraient à l'essai de ces hypothèses. Un problème courant des activités de statistique appliquée est que les données disponibles ne sont pas parfaitement adaptées au travail à accomplir. Le projet actuel ne faisait pas exception. Il n'était pas possible de faire l'essai d'hypothèses sur les valeurs immobilières et une certaine refonte des hypothèses touchant les types d'occupation préférés et les conversions était nécessaire. En outre, il a fallu remplacer les mises en chantier d'appartements et les taux d'inoccupation par les mises en chantier de logements locatifs et les taux d'inoccupation. À part ces changements nécessaires, des essais appropriés des hypothèses proposées par la SCHL ont été conçus et exécutés, et les

résultats ont été interprétés. Malgré les limites de l'information, nous croyons que les résultats des estimations produisent des essais instructifs de la plupart des hypothèses.

Il y avait deux types d'essais. Le premier était basé sur des procédures paramétriques standard, comme on les nomme dans la documentation statistique et économétrique. Le deuxième type était non paramétrique. Les deux types d'essais ont conduit essentiellement aux mêmes conclusions. Il était donc possible d'aborder ces conclusions avec une plus grande confiance qu'on ne l'aurait fait autrement.

Les données utilisées dans l'étude provenaient de diverses sources. Elles comprenaient des séries chronologiques d'une région métropolitaine servant dans certains essais et des séries chronologiques provinciales servant dans d'autres essais. Une enquête méticuleuse sur la réglementation des loyers appliquée dans les dix provinces entre 1971 et 1993 a été un élément importante de l'étude. On a préparé la taxinomie de la réglementation d'après cette enquête. La taxinomie a servi à classer chacune des provinces et des années dans une des trois catégories suivantes : A) pas de réglementation des loyers; B) contrôle des loyers avec examen (obligatoire); C) arbitrage en matière de loyers (volontaire). (La taxinomie fournit également certains détails supplémentaires comme on l'indique dans le rapport principal.) L'analyse subséquente a cherché à déterminer les effets de la réglementation des types B et C sur le taux d'augmentation des loyers, sur l'influence qu'exercent les taux d'inoccupation et les loyers sur les mises en chantiers

d'immeubles d'appartements, sur le niveau des taux d'inoccupation dans le marché locatif, sur la proportion de ménages qui louent leur logement, sur le taux de conversion de logements individuels en logements collectifs et sur la proportion de logements locatifs qui ont besoin de réparations importantes.

La principale conclusion de l'étude est la suivante : il ne semble pas exister de preuve concluante que la réglementation des loyers, telle qu'elle existe dans diverses provinces au Canada du début des années 1970 au début des années 1990, a produit d'importants effets sur les loyers, sur la construction d'immeubles locatifs ou sur les taux d'inoccupation. Voici un énoncé plus détaillé des plus importantes conclusions :

1) Les données que nous avons utilisées et les essais que nous avons exécutés n'apportent pas de preuves indiquant que les contrôles des loyers réduisent le taux d'augmentation des loyers à long terme.

2) Certains indices laissent entendre que les contrôles font augmenter les loyers plus rapidement qu'ils ne le feraient autrement lorsque le marché locatif est faible, surtout lorsque le régime de contrôle est du type B. Si le taux à long terme d'augmentation n'est pas touché, cela signifie qu'en fonction des contrôles, les augmentations de loyer doivent être moins rapides dans les périodes de marché serré. Cependant, rien ne prouve de façon décisive que tel est le cas. Nous avons donc tendance à ne pas tenir compte des effets sur un marché faible et à mettre l'accent sur

l'absence d'effets à long terme. Compte tenu des difficultés pratiques que soulève une définition précise d'un marché faible et d'un marché serré, la dernière conclusion nous semble donc la plus crédible.

3) Rien ne prouve de façon concluante que les mesures de contrôle des loyers atténuent l'effet des taux d'inoccupation ou des loyers sur les mises en chantier de logements locatifs.

4) La preuve statistique formelle indique que les contrôles du type B tendent à être associés à des taux d'inoccupation plus faibles. Toutefois, les contrôles du type C ne produisent pas le même effet, ni une combinaison des types B et C en un régime unique de contrôle des loyers. Compte tenu des difficultés statistiques pratiques rencontrées lorsque l'on cherche à établir la distinction entre les effets d'un type particulier de régime d'une part et les caractéristiques des provinces dans lesquelles le régime est en vigueur d'autre part, nous sommes enclins à mettre l'accent sur l'absence d'effets lorsque la distinction entre B et C est ignorée, et à conclure qu'en général rien ne prouve de façon concluante que les contrôles sont accompagnés de taux d'inoccupation plus faibles.

5) Des indices montrent que les contrôles sont associés à une plus forte proportion de ménages locataires. Cependant, on ne doit pas simplement en conclure que les préférences des ménages pour la location augmentent lorsqu'il existe un contrôle des loyers, bien que cela puisse être le cas; la

proportion de locataires dépend également de la disponibilité de logements locatifs. Le fait est prouvé pour les contrôles du type B et ceux du type B et C combinés, mais pas pour ceux du type C seulement. Compte tenu de notre scepticisme à l'égard de la fiabilité des distinctions entre les deux types, nous mettrons encore davantage l'accent sur la preuve obtenue lorsque les deux types sont combinés.

6) Rien ne prouve que les contrôles des loyers influent sur le taux de conversion de logements individuels en logements collectifs.

7) Rien ne prouve que les contrôles des loyers font augmenter la proportion de logements locatifs occupés qui ont besoin de réparations importantes.

## ABSTRACT

## TESTING HYPOTHESES ABOUT RENT CONTROLS

Frank T. Denton, Christine H. Feaver, R. Andrew Muller,
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This study involved the formal statistical testing of a range of hypotheses about the effects of controls on the rental housing market. A data base was assembled from a variety of sources, econometric models appropriate to different categories of hypotheses were developed and estimated using pooled metropolitan area time series or provincial time series, and a large number of tests were carried out. The principal conclusions drawn from the test results are as follows: (1) there is no evidence that controls influence the long-run rate of increase of rents, (2) there is no convincing evidence that controls affect the responsiveness of apartment unit starts to either vacancy rates or rents, (3) in general it appears that controls do not tend to lower vacancy rates, although there is some ambiguity in the evidence in this regard, (4) the evidence suggests some tendency for controls to be associated with higher proportions of renter households, (5) there is no evidence that controls affect the rate of conversion of single housing units to multiple units, and (6) there is no evidence that controls increase the proportion of occupied rental dwellings in need of major repairs.

# TESTING HYPOTHESES ABOUT RENT CONTROLS

Executive Summary

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October 1993

This project was carried out under the terms of a research contract with Canada Mortgage and Housing Corporation. The project involved the formal statistical testing of a range of hypotheses about the effects of rent controls on the rental housing market. The original statement of the hypotheses, as prepared by CMHC, was as follows:

(1) Rents

Over the long run, rent regulations have no impact on rents. Rent regulations act to restrict rents from sharply increasing during periods of strong economic growth, but also inhibit sharp rent decreases in recessionary periods.

(2) Housing Starts

There are no significant differences in the responsiveness of rental supply with respect to vacancy rates and rents, and with respect to changes in vacancy rates and rents.

(3) Vacancy Rates

Rent regulations are associated with lower vacancy rates, other things equal.

(4) Property Values

Rent regulations decrease the relative attractiveness of investment in rental housing. This is reflected in the fact that percentage changes in rental property values are smaller in regulated markets than in unregulated markets.

(5) Tenure Preferences

Under rent regulations, the relative financial advantages of homeownership are lower. This increases the preference for renting.

(6) Conversions

Rent regulations encourage conversions from rental to owner-occupied housing (particularly condominiums).

(7) Maintenance and Repairs

Rent regulations discourage maintenance and repairs, and reduce the services (e.g., cleaning) landlords provide to tenants.

We undertook a careful search for data that would be suitable for testing these hypotheses. It is a common problem in applied statistical work that the data that are available are not ideally suited to the task at hand. The current project was no exception. It was not possible to test any hypothesis about property values, and some recasting of the hypotheses relating to tenure preferences and conversions was required. In addition, it was necessary to substitute apartment unit starts and vacancy rates for rental unit starts and vacancy rates. Aside from those necessary changes, appropriate tests of the hypotheses proposed by CMHC were designed and executed, and the results interpreted. In spite of the limitations of the data we believe that the results of the estimation provide informative tests of most of the hypotheses.

The tests were of two types. The first was based on standard "parametric" procedures, as they are termed in the statistical and econometric literature. The second type was "nonparametric." The two types of tests led essentially to the same conclusions, thus making it possible to view those conclusions with a greater degree of confidence than would otherwise have been the case.

The data used in the study came from a variety of sources. They included metropolitan area time series for use in some of the tests and provincial time series for use in others. An important element of the study was a careful survey of the rent control regulations that have been in effect in the ten provinces over the period 1971-93. Based on that survey, a taxonomy of control regulations was developed. The taxonomy was used to classify each province/year to one of three categories: (A) no rent regulation; (B) rent control with review (mandatory); and (C) rent arbitration (voluntary). (The taxonomy provided also some additional detail, as described in the main report.) The subsequent analysis then sought to determine the effects of regulations of types B and C on the rate of increase of rents, the responsiveness of starts of apartment units to vacancy rates and rents, the level of vacancy rates in the rental market, the proportion of households that rent, the rates of conversion of single housing units into multiple units, and the proportion of rental units in need of major repairs.

The major conclusion of the study was that there appears to be no convincing evidence that rent regulations, as they existed in various provinces in Canada from the early 1970s through to the early 1990s, had significant effects on rents, on the construction of rental units, or on vacancy rates. A more detailed statement of the more important findings follows:

(1) The data that we have used and the tests that we have carried out provide no evidence to suggest that rent controls reduce the rate of increase of rents in the long run.

(2) There is some evidence to suggest that controls cause rents to rise more rapidly than they would otherwise in periods when the rental market is "soft," especially when the control regime is type B. If the long-run rate of increase is unaffected, that would imply that under controls rent increases must be less rapid in periods of market "tightness." However, there is no significant evidence that that is the case. We are inclined therefore to discount the evidence of "soft" period effects, and emphasize the finding of no long-run effects. Given the practical difficulties of defining market "softness" and "tightness" with precision, the latter seems to us the more credible finding.

(3) There is no convincing evidence that the responsiveness of rental unit starts to vacancy rates or rents is reduced by the imposition of controls.

(4) The formal statistical evidence suggests that type B controls tend to be associated with lower vacancy rates. However, there is no evidence of that for type C controls, or when types B and C are combined into a single rent control regime. Given the practical statistical difficulties in trying to distinguish between the effects of a particular type of regime, on the one hand, and the characteristics of the provinces in which the regime is in force, on the other, we are inclined to emphasize the lack of effects when the distinction between B and C is ignored, and to conclude that overall there is no strong evidence that controls are accompanied by reduced vacancy rates.

(5) There is some evidence that controls are associated with a higher proportion of renter households. However, that should not be taken simply as evidence that household preferences for renting are increased, although that may be the case; the proportion of renters depends also on the availability of rental units. The evidence is present for type B controls and for types B and C combined, but not for type C alone. In light of our scepticism about the reliability of distinctions between the two types we would again attach greater emphasis to the evidence obtained when the two are combined.

(6) There is no evidence that rent controls affect the rate of conversion of single housing units into multiple units.

(7) There is no evidence that rent controls increase the proportion of occupied rental dwellings that are in need of major repairs.

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## 1. INTRODUCTION

This represents the final report on a project to test a series of hypotheses about the effects of rent controls in the provinces of Canada. The project was carried out under contract for the Canada Mortgage and Housing Corporation.

The terms of the contract called for the development of formal statistical testing methods and the application of those methods to such data as were available and relevant. In reporting on what we have done it is necessary to deal with a range of issues relating to statistical or econometric theory and procedure. Large parts of the report therefore are unavoidably quite technical. However, to the extent possible we have tried to provide also some broader discussion and interpretation of results so that persons not interested in technical details can understand the findings of the study.

The outline of the report is as follows. We begin, in section 2, with the seven hypotheses, as stated originally by CMHC, that formed the basis for our study. In section 3 we then provide a restatement of the hypotheses in a form suitable for formal statistical testing, and some elaboration of the original seven statements to identify a number of "sub-hypotheses" contained (explicitly or implicitly) within them. We discuss issues of data availability in section 4, and the implications of the unavailability of certain types of data for the feasibility of testing particular hypotheses. In section 5 we report on a survey of provincial rent controls and a taxonomy of the controls that have been in effect in particular provinces in particular years, going back to 1971. We describe the general approach adopted for test-

ing the hypotheses in sections 6 (parametric testing) and 7 (non-parametric testing), and the specification and estimation of the equations used as a basis for the tests in section 8. There are two particular technical issues requiring separate treatment, namely the classification of particular years according to whether they were years of strong or of weak upward pressure on rents, and the special procedures followed for carrying out tests on long-run parameter estimates; those we deal with in sections 9 and 10. We report and discuss the full range of test results themselves in section 11 and conclude the report with a summary interpretation of the results in section 12.

## 2. CMHC's STATEMENT OF HYPOTHESES

It is convenient to repeat here the seven hypotheses, as stated originally by CMHC. The original hypotheses need some rewording and additional specification in order to convert them into a form in which formal testing procedures could be applied; that we deal with in the next section. But the original statements are the ones that dictate, in general terms, the problems to be addressed. The statements are as follows:

(1) Rents

Over the long run, rent regulations have no impact on rents. Rent regulations act to restrict rents from sharply increasing during periods of strong economic growth, but also inhibit sharp rent decreases in recessionary periods.

(2) Housing Starts

There are no significant differences in the responsiveness of rental supply with respect to vacancy rates and rents, and

with respect to changes in vacancy rates and rents.

(3) Vacancy Rates

Rent regulations are associated with lower vacancy rates, other things equal.

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Rent regulations decrease the relative attractiveness of investment in rental housing. This is reflected in the fact that percentage changes in rental property values are smaller in regulated markets than in unregulated markets.

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Rent regulations encourage conversions from rental to owner-occupied housing (particularly condominiums).

(7) Maintenance and Repairs

Rent regulations discourage maintenance and repairs, and reduce the services (e.g., cleaning) landlords provide to tenants.

## 3. ADAPTATION AND ELABORATION OF THE HYPOTHESES

The hypotheses, as just stated, must be adapted and converted into a more precise form for purposes of statistical testing. The standard testing framework requires the specification of a null hypothesis and an alternative one, against which the null is to be tested. We present, in Table 1, a reworking of the hypotheses to put them into that framework.

There are fourteen pairs of null and alternative hypotheses shown in the table. However, in fact there are three times that number because we identify two types of rent control regimes (labeled B and C), and each pair of hypotheses can be posed for each of the two regimes and for both regimes combined. (Do controls of type B have an effect? Do controls of type C have an effect? If B and C are grouped together, is there an effect associated with the two combined?) Based on Table 1, there are thus forty-two pairs of hypotheses to be considered. Data limitations and technical problems preclude the actual testing of some of the forty-two, as they are stated in the table. On the other hand, we have considered it desirable to extend the range of testing considerably, in some cases, with the net result that the actual number of tests we have carried out is much greater than forty-two. At this point, though, we are concerned only with providing a basic restatement of the original hypotheses in a form that would be suitable for testing if data were available and technical considerations did not impose any limits.

The hypotheses, as stated in Table 1, are generally self-explanatory. However, some supplementary observations may be helpful. With regard to category 1, "rents," there are three pairs of null and alternative hypotheses (or nine, if the separate testing of the control regimes is allowed for). The first, (1a), relates to the long-run rate of increase of rents under rent controls. The alternative in this case is a "one-sided" alternative, namely that rents increase <u>less rapidly</u> under controls. (A two-sided alternative would be simply that the rate of increase is different -- either higher or lower.)

The other two pairs of hypotheses under the heading of "rents" relate to what CMHC referred to originally as "periods of strong or weak economic growth." We have referred instead to "periods of strong or weak upward pressure on rents." The reason for the change of wording is that strength or weakness in the housing market, and more specifically the rental housing market, need not correspond closely in timing with strength or weakness in the general economy. (That is well known, of course, and we found it to be the case in an earlier study for CMHC.) Housing market "cycles" may differ in timing from those in the economy at large, as indicated by the rate of growth of the real gross domestic product or some other measure of the general level of economic activity. We have, therefore, tested hypotheses relating specifically to periods of strength or weakness in the rental market. How we have defined those periods in practice is discussed in section 9.

We have separated the original hypothesis about periods of strength or weakness into two parts. First, there is a joint null hypothesis, (1b), to the effect that rent controls have no effect in either type of period; that should be tested against the alternative that rent increases are lower in periods of strong upward pressure and higher in periods of weak upward (or possibly downward) pressure, as implied by the original CMHC statement. Secondly, there is the possibility that rent increases are unaffected by controls in periods of strong upward pressure, as opposed to the alternative that they are lower in such periods, disregarding any effects in periods of weak upward pressure. That is case (1c); it represents a "simple" hypothesis, as opposed to

the "composite" hypothesis to be tested in case (1b).

There are six pairs of hypotheses under the heading of "housing starts" (eighteen, when the different control regimes are considered). The issue under this heading is whether starts of rental-unit housing respond to vacancy rates differently when there are rent controls than when there are not. CMHC's original statement contemplated differences in the relationships between starts, on the one hand, and vacancy rates and rents, on the other, and between starts and changes in vacancy rates and rents. We have therefore carried out separate tests for the two types of relationship. In both cases we have tested for joint effects (effects of vacancy rates and rents, considered together) and for separate effects (vacancy rates and rents considered independently). Hypotheses (2a), (2b), and (2c) relate to vacancy rate and rent levels; hypotheses (2d), (2e), and (2f) relate to changes in vacancy rates and rents.

There is only one pair of hypotheses specified under each of the remaining five categories (or three pairs, when the different control regimes are taken into account). Under the heading "vacancy rates" the null hypothesis is that the rates are unaffected by regulations, while the alternative is that they are lowered. (This is thus a "one-sided" test; a "two-sided" test would consider merely the possibility that vacancy rates are altered.)

Under the heading "property values," the null hypothesis is that there is no effect on the rate of increase of rental property values. Two alternatives are identified here: one is that rates of increase are lower under regulations (a "one-sided" test); the

other is that rates of increase are different, whether lower or higher (a "two-sided" test). The reason for considering the second alternative is the following. When controls are first introduced, there may be an expectation of a reduced stream of future profits on the part of investors in rental housing, and a corresponding reduction of the present value of the stream, and hence of the price of rental assets. But as time passes, landlords may learn to cope more effectively with the new regulations, and pressure exerted by landlords may lead to some softening of the original rules; hence an increase in expected profits, and hence in the asset price. However, these possibilities are probably of academic interest only, inasmuch as it has not been possible to obtain data that could be used to test the property value hypothesis in any form. (Issues of data availability are discussed in section 4.)

The null hypothesis under the "tenure preferences" heading is that rent regulations have no effect on household preferences for renting, as opposed to owning. Against this we set two alternatives: (1) household preferences for renting are increased by regulations; and (2) preferences for renting are simply altered (increased or decreased). The argument here is that lower rents may encourage households to rent rather than own, but on the other hand the levels of maintenance, repair, and services may be reduced, which would tend to discourage households from renting; in other words, while the rental price is lower, what the buyer (the household) receives in return for that price may be of reduced quality. Unfortunately, no data relating to tenure preferences could be found either, so that no direct tests of this hypotheses

were possible. However, we have carried out tests relating to tenure realizations, i.e., to the actual proportions of dwelling units that are rented. Those tests are discussed in later sections.

With regard to "conversions," the null hypothesis is that rates of conversion of rental to owner units are not affected by rent regulations. Against this there are again two alternatives: (1) that rates of conversion are higher under regulations (as landlords try to escape from a low-profit market); and (2) that rates of conversion are simply different under regulations, as compared with the no-regulation case. The argument in support of alternative (2) is that while regulations may encourage landlords to move out of the rental market, the regulations themselves may include rules that make conversion difficult. (That might be true in Ontario, in particular.) Once again, data restrictions have made it necessary to replace the original hypothesis with a somewhat different, though related one, as described later.

The last category is "maintenance and repairs." Here there is a single null and a single alternative hypothesis. The null is that maintenance, repairs, and the provision of services by landlords are unaffected by rent regulations; the alternative is that they are decreased.

## 4. THE DATA

The data we have used for testing the various hypotheses fall into two broad categories: (1) data relating specifically to the rental housing market, and (2) economic and demographic data of a more general type, representing variables that affect the rental

market but which, for practical purposes, may be regarded as exogenous to it. All data are annual and relate either to provinces or to major metropolitan areas over the twenty-one year period 1971 - 1991, except as noted below. (As detailed later, some hypotheses were tested with provincial data, some with metropolitan area data.) We consider the category (2) data first.

The general economic and demographic variables used in the analysis include population, real personal disposable income, the general price level, and a representative rate of interest.

Population: Census and annual intercensal population series were available on the CANSIM data base at the provincial level. Consistent population series for metropolitan areas were not immediately available. However, we obtained from Statistics Canada partially overlapping series of population totals based on the census metropolitan area definitions at successive quinquennial censuses. These series were spliced so as to obtain consistent totals back to the early 1970s, based on the 1991 area definitions. (Some other minor adjustments were required to bring the pre-1991 figures into line with the 1991 Census definition of population, which included, for the first time, non-permanent residents.) We were able to use the population totals themselves, and also to construct consistent series of population growth rates for use in the subsequent analysis.

Real Personal Disposable Income: Personal disposable income series were available at the provincial level from the Statistics Canada provincial accounts. Based on our previous experience in the use of such series we considered it advisable to adjust them to remove the effects of short-run fluctuations in farm income.

Such fluctuations are related in large measure to farm inventory variations and are particularly significant in the case of the Prairie Provinces; they are likely to have very little to do with the rental housing market, and especially the urban component of that market. Following previous practice, we estimated the short-run fluctuations by taking the residuals from trend equations (third-degree polynomials) fitted to the farm income component of personal income, separately in each of the ten provinces. The residuals were then subtracted from the overall personal disposable income series for each province. The adjusted series so obtained were converted to "real" form using the implicit price deflators for consumer expenditure from the provincial accounts, divided by population to put them into per capita form, and then converted to annual percentage rates of change, as required for the subsequent analyses. No suitable income series were available at the metropolitan area level and it was necessary therefore to use the rates of change of per capita provincial real income as proxies in those analyses that are based on metropolitan areas.

General price level: For analyses at the metropolitan area level we used the city consumer price indexes available from CANSIM. For analyses at the provincial level we used the personal expenditure price deflator series from the provincial accounts.

Interest rate: There are many interest rate series, with varying degrees of relevance to the housing market (on the demand side, the supply side, or both). As a representative series we chose the rate on three-month treasury bills. The treasury bill rate, is, of course, uniform across the country.

We turn now to the data we have used relating specifically to

the housing market. Those data include rental prices, starts of apartment units, apartment vacancy rates, tenure data, housing stocks, conversions, and occupied rental housing units in need of repairs. In all cases it would have been preferable to work with estimates relating only to "for-profit" rental units -- that is, to have excluded social housing. In practice it was not always possible to obtain data on that basis. Instead, an overall measure of rents was used as an indicator of rents in the "for-profit" market and "apartment unit starts" were used to approximate the new construction of for-profit rental units. (Apartment unit starts include not only social housing but also apartment condominium units, some of which would be owner occupied, and exclude rental units in the form of row or detached housing.) While not ideal, the series that we used were the best available. We would expect movements of these series to be highly correlated with the (unavailable) ideal series.

Rental prices: We have used the rental index component of the overall consumer price index. Rental price indexes were available (from CANSIM) for sixteen metropolitan areas back to 1971, in most cases. Exceptions are the series for Charlottetown, which was available only back to 1979, and for Victoria, which was available only back to 1984.

Apartment unit starts: Series of starts of apartment units were available from CANSIM back as far as were required for the analysis at the metropolitan area level.

Apartment vacancy rates: The vacancy rates used relate to apartment buildings with six or more units. The series were available (from CANSIM) at the metropolitan area level, going back as far as

was necessary for our purposes. Vacancy surveys are carried out twice a year; to obtain annual figures we averaged the two rates for each year.

Tenure data: Series of households by tenure status were available from the Statistics Canada Household Facilities and Equipment Survey, which is conducted annually in the month of May. The series were available only at the provincial level; the analysis in which they were used was therefore carried out at that level. The series were obtained by special request from Statistics Canada.

Housing stocks: Annual estimates of housing stocks were available from CANSIM, but at the provincial level only. Again the analysis in which those series were required was therefore carried out at that level.

Conversions: Series of annual numbers of conversions from single-unit to multiple-unit dwellings were obtained from Statistics Canada, by special request. They were available only at the provincial level. Series of conversions from multiple-unit to single-unit dwellings (which relate more closely to the hypothesis to be tested) were not available.

Housing in need of repairs: Occupied dwelling units are classified in the Household Facilities and Equipment Survey by tenure, and according to whether they are in need of major repairs, minor repairs, or no repairs. For our purposes we chose to work with occupied rental units in need of major repairs as a proportion of total rental units. The data for constructing such series were available from the Survey at the provincial level for the years 1982, 1985, 1987, 1988, 1989, 1990, 1991, and 1992. We required

continuous annual series within each province, and therefore used only the 1987-92 observations.

The housing market series that we have used, as described above, do not always match those that the statements of hypotheses in section 3 call for. Some further comments are therefore in order.

We inquired rather broadly to determine whether there were series of rental property values that could be used for our purposes. In that regard we contacted CMHC, Statistics Canada, Royal LePage, Knowlton Realty, the Appraisal Institute of Canada, and Moore Data Management Services. Unfortunately none of those agencies had any suitable data or were able to suggest further avenues of inquiry that might have led us to suitable data. We were therefore forced to conclude that testing hypotheses about property values was not feasible.

With regard to tenure preferences, we attempted to find appropriate attitudinal data, as called for by the statement of hypotheses in Table 1, but again we were unsuccessful. There was some possibility that data from the Environics "home survey" (some 4000 households across the country) would be helpful. The data that we were interested in were not generally available, but could be purchased. However, we concluded that they would not, in fact, be useful for our purposes. As a feasible (but clearly second-best) alternative we elected to work with actual renter proportions, as noted above. Those proportions may be regarded as market <u>realizations</u>, as distinguished from household <u>preferences</u>. They thus represent the result of factors on the supply (housing availability) side of the rental market, as well as preferences on

the demand side. We have not been able to test any preference hypotheses directly.

## 5. SURVEY OF RENT CONTROLS

Dr. Muller, a member of the research team, has updated the survey of provincial rent controls that he carried out initially a few years ago. He has used information from a number of sources including, in the majority of cases, information obtained by direct telephone contact. (The list of contact persons, with addresses and telephone numbers, is provided in Appendix I.) The results of his survey are summarized in tabular form in Tables 2-11. (Table 2 relates to Newfoundland, Table 3 to Prince Edward Island, etc.)

The rent regulation survey covers the period back to 1971. For purposes of testing the various hypotheses it is necessary to code the information for each province in each year so that the province/year data can be sorted into three rent control categories, or regimes. The first, which we label regime A, is "no rent regulation". The second, regime B, is "rent control with (mandatory) review." The third, regime C, is "(voluntary) rent arbitration." We use these letter codes, and append to them numeric codes to indicate whether a regime is "new" (meaning less than two years since its initiation) and to indicate also any special features of the regime that may be relevant to the selection of data or interpretation of statistical findings.

The results of the coding are shown in Table 12, a year-by-province matrix in which each cell has a three-character or four-character code. The first character indicates the regime in

effect at the start of the year, the second indicates the regime at the end of the year. The third character of each code is either a 0 or a 1, 0 indicating no change in type of regime and 1 indicating a change within the current or the previous year. (The purpose here was to distinguish recently instituted regimes from those that had been in effect for a longer period, in case we should wish to drop those observations flagged with a 1 from the analysis to avoid start-up effects. That issue is discussed later.) The fourth character, which may or may not be included in the code, is the special-features indicator; it takes on values from 1 to 8. More detail about the coding is provided in the notes to the table.

The distinction between rent controls of types B and C is maintained throughout the statistical analysis reported below. However, it is should be recognized that there can be considerable differences in the details and practical applications of controls within each type of regime, either from period to period or from province to province. Indeed, the variation within a given type may be as great as the variation between types. That is a point that we shall have in mind when we interpret the results of the statistical analysis. To anticipate our view, as expressed later, we shall attach greater credibility to the test results obtained when B and C are combined into a single rent control regime than to the results for each regime considered separately.

## 6. TESTING THE HYPOTHESES: A PARAMETRIC APPROACH

Our testing of the various hypotheses is based on two quite different approaches -- a standard parametric approach, derived

from normal error distribution theory, and a nonparametric approach, based on the theory of randomization. We discuss the parametric approach in this section and the nonparametric approach in the next one.

The construction of a detailed econometric model of the rental housing market in each province or metropolitan area was beyond the scope of the present project, and in any event unnecessary for our purposes. Instead we proceeded as follows. Any structural model of a rental market would incorporate a number of endogenous variables (starts of rental housing units, rents, vacancy rates, etc.) and also a number of exogenous variables. If the model were cast into "reduced form," the endogenous variables would become functions of the exogenous variables and of whatever lagged endogenous and exogenous variables the structural model might contain.

Let $R_t$ denote the level of an appropriate rental price index in year t and let $r_t = (R_t - R_{t-1})/R_{t-1}$ denote the proportionate change in the index. Also, let $x_t$ stand for an appropriate vector of exogenous variables. Assuming (as we shall in practice) that lags up to two years are sufficient, we may write the reduced form equation for $r_t$ as

$$(1) \quad r_t = g\left(x_t, x_{t-1}, x_{t-2}\right) + \varepsilon_t$$

where $\varepsilon$ is a random error term and g is a functional operator.

Equation (1) may be regarded as the reduced form equation for determining the rate of increase of rents <u>in the absence of any rent controls</u> (i.e., in a regime of type A). We shall assume the

functional form to be linear, and hence write

$$(2) \quad r_t = \alpha + \beta_0' x_t + \beta_1' x_{t-1} + \beta_2' x_{t-2} + \varepsilon_t$$

where $\alpha$ is an intercept constant, $\beta_0$, $\beta_1$, and $\beta_2$ are column vectors of parameters (the prime indicating transposition), and the x-vectors are taken to be column vectors also. To capture possible effects of rent controls, equation (2) may be rewritten to include shift (or binary dummy) variables, as follows:

$$(3) \quad r_t = \alpha + \beta_0' x_t + \beta_1' x_{t-1} + \beta_2' x_{t-2} + \phi B_t + \theta C_t + \varepsilon_t$$

$B_t$ is a variable with value 1 if the rent control regime is of type B in year t and value 0 otherwise; similarly, $C_t$ is a variable with value 1 if the regime is of type C in year t and 0 otherwise. $\phi$ and $\theta$ are the corresponding (scalar) coefficients. Aside from other influences, rents are undoubtedly serially correlated. To allow for that, and for lagged responses generally, we modify equation (3) to incorporate terms involving $r_{t-1}$ and $r_{t-2}$:

$$(4) \quad r_t = \alpha + \beta_0' x_t + \beta_1' x_{t-1} + \beta_2' x_{t-2} + \gamma_1 r_{t-1} + \gamma_2 r_{t-2} + \phi B_t + \theta C_t + \varepsilon_t$$

If equation (4) is fitted by least squares and $\varepsilon$ is distributed normally, independently of the explanatory variables, serially independently, and with mean zero and constant variance, then standard t-tests and F-tests can be used as exact tests of restrictions on the $\phi$ and $\theta$ coefficients. (Strictly speaking, the

statistics used in the tests do not have exactly the t and F distributions, owing to the presence of the lagged r values. However, that is a minor technical point; the t and F tests are standard procedure with an equation of the form of equation (4).) Restrictions of interest would be the following: (1) $\phi = 0$ (the rate of increase of rents is unaffected by controls of type B); (2) $\theta = 0$ (the rate of increase is unaffected by controls of type C); and (3) $\phi = \theta = 0$ (the rate of increase is unaffected by controls of type B or type C).

We now take the specification of the reduced form equation a step further. Let the data period be broken (exhaustively) into subperiods of "strong" and "weak" upward pressure on rents. Using the superscript "+" to indicate "strong" and "-" to indicate "weak," we break each of the B and C variables into two separate binary (zero-one) components. Thus,

$$(5) \quad B_t = B_t^+ + B_t^-; \quad C_t = C_t^+ + C_t^-$$

Allowing also for different effects of rent controls in the two types of period, we have (in more compact notation)

$$(6) \quad r_t = \alpha + \sum_{k=0}^{2} \beta_k' x_{t-k} + \gamma_1 r_{t-1} + \gamma_2 r_{t-2} + \phi_1 B_t^+ + \phi_2 B_t^- + \theta_1 C_t^+ + \theta_2 C_t^- + \varepsilon_t$$

Again, tests of various restrictions on the (enlarged) set of $\phi$ and $\theta$ coefficients can be carried out: $\phi_1 = 0$ (controls of type B have no effect on the rate of increase of rents in periods of strong upward pressure); $\phi_1 = \phi_2 = 0$ (controls of type B have no effect in periods of either strong upward pressure or weak upward

pressure); and so on.

An equation of the form of (6) is fitted to pooled metropolitan area time series data. (The time series data are "pooled" in the sense that the annual observations for all the metropolitan areas or all provinces are combined for purposes of equation estimation. The term is commonly used in econometrics.) The data are sorted into control regimes in accordance with the coding scheme employed in Table 12. They are sorted also into periods of strong and weak upward pressure, as described in section 9. To recognize the pooled nature of the data we add the subscript i to indicate the $i^{th}$ geographic area, and rewrite equation (6) as

$$(7)\quad r_{it} = \alpha + \sum_{k=0}^{2} \beta_k' x_{i,t-k} + \gamma_1 r_{i,t-1} + \gamma_2 r_{i,t-2} + \phi_1 B_{it}^{+} + \phi_2 B_{it}^{-} + \theta_1 C_{it}^{+} + \theta_2 C_{it}^{-} + \varepsilon_{it}$$

Our choice of variables to be regarded as exogenous (and relevant) to the rental housing market (the x-variables) reflects a conception of the operation of that market. In particular, we would expect demand to be affected by the incomes of consumers, by their numbers, and by the cost of renting relative to owning; we would expect supply to be affected by the expected profitability of investment in the provision of rental housing relative to alternative investments. The exogenous variables chosen for inclusion in the estimation of the reduced form equations reflect these considerations. More specifically, the variables regarded as exogenous are the following:

(1) the rate of population growth;

(2) the rate of growth of real personal disposable income

per capita;

(3) the rate of inflation;

(4) the rate of interest on three-month treasury bills.

Allowing for lags, there are thus twelve x-variables in equation (7). Note that it is not, in general, possible to attach signs to the coefficients of those variables as they represent the net effects of supply and demand factors operating within a (implicit) structural model. Note too that the x-variables may be highly correlated among themselves -- especially with their own lagged values. However, none of that is of any consequence for our purposes; we are not interested in the effects of the x-variables, as such, but merely with controlling for those effects so that we can identify the effects (if any) of the rent control variables. It is strictly the effects of the latter variables that are of interest in the present study.

What we have just described relates to the testing of hypotheses under the heading of "rents." We have given a somewhat detailed description in order to indicate a general approach that we have adopted. Adaptations and extensions of this approach to accommodate the special requirements of particular categories of hypotheses are discussed later.

The hypotheses relating to housing starts, as modified in light of data availability, call for an investigation of the relationships between starts of apartment units, on the one hand, and either the levels of vacancy rates and rents, or the changes in those variables, on the other. A complication introduced here is that vacancy rates and rents cannot be regarded as exogenous. We therefore use an instrumental variables approach to estimate

the vacancy rate and rent coefficients, the instruments being the same set of current and lagged exogenous and lagged endogenous variables as were used in equation (7). (This is equivalent to a two-stage least squares procedure in the context of a structural model.) Similar test procedures are used. (The Wald statistic replaces the F statistic, but that is only a minor change.)

For tests under the heading "vacancy rates" we use a reduced form procedure similar to the one described for the "rents" hypotheses. Differences between periods of "strong" and "weak" pressure are ignored but otherwise the tests proceed in the same way as before (i.e., tests of the coefficients of the B and C variables).

With regard to property values, as noted previously, no suitable data exist, and hence there is no formal hypothesis testing. With regard to tenure and conversions, the reduced form approach is again used, but with data relating to provinces rather than metropolitan areas. It is used also in the tests relating to the need for major repairs, although in that case the time series are so short that some modification is required. Provincial data are used here too.

## 7. TESTING THE HYPOTHESES: A NONPARAMETRIC APPROACH

The parametric approach discussed in the previous section requires the assumption of errors that are (at least approximately) normally distributed. That approach is standard; the tests are familiar to anyone with training in statistics or econometrics. However, the conclusions reached are conditional on the normality assumption, and may not be correct if that assumption is

violated. An alternative approach is based on what may be termed "randomization" theory. Tests based on randomization have the advantage of being "nonparametric;" that is to say, they do not depend on the assumption of a normal or other particular probability distribution for the error terms in the equations. They also offer the further major advantage of being very flexible with regard to their design.

Randomization tests have been recognized in the statistical literature for a long time but they have received rather little attention in the econometrics literature. There was an important (for our purposes) article by Freedman and Lane in the Journal of Business and Economic Statistics in 1983 having to do with randomization tests in the regression context. However, it is only recently that a paper has been available in which the potential for randomization tests is considered in greater detail from the point of view of econometrics; that paper (as yet unpublished) is by Kennedy (1992, revised version). General references on randomization testing (mostly in other fields) include Edgington (1987), Noreen (1989), and Manly (1991), as well as others cited in the Kennedy paper. We shall not attempt here to describe the theory underlying randomization testing in any detail but rather indicate how it will be applied in the present project, with reference to equation (7).

Suppose that the null hypothesis to be tested is that some of the coefficients of all the B and C variables in equation (7) are zero, implying that the dependent variable is unaffected by the imposition of rent controls, or that the effect of such controls is the same in periods of "strong" upward pressure as in periods

of "weak" upward pressure. The equation can be fitted under the null hypothesis by imposing the coefficient restrictions associated with that hypothesis. (It can be fitted by restricted least squares or some equivalent procedure.) If the null is true, the residuals ($\hat{\varepsilon}_t$) from the fitted equation can be regarded as estimators of the true errors ($\varepsilon_t$). Following Freedman and Lane, the residuals are permuted, and added to the fitted values of the dependent variable ($\hat{r}_t$), to obtain a new set of "pseudo-observations" on the latter variable. (Under the null, the pseudo-observations can be thought of as values that could have arisen had the independent random errors been distributed differently across the dependent variable observations.) The pseudo-observations are then used as the dependent variable in a new regression of the form of equation (7); only the values of the dependent variable are different from their original values. A different random permutation of the residuals yields a different set of pseudo-observations on the dependent variable, and hence a different set of coefficient estimates when equation (7) is refitted. In principle, there are as many possible estimates of equation (7) as there are permutations of the residuals. However, because that number is apt to be very large, the usual procedure is to sample the possible permutations -- to refit equation (7) one thousand times, for example, based on one thousand random permutations out of the total possible number of permutations.

An appropriate statistic is chosen for testing a given hypothesis, and that statistic is calculated for each of the (say) one thousand sets of regression results. The statistic need not be a t, F, or other "standard" type of statistic, although that is a

possibility; it could be, for example, the value of an estimated coefficient of interest, the absolute value of the sum of two coefficients, and so on. Whatever the statistic, a thousand values are obtained, thus providing an estimate of the statistic's "probability" distribution. The statistic calculated from the regression using the actual (i.e., unpermuted) observations is then compared with this distribution to see whether it is more or less in the central region of the distribution, or far out on one of the tails. The basic idea is that if the null hypothesis is true, then permuting the residuals should make little difference, and whatever difference is observed in the chosen statistic is the result simply of chance variation. At the end of the process a "probability" statement can be made, corresponding to the "P-value" in the standard (parametric) testing framework: for example, the probability of getting a coefficient estimate of a certain size from the actual data might be 28 percent when compared with the distribution over a thousand such coefficients, based on permuted data and a null hypothesis that the coefficient is zero (causing the null to be accepted, in this case, no doubt), or it might be that the probability is only a tenth of one percent (causing the null to be rejected).

The foregoing hardly does justice to the richness of the randomized testing approach; it is intended merely to indicate in a general way how such procedures can be used in a project such as the present one. The details of our implementation of the approach are provided later.

There are, then, the two quite different approaches that we follow in testing hypotheses -- the parametric and the nonpara-

metric approaches. Carrying out a test in the two different ways can be thought of as reinforcing the results obtained by a single approach or, if the results differ, as suggesting uncertainty and appropriate caution in interpreting the results from either approach taken by itself.

## 8. DETAILED SPECIFICATION AND ESTIMATION OF EQUATIONS

We move now to a detailed description of the theoretical equations that constitute the basis for the hypothesis tests. (The estimated forms of the equations are provided in Appendix II.) There are six categories of tests (excluding property values, for which relevant data do not exist), and corresponding to each, one or more equations. We discuss the categories and associated equations in sequence, including the procedures used in estimation. For convenience in subsequent references we label the categories A, B, C, D, E, and F.

### A. Rents

We used the equation for this category in section 6 to illustrate the parametric approach to hypothesis testing. The equation is estimated with pooled metropolitan area time series data, and most of the explanatory variables therefore bear two subscripts: i for area, t for year. Rewriting equation (7) in full detail, we have

$$(8)\quad r_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2} + \beta_{20}y_{it} + \beta_{21}y_{i,t-1}$$

$$+ \beta_{22}y_{i,t-2} + \beta_{30}p_{it} + \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2}$$

$$+ \beta_{40}\pi_{t} + \beta_{41}\pi_{t-1} + \beta_{42}\pi_{t-2} + \gamma_{1}r_{i,t-1} + \gamma_{2}r_{i,t-2}$$

$$+ \phi_1 B^+_{it} + \phi_2 B^-_{it} + \theta_1 C^+_{it} + \theta_2 C^-_{it} + \varepsilon_{it}$$

The variables are based on annual averages, where relevant. They are defined as follows:

- $r$ -- percentage rate of increase of rental price index
- $n$ -- percentage rate of population growth
- $y$ -- percentage rate of increase of real personal disposable income per capita
- $p$ -- percentage rate of increase of general price index
- $\pi$ -- percentage rate of interest on three-month treasury bills
- $B^+$ -- dummy variable: value 1 if rent control regime type B is in effect in given area and year and the year is one of strong upward pressure on rents; value 0 otherwise
- $B^-$ -- dummy variable: value 1 if rent control regime type B is in effect in given area and year and the year is one of weak upward pressure on rents; value 0 otherwise
- $C^+$ -- dummy variable: value 1 if rent control regime type C is in effect in given area and year and the year is one of strong upward pressure on rents; value 0 otherwise
- $C^-$ -- dummy variable: value 1 if rent control regime type C is in effect in given area and year and the year is one of weak upward pressure on rents; value 0

otherwise

$\varepsilon$ -- random error

Whether a metropolitan area is subject to rent controls of one type or the other depends, of course, on whether the controls are in effect in the province in which the area is located.

As noted in section 4, personal disposable income series are not available for metropolitan areas, and we were forced therefore to use provincial values of y as proxies. (The assumption -- an approximation, of course -- is that real personal disposable income per capita increases in the same proportion in a metropolitan area as in the province as a whole, from one year to the next.)

Equation (8) is regarded as a reduced form equation derived (implicitly) from a larger (but unspecified) structural model. We assume that the exogenous variables in the structural model can be adequately represented by the current, once-lagged, and twice-lagged values of n, y, p, and $\pi$, supplemented by once-lagged and twice-lagged values of the dependent (endogenous) variable r.

We were able to obtain the required data (aside from income data) for sixteen metropolitan areas, back to the year 1971 in most cases. (Exceptions were Charlottetown and Victoria, as noted in section 4.) Based on the taxonomy of provincial control regimes shown in Table 12, we experimented with four different data sets: (1) data for all years except those in which there was a change of regime (i.e., all years except those for which the first two characters of the code are AB, AC, BA, etc.); (2) data for all years except years in which there was a change and the immediately following years (i.e., all years with a zero in the third character position); (3) data for all years except years of change and

the immediately following years (as in (2)) and years in which Table 12 shows a qualifying note indicating some modification of the controls, although no change in the basic type of regime (i.e., years with a numerical code 2 to 8 in the fourth character position); (4) data as in (3) plus years representing the first year subsequent to the one in which a new regime was implemented (i.e., all years except those in which a change of regime occurred or a significant qualifying note was attached.) Our experimentation indicated that the choice of data set did not affect the test results in any important way. In the end we chose set 4 as the basic data set for the equations and tests in the rent and all other categories. That choice is more conservative in one sense than we had originally anticipated but more liberal in another: it is more conservative in that it ignores years in which there were changes within a given regime (as indicated by qualifying notes) but more liberal in that it allows for a shorter period in which start-up effects may be significant. The trade-off, of course, is between number of observations and homogeneity of the observations. We believe our choice to be a reasonable one.

Equation (8) was estimated by ordinary least squares (OLS). Because different years had to be omitted in different provinces, and the series in the different provinces were therefore interrupted at different points, the usual tests for serial correlation in error terms could not be applied. Instead we examined plots of the residuals. It appeared, from that examination, that serial correlation was not a serious problem. It appeared also that there was no significant problem of heteroscedasticity in the error terms. The use of OLS therefore seemed reasonable.

The sixteen metropolitan areas for which data could be obtained, and which were therefore incorporated into the analysis (equation estimation) are the following:

St. John's, Newfoundland

Charlottetown, Prince Edward Island

Halifax, Nova Scotia

Saint John, New Brunswick

Quebec, Quebec

Montreal, Quebec

Ottawa, Ontario (i.e., the Ontario portion of the Ottawa-Hull metropolitan area)

Toronto, Ontario

Thunder Bay, Ontario

Winnipeg, Manitoba

Regina, Saskatchewan

Saskatoon, Saskatchewan

Edmonton, Alberta

Calgary, Alberta

Vancouver, British Columbia

Victoria, British Columbia

(We refer to these areas as "metropolitan." In the terminology of the Census, Charlottetown would be referred to as a "census agglomeration"; however, the term metropolitan is a convenient one.)

B. Housing Starts

The category "housing starts" refers to starts of rental units. However, series of rental-unit starts are not available; they are proxied by starts of apartment units. Unlike the other

categories, which are represented by reduced form equations, the equations for this one (there are two equations) are in the nature of "structural" equations. Starts are related to vacancy rates and rents, both of which should properly be regarded as endogenous variables within a larger model of the housing market. (A complete structural model would have separate equations to explain them.) The first equation for starts, which is fitted to pooled metropolitan area time series, can be written as

$$(9)\quad (S_a/N)_{it} = \alpha + (\beta_1+\phi_1 B_{it}+\theta_1 C_{it})v^*_{it} + (\beta_2+\phi_2 B_{it}+\theta_2 C_{it})(R/P)_{it}$$

$$+ \gamma(S_a/N)_{i,t-1} + (\text{MA terms}) + \varepsilon_{it}$$

where $v^*_{it} = \ln(v_{it}/(1-v_{it}))$ (a logit transformation), the subscripts i and t refer to area and year, as before, and the basic variables are defined as follows:

- $S_a$ -- starts of apartment units
- N -- population
- v -- vacancy rate in apartment buildings with 6 or more units
- R -- rental price index
- P -- general price index
- B -- dummy variable: value 1 if rental regime type B is in effect in given area and year; value 0 otherwise
- C -- dummy variable: value 1 if rental regime type C is in effect in given area and year; value 0 otherwise
- $\varepsilon$ -- random error

The "MA terms" refer to a set of dummy (zero-one) variables representing the individual metropolitan areas. (The dummy variable for St. John's has value 1 if the area is St. John's, 0 otherwise; the variable for Halifax has value 1 if the area is Halifax, 0 otherwise; and so on.) The area-specific dummy variables are included in this equation in order to shift the intercept from city to city, in recognition of the fact that some cities have larger proportions of apartment units in their housing stocks, and hence have larger numbers of starts of such units, other factors aside. (Area-specific dummy variables were experimented with also in the equations for other categories but were found not to be statistically significant, and were therefore discarded. In the case of housing starts they are generally significant.) A minor technical point is that not all of the area-specific dummy variables can be included, as well as the intercept term, because of a well known problem of matrix singularity. The variable for the Toronto metropolitan area was therefore dropped. (Dropping Toronto makes it a reference case: the coefficients of the variables for the other areas are interpreted as differences from the Toronto coefficient. Also, although the choice of an area to drop is arbitrary, it has no effect on the coefficients in the equation other than the intercept and the coefficients of the area-specific dummies themselves.)

Expressing starts as a ratio to population is a convenient scaling device, allowing for the greatly differing numbers of starts as between large metropolitan areas and small ones. The use of the logit transformation $v^*$, rather than v itself, was made after some experimentation with the use of v, and in recognition

of the fact that small changes in the vacancy rate when it is close to zero would be expected to have much greater effects than the same small changes in the rate when it is relatively high (e.g., a move from 1 percent to 1/2 percent, compared with a move from 7 percent to 6 1/2 percent). The $v^*$ transformation recognizes that small changes when the rate is low may represent a substantial increase in market "tightness," and hence in the inducement to create new units. Expressing the rental price relative to the general price level recognizes that it is the "real" level of rents that should influence starts, rather than the actual or "nominal" level. Including the one-period lagged value of $S_a/N$ allows for delayed responses to changes in $v^*$ and R/P. (One may think of this as having come from what is known in the econometrics literature as a Koyck distributed lag.)

Equation (9) makes explicit the allowance for possible shifts in the coefficients of the $v^*$ and R/P variables from one control regime to another. One would expect the coefficient of $v^*$ to be negative (lower vacancy rate, more starts), and the null hypotheses about $\phi_1$ and $\theta_1$ would be that they are positive -- that rent controls make the coefficient of $v^*$ "less negative." Similarly, for R/P one would expect a positive coefficient (higher real rental price, more starts), and the null hypotheses about $\phi_2$ and $\theta_2$ would be that they are negative -- that rent controls reduce the response of starts to rent increases by making the coefficient "less positive." For actual estimation purposes, equation (9) is rewritten in the form

$$(10)\quad (S_a/N)_{it} = \alpha + \beta_1 v^*_{it} + \phi_1 (Bv^*)_{it} + \theta_1 (Cv^*)_{it} + \beta_2 (R/P)_{it}$$

$$+ \phi_2(B(R/P))_{it} + \theta_2(B(R/P))_{it} + \gamma(S_a/N)_{i,t-1}$$

$$+ (\text{MA terms}) + \varepsilon_{it}$$

Equation (9), rewritten in the form (10), was estimated by the method of instrumental variables. (The use of OLS would be inappropriate in this case, for well known econometric reasons, since the equation has endogenous variables on its right side.) The variables used as instruments are current, once-lagged, and twice-lagged p, n, y, and $\pi$ (as defined previously), once-lagged and twice-lagged $v^*$, R/P, and $S_a/N$, the B and C dummy variables, and the MA dummy variables.

The second equation defined for the housing starts category is similar to equation (9), except that $v^*$ and R/P are replaced by their first differences, $\Delta v^*$ and $\Delta(R/P)$. The role of this second equation is to make possible the testing of hypotheses about the effects of rent controls on the relationships between starts and changes in the vacancy rate and the rental price index, as requested by CMHC. The equation is

$$(11)\quad (S_a/N)_{it} = \alpha + (\beta_1+\phi_1 B_{it}+\theta_1 C_{it})\Delta v^*_{it}$$

$$+ (\beta_2+\phi_2 B_{it}+\theta_2 C_{it})\Delta(R/P)_{it}$$

$$+ \gamma(S_a/N)_{i,t-1} + (\text{MA terms}) + \varepsilon_{it}$$

where $\Delta v^*_{it} = v^*_{it} - v^*_{i,t-1}$, $\Delta(R/P)_{it} = (R/P)_{it} - (R/P)_{i,t-1}$. Aside

from the change of variables, this equation is treated in exactly the same way as before; it is converted to a form corresponding to equation (10) and estimated by instrumental variables, using the same set of instruments as in the case of the earlier equation.

It is perhaps worth noting, before proceeding, that we are "recycling" some of the parameter symbols: the symbols have different meanings (and values) in different equations. However, for subsequent interpretive convenience we have followed the practice everywhere of using the $\phi$ and $\theta$ symbols to relate to control regimes B and C, respectively, and the $\gamma$ symbol to relate to lagged values of the dependent variable. Subscripts are attached to $\phi$, $\theta$, and $\gamma$, where necessary.

C. Vacancy Rates

The vacancy rate series that we are using here is the same as in the previous category, namely vacancies in apartment buildings with six or more units. As before too, we use $v^*$, the logit transformation of v, to capture the nonlinear market "tightness" effect associated with low vacancy rates. The equation (there is only one in this, and in each of the subsequent categories) is a reduced form equation similar to (8), except that there is no distinction between years of strong and weak upward pressure on rents. Specifically, it is

$$(12)\quad v^*_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2} + \beta_{20}y_{it} + \beta_{21}y_{i,t-1}$$
$$+ \beta_{22}y_{i,t-2} + \beta_{30}p_{it} + \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2}$$
$$+ \beta_{40}\pi_t + \beta_{41}\pi_{t-1} + \beta_{42}\pi_{t-2} + \gamma_1 v^*_{i,t-1} + \gamma_2 v^*_{i,t-2}$$

$$+ \phi B_{it} + \theta C_{it} + \varepsilon_{it}$$

All variables are as defined previously. The equation is estimated using pooled metropolitan area time series data, as in both of the previous categories. Since it is a reduced form equation, OLS is appropriate, and that is the method that was used.

D. Proportion of Renter Households

This category represents our replacement of the original "tenure preferences" category proposed by CMHC, the replacement being made in light of the lack of direct information about preferences. The equation respresenting the category is the following reduced form equation:

$$(13)\ (H_r/H)_{it} = \alpha + \beta_{10} n_{it} + \beta_{11} n_{i,t-1} + \beta_{12} n_{i,t-2}$$

$$+ \beta_{20} y_{it} + \beta_{21} y_{i,t-1} + \beta_{22} y_{i,t-2} + \beta_{30} p_{it}$$

$$+ \beta_{31} p_{i,t-1} + \beta_{32} p_{i,t-2} + \beta_{40} \pi_t + \beta_{41} \pi_{t-1}$$

$$+ \beta_{42} \pi_{t-2} + \gamma (H_r/H)_{i,t-1} + \phi B_{it} + \theta C_{it}$$

$$+ (\text{PROV terms}) + \varepsilon_{it}$$

$H_r$ stands for renter households and H for total (renter plus owner) households. (Note that $H_r$ and H represent occupied dwellings, as distinct from stocks of housing units, some of which would be unoccupied.) The exogenous variables on the right side

of the equation are the same as in equation (12). However, only the one-period lagged value of the dependent variable is included; the first-order serial correlation of the $H_r/H$ series is extremely high, as one would expect, and the inclusion of the two-period lagged value as well would have added virtually nothing to the explanatory power of the equation and would have made the individual estimates of the coefficients of the two lagged variables quite unreliable.

Equation (13) was fitted to pooled provincial time series, rather than metropolitan area series, since annual time series of tenure proportions could be calculated only at the provincial level. (As noted previously, the tenure data are from the Statistics Canada Household Facilities and Equipment Survey.) The i subscript therefore stands for province in this case. Also, recognizing that tenure proportions may vary from province to province for historical, social, or other reasons, we have included in the equation a set of dummy (zero-one) variables for the individual provinces; those are represented in equation (13) by what we have called "PROV terms." (The same problem of matrix singularity as in equation (9) would arise here if all ten of the dummy variables had been included; we therefore dropped Ontario, which thus became the reference province for interpreting the coefficients of the dummy variables.)

E. Conversions

The conversions equation is a reduced form equation similar to equation (12), except of course for the dependent variable.

$$(14)\quad (C/K_s)_{it} = \alpha + \beta_{10} n_{it} + \beta_{11} n_{i,t-1} + \beta_{12} n_{i,t-2}$$

$$+ \beta_{20}y_{it} + \beta_{21}y_{i,t-1} + \beta_{22}y_{i,t-2} + \beta_{30}p_{it}$$

$$+ \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2} + \beta_{40}\pi_t + \beta_{41}\pi_{t-1}$$

$$+ \beta_{42}\pi_{t-2} + \gamma_1(C/K_s)_{i,t-1} + \gamma_2(C/K_s)_{i,t-2} + \phi B_{it}$$

$$+ \theta C_{it} + \varepsilon_{it}$$

The new variables are C and $K_s$: C stands for conversions from single dwelling units to multiple units; $K_s$ stands for the beginning-of-year stock of single dwelling units. The equation is estimated, by OLS, using provincial data; the i subscript thus refers to province. As we stated earlier, we would have preferred to have conversion series representing conversions from renter to owner units, as envisaged by CMHC in its original statement of hypotheses. Given that such series were not available we would have liked to have had conversions from multiples to singles, but series of that kind were not available either. The only series that were available were ones representing conversions of singles to multiples, and so we have worked with those.

The actual conversions series represent the numbers of units <u>after</u> conversion, less one in each case. (If a single unit is converted into three multiple units, the number of conversions is counted as two; why that is so is not clear to us, but that appears to be the way the calculation is made by Statistics Canada.) We have "standardized" the series across provinces by expressing the number of conversions during a year as a proportion of the

stock of singles at the beginning of the year.

F. Major Repairs

This category is the stand-in for the "maintenance and repairs" category proposed by CMHC. The only data that we could obtain of relevance to the question of whether maintenance and repairs are reduced under rent controls were provincial series from the Statistics Canada Household Facilities and Equipment Survey relating to the need for "major repairs." Annual series were required for our purpose, which meant that we had only six observations to work with in each province, and only five after allowing for a one-period lag. The equation for the major repairs category is again a reduced form equation. However, because the series were so short it was not possible to take account of as many explanatory variables as in the other reduced form equations. The equation is as follows:

$$(15)\quad (H_r^*/H_r)_{it} = \alpha + \gamma(H_r^*/H_r)_{i,t-1} + \phi B_{it} + \theta C_{it} + \varepsilon_{it}$$

$H_r$ is the number of occupied rental dwellings (equal to the number of renter households) and $H_r^*$ is the number of such households classified as "in need of major repairs." The subscript i stands for province. The equation is a simple autoregressive equation, augmented by the inclusion of the two rent control shift dummies, B and C. The idea underlying the equation is that if rent con trols discourage the making of major repairs by landlords, the proportion of occupied dwellings in need of such repairs would move to a higher level in the long-run with controls in effect than without them. The theoretical long-run (or "stationary

state") proportion is equal to $(\alpha + \phi + \theta)/(1-\gamma)$, and thus (assuming $\gamma$ to be a positive fraction, as it should be, and in fact turns out to be when estimated) one would look for positive values of $\phi$ and $\theta$.

Equation (15), being a reduced form equation, was appropriately estimated by OLS.

## 9. CLASSIFICATION OF YEARS BY STRONG OR WEAK UPWARD PRESSURE ON RENTS

For purposes of equation (8), the rents category equation, it was necessary to classify every year for every metropolitan area as a year of either "strong" upward pressure on rents or "weak" upward pressure. We considered a number of alternative ways of doing that, including classification on the basis of a simple inspection of plots of r. In the end we chose the following. We fitted to pooled metropolitan area time series an equation of the form

$$(16)\quad r_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}\Delta n_{it} + \beta_{12}\Delta n_{i,t-1} + \beta_{20}y_{it} + \beta_{21}\Delta y_{it}$$
$$+ \beta_{22}\Delta y_{i,t-1} + \beta_{30}p_{it} + \beta_{31}\Delta p_{it} + \beta_{32}\Delta p_{i,t-1}$$
$$+ \beta_{40}\pi_t + \beta_{41}\Delta\pi_t + \beta_{42}\Delta\pi_{t-1} + \phi B_{it} + \theta C_{it} + \varepsilon_{it}$$

where $\Delta$ denotes a first difference (e.g., $\Delta n_{it} = n_{it} - n_{i,t-1}$). This equation may be thought of as an estimate of the long-run form of equation (8) (see the discussion in section 10), without the dichotomization of B and C into $B^+$, $B^-$, $C^+$, and $C^-$. The equation was fitted by the method of instrumental variables, using the

same set of data as were used subsequently in estimating equation (8). Based on the fitted equation (16), we then asked "what would r have been for each metropolitan area in each year if there had been no rent controls in effect." To formulate an answer we set B and C to zero, inserted actual values of $n_t$, $y_t$, $p_t$, and $r_t$ into the equation, set all the first differences to zero, and generated a set of "what if" values for r. Let us label those values $r^*$. Note that an $r^*$ value was calculated for every metropolitan area in every year for which data were available, not just those years that were used in estimating the equation. We thus had complete series of $r^*$ values for each area over the eighteen-year period 1974-91. We then calculated the mean values of $r^*$ for each area over that period, and classified the individual years according to whether their $r^*$ values were above ("strong" upward pressure) or below ("weak" upward pressure) the area means. The $r^*$ values, after subtracting means, were thus interpreted as reflecting the degree of pressure exerted by population growth, real per capita income growth, inflation, and interest rates, ignoring any restraints that might in practice have been imposed by controls. As a check we plotted the $r^*$ values for each area and examined the plots for reasonableness. We concluded that the time series patterns exhibited by the series were in fact reasonable.

## 10. ESTIMATING THE DISTRIBUTIONS OF LONG-RUN PARAMETER ESTIMATES

Long-run responses to rent controls are of interest, as well as shorter-run responses. As a basis for the discussion to follow, consider a "generic" equation of the form

$$(17)\quad z_t = \alpha + \beta' x_t + \gamma z_{t-1} + \phi B_t + \theta C_t + \varepsilon_t$$

z is any dependent variable of interest, x is any vector of explanatory variables, with coefficient vector $\beta$, and B and C have the usual (dummy variable) definitions. (For simplicity, the area subscript i is suppressed.) The parameters $\phi$ and $\theta$ represent the short-run or immediate responses of z to the presence of controls. The long-run (stationary state) responses are calculated by setting $z_t$ equal to $z_{t-1}$ and solving. The resulting long-run response parameters associated with B and C are then found to be $\phi^* = \phi(1-\gamma)^{-1}$ and $\theta^* = \theta(1-\gamma)^{-1}$. Because $\phi^*$ and $\theta^*$ are nonlinear functions of the original parameters, it is troublesome to estimate the distributions -- in particular, the variances -- of their estimators. What we have done, therefore, is to use the following result, due to Bewley (1979).

It can be shown that equation (17) is equivalent (the result of a straightforward transformation) to an equation of the form

$$(18)\quad z_t = \alpha^* + (\beta^*)' x_t - \gamma^* \Delta z_t + \phi^* B_t + \theta^* C_t + \varepsilon_t^*$$

where $\alpha^* = \alpha(1-\gamma)^{-1}$, $\beta^* = \beta(1-\gamma)^{-1}$, $\gamma^* = \gamma(1-\gamma)^{-1}$, $\varepsilon_t^* = \varepsilon_t(1-\gamma)^{-1}$, and $\phi^*$ and $\theta^*$ are as defined above. If this equation is estimated by an appropriate procedure, direct estimates of $\phi^*$ and $\theta^*$ are obtained, and along with them distributional estimates (variances, in particular) that can be used for carrying out hypothesis tests.

The presence of $\Delta z_t$ on the right side of equation (18) has implications for the method by which the equation should be estimated, since $\Delta z_t$ (equal to $z_t - z_{t-1}$) is an endogenous variable,

thus making OLS inappropriate. Following Bewley, we therefore use an instrumental variables procedure (equivalent to two-stage least squares) in which the instruments are $x_t$, $z_{t-1}$, $B_t$, and $C_t$. It can be shown that the values of $\phi^*$ and $\theta^*$ obtained by estimating equation (18) by the instrumental variables procedure are exactly equal to the values obtained by estimating equation (17) by OLS. The advantage of going to equation (18) is that it provides a straightforward way of carrying out tests of the long-run parameters, since those parameters are now simply coefficients in a linear equation. We have used equations of the form of (17) for tests involving the short-run parameters $\phi$ and $\theta$, and equations of the form of (18) for tests involving the long-run parameters $\phi^*$ and $\theta^*$.

An analogue of the transformation that converted (17) into (18) can be applied to equations with more than one lag on the dependent variable. Consider an equation of the form

$$(19)\quad z_t = \alpha + \beta' x_t + \gamma_1 z_{t-1} + \gamma_2 z_{t-2} + \phi B_t + \theta C_t + \varepsilon_t$$

A transformation similar to the previous one converts this equation into one of the form

$$(20)\quad z_t = \alpha^* + (\beta^*)' x_t - \gamma_1^* \Delta z_t - \gamma_2^* \Delta z_{t-1} + \phi^* B_t + \theta^* C_t + \varepsilon_t^*$$

As before, this equation is appropriately estimated by instrumental variables, the set of instruments now including $z_{t-2}$ as well as the ones used previously. The definitions of the "starred" symbols are now $\alpha^* = \alpha(1-\gamma_1-\gamma_2)^{-1}$, $\beta^* = \beta(1-\gamma_1-\gamma_2)^{-1}$,

$$\gamma_1^* = \gamma_1(1-\gamma_1-\gamma_2)^{-1},\ \gamma_2^* = \gamma_2(1-\gamma_1-\gamma_2)^{-1},\ \phi^* = \phi(1-\gamma_1-\gamma_2)^{-1},$$

$$\theta^* = \theta(1-\gamma_1-\gamma_2)^{-1}, \text{ and } \varepsilon_t^* = \varepsilon_t(1-\gamma_1-\gamma_2)^{-1}.$$

## 11. THE TESTS

The test results are presented in Tables 13-19. (A total of 181 tests are reported.) Preceding each table is a page of notes with some descriptive information about the procedures used and the basic characteristics of the tests: form of equation, method of estimation, type of data, number of observations, and definitions of variables and their subscripts. The tables themselves all have the same format. Each has seven columns. The first provides an identifying label for each test. The second shows any restrictions that were imposed on the estimated equation (e.g., the coefficients are restricted to be the same for rent control regimes B and C). The third shows the restrictions tested, i.e., the null hypotheses, while the fourth shows the corresponding alternative hypotheses. The fifth column indicates the type of test -- an S for a "standard" test (one based on normal distribution theory), an R for a "randomization" test (as discussed in section 7, and discussed further below). The sixth column shows the test statistics used (t, F, etc.) and the seventh reports the P-values associated with the tests.

The P-value for a test is the indicator on which the null hypothesis is "accepted" or "rejected." It represents the probability of getting a value of the test statistic at least as great as the one actually obtained if the null hypothesis is true. In a "significance level" framework, one chooses a particular level (or

test size), such as 1 percent or 5 percent. If the P-value is at or below the chosen level, the null hypothesis is "rejected"; otherwise it is "accepted" (some would prefer to say "not rejected"). Showing the P-values themselves provides more information than simply reporting whether hypotheses are accepted or rejected at a given level of significance. (Anyone who prefers to work instead in the significance level framework can simply choose the level and base conclusions on it; this procedure is illustrated below.)

The statistics that we have used in the "standard tests" are the t, F, and Wald statistics. The reported P-values for those statistics are based on Student's t distribution, the F distribution, and (in the case of the Wald statistic) the chi-square distribution. The equations underlying the tests all have lagged dependent variables; some also have endogenous variables on the right side, and were therefore estimated by the method of instrumental variables. For those reasons, strictly speaking, the theoretical distributions are only approximations to the actual ones. They may be regarded as asymptotically correct -- correct for "large" samples, that is, but not strictly so for "small" samples. However, the small-sample distributions are unknown and standard procedure is to base tests such as the ones we have carried out on the known large-sample distributions. Given the sample sizes we have been working with it can reasonably be supposed that the approximations are close, as long as the errors in the equations being estimated are assumed to be approximately normally distributed.

The "randomization" tests are based on 5000 random permuta-

tions of the residuals from the fitted equations, as described in section 7. The sampling to generate the permutations was done (for ease of calculation) with replacement. It is therefore possible that the same permutation would occur, by chance, more than once. However, given the total numbers of possible permutations a repetition would be a rare event, and such an event (even if it occurred) would be of no practical concern.

The tests reported in the tables fall into several broad categories. In some cases B and C are combined to form a single variable, and the coefficient of that variable is tested. (Could it be zero -- implying no effect of rent controls -- and a nonzero coefficient obtained simply as a result of random variation in the data?) In other cases the B and C variables are kept separate and tests are carried out on their individual coefficients. Some of the tests refer to the short-run effects of B and C, others to their long-run effects, as represented by parameters bearing an asterisk superscript, and defined in section 10. (Could the short-run effect of regime type B, as represented by $\phi$, say, be zero? Could the long-run effect, as represented by $\phi^*$, be zero?) Still other tests have to do with the equality of the coefficients of the B and C regimes. (Could the effects of B and C be the same, in spite of the different approaches to rent control that they represent? The possibility that the coefficients are both zero then becomes a special case of this equality hypothesis.)

For each null hypothesis there must be a precisely defined alternative hypothesis. The alternative can be an inequality hypothesis, such as $\phi \neq 0$, when the null is $\phi = 0$. Or it can be a "greater than" ($\phi > 0$) or "less than" ($\phi<0$) hypothesis. In test-

ing a null hypothesis about an individual coefficient, such as $\phi = 0$, a $\phi \neq 0$ alternative gives rise to a "two-sided" test while a $\phi > 0$ or $\phi < 0$ alternative gives rise to a "one-sided" test.

The test statistic for a randomization test need not be one of the conventional ones, such as t or F, although those are possible choices. The advantage of randomization testing is that it does not require the assumption of a particular type of error distribution, such as (and most frequently) the normal distribution, and hence the statistic on which the test is based does not have to be one associated with the normal or other particular distribution. In the randomization tests reported in the tables we have used the coefficient itself that is being tested as the statistic (for a test of whether the coefficient is zero), the sum of the absolute values of two coefficients (for a test of whether both coefficients are zero), or the absolute value of the difference of two coefficients (for a test of their equality).

Randomization tests require considerable computing time. For that reason, and in order to avoid a further proliferation of tests (for which the reported number is already very large), we have confined the randomization tests to only some of the hypotheses of interest. In practice, for those hypotheses for which both standard and randomization tests were done the conclusions were essentially the same in both cases. That gave a substantial measure of support to the standard testing procedures, and allowed us to draw conclusions from the standard tests (where those alone were available) with a greater degree of confidence than would otherwise have been the case.

We proceed now to discuss the tests under the heading of each

of the hypothesis categories in turn. (The tables corresponding to the hypothesis categories are noted.)

Hypothesis Category A: Rents (Table 13)

The tests for this category are identified as A1 to A50. (The equation on which the tests are based is shown in theoretical form on the page of notes preceding Table 13. The same is true of Tables 14-19.) All of the tests relate to possible influences on the rate of increase of the rent index. There are 192 observations available for the tests; see Appendix III. The B and C regimes are combined into a single rent controls variable in Tests A1 to A18 (as indicated by the equality restrictions imposed on their coefficients), and various hypotheses are considered, consistent with CMHC's original statement, our "formalization" of it for statistical purposes (as in Table 1), and some extensions that occurred to us subsequently. Tests A19 to A22 are tests of whether the coefficients of B and C are in fact identical. Tests A23 to A50 are based on the separate treatment of the B and C regimes, thus allowing for possible differences in their effects. Both short-run and long-run coefficients are tested at every step in the analysis. Possible differences in the effects of controls in periods of "strong" and "weak" upward pressure on rents are tested also.

If we take as significant a P-value in the neighbourhood of 10 percent or lower (a criterion that is somewhat on the liberal side, by conventional standards), the only results in Table 13 that suggest statistical significance (rejection of the null hypotheses) are those of tests A13 to A18 and (with a slight weakening of the criterion) tests A43 to A46. Tests A15 to A18 are tests of

whether the effects of controls are the same in periods of "strong" and periods of "weak" upward pressure on rents. The results of those tests suggest that the effects may be different, and the results of tests A13 and A14 and A43 to A46 suggest that the difference may be the result of a tendency for rents to rise more rapidly in periods of "weak" pressure. Also, tests A43 to A46 suggest that it is regimes of type B in which that is more likely to occur. (See below, though, for discussion of the difficulties in separating the types B and C effects.) Taking the results at face value, the interpretation might be that rent controls (especially those of type B) are designed so that they are less binding in periods when the rental market is relatively weak, and that landlords may find it possible (and desirable) to raise rents during such periods by more than they would otherwise. (Another line of argument is that there may be guidelines associated with a control regime, and that those may provide a "focal point" for landlords; the literature on industrial organization suggests that cooperation in price setting by suppliers is facilitated by the existence of such a focal point.) But the other side of the coin is that if there is no effect on the rate of increase in the long run, landlords should find themselves raising rents by <u>less</u> than they would otherwise when the rental market is stronger, and the controls more binding -- and there is no strong evidence of that occurring. Given all of this, and the fact that the P-values, though lower than elsewhere in the table, are still not very low by the standards of conventional statistical testing criteria (none indicate significance at the 5 percent level, let alone the widely used 1 percent level), we are inclined to be

sceptical about the indications of more rapid increases in "weak" periods. In sum, it seems fair to say (1) that the tests do not indicate any significant tendency for rent controls to reduce the rate of increase of rents in the long run, or on average over rental market "cycles," and (2) that if there is any tendency for rates of increase to be affected it appears most likely that they are greater in periods of "softness" in the market.

This is perhaps a good point at which to note that a test at the 10 percent level of significance can be expected to indicate rejection of a null hypothesis, simply by chance, 10 percent of the time, when the null hypothesis is in fact true. Similarly, a test at the 5 percent level will indicate rejection 5 percent of the time when the null is true. Given the large number of tests that we are reporting, one might expect, therefore, that there would be a small proportion of "false" rejections. (The tests are not all independent, by any means, since many of them depend on the same data. Such lack of independence would affect the proportion of false rejections.)

Hypothesis Category B: Housing Starts (Tables 14 and 15)

It will be recalled that there are two types of influence, and correspondingly two types of equation, being considered for this category -- one relating to the relationships between starts of rental units (as proxied by apartment units) and the levels of vacancy rates and rents, the other to relationships between starts and changes in vacancy rates and rents. Table 14 relates to "levels effects," Table 15 to "changes effects." We label the corresponding subcategories BA and BB, and identify the tests in the two tables as BA1, BA2, etc., and BB1, BB2, etc.

The sequences of tests reported in these two tables are similar in structure to those of the previous one, except that there is no attempt to differentiate between periods of "strong" and "weak" pressure. To take Table 14 as the example, tests BA1 to BA12 combine regimes B and C into a single regime and test null hypotheses that rent controls have no effects on the responsiveness of starts to either vacancy rates or rents, in either the short or the long run. (In the case of vacancy rates, one would expect any effects to take the form of making a negative coefficient "smaller," in the sense of moving it closer to zero; hence the "greater than" alternative specified for tests BA1 to BA4. In the case of rents one would expect the effects to take the form of moving a positive coefficient down toward zero; hence the "less than" alternative specified for tests BA5 to BA8. Tests based on "one-sided" alternatives such as these are difficult to implement when the null hypotheses are joint or composite, as in BA9 to BA12; hence the alternatives for those tests are expressed in a "not-equal-to" form.)

Tests BA13 to BA24 are concerned with whether the effects of regimes B and C are in fact the same. Some of the tests are concerned with effects on the response to vacancy rates (BA13 to BA16), some with effects on the response to rents (BA17 to BA20), and some with both (BA21 to BA24). Tests BA25 to BA38 allow for possible differences between regime type B and regime type C effects. Otherwise the tests are of a similar form to BA1 to BA12. The tests reported in Table 15, which run from BB1 to BB38, correspond exactly in form with those of Table 14.

An examination of Table 14 indicates immediately that there

are no rejections of null hypotheses at any level of significance corresponding even remotely to conventional ones. The smallest P-value is .321.

An examination of Table 15 tells a somewhat different story. When regimes B and C are combined, the null hypothesis that the responsiveness of starts to changes in rents is zero is rejected at somewhat less than the 10 percent level, by both standard and randomization tests (BB5 and BB6). When the two regimes are treated separately, it turns out that those apparently significant effects are associated entirely with regime type C, as evidenced by the P-values of tests BB33 to BB38, which are generally very low. Corroborative evidence is provided by the extremely low P-values of tests BB17 to BB24, which suggest emphatically a rejection of the hypotheses of equality of effects on the response of starts to rent changes under the two regimes. In short, if one accepts the test results at face value, it appears that type C controls do tend to make starts of rental units less responsive to changes in rents, whereas that appears not to be true of type B controls.

There are three reasons why we are not in fact inclined to accept these results at face value. The first is a theoretical reason; the second and third are statistical ones.

The theoretical reason is that, in our opinion, the specification of an equation in which the level of starts of rental units is a function of changes in vacancy rates and real rents is inappropriate. We contend rather that the level of starts should be regarded as a function of the level of the vacancy rate (or a transformation of it, such as the logit transformation that we

have used) and the level of rents. In support of that contention, consider the following "thought experiment." Suppose, for example, that the vacancy rate is high in one year, falls to a very low point in the next year (for whatever reason), and then remains at that low point for the next several years. Under the "levels" specification of the equation (equation (9)), starts will continue to respond positively as long as the vacancy rate remains low: the sustained "tightness" of the market will provide a continuing inducement to increase the supply of rental units. Under the "changes" specification (equation (11)), on the other hand, there will be an effect on starts in the year in which the vacancy rate drops, but thereafter the first difference of the starts variable on the right side of the equation will be zero; the only effects in the second and subsequent years therefore will be the "carry-forward" effects of the initial change, resulting from the inclusion of the lagged dependent (starts) variable on the right side of the equation. There will be no further response to the current vacancy rate, no matter how low it may be. An exactly analogous argument can be made with regard to the rent variable: high real rents (and hence prospective builder and landlord profits) will provide a continuing stimulus to starts when the "levels" equation is used but only a one-year stimulus (aside from "carry-forward" effects) when the "changes" equation is used. In light of this type of argument we think that the "levels" form of equation is the appropriate one.

The first of the statistical reasons is simply that the "levels" equation provides a much better fit to the data than the "changes" equation. That is evident from the summary statistics

attached to the estimated equations shown in section B of Appendix II.

The second statistical reason for not being inclined to accept (or at least being sceptical of) the test results is as follows. The evidence based on the "changes" equation appears to indicate that it is type C controls that make starts less responsive to changes in rents. But type C controls were in effect only in Newfoundland and Quebec over the data period to which the equation was fitted. That implies that there is very strong correlation between effects associated with these two provinces (which might not be related at all to rent controls) and effects associated with the type of control regime. Disentangling the two types of effects cannot be done with much reliability given the data that are at present available.

The latter argument is an argument for being sceptical about the apparent significance of type C controls. The other two are arguments for doubting the "changes" equation specification, and hence the test results for either type of controls based on that equation.

Hypothesis Category C: Vacancy Rates (Table 16)

The tests in Table 16 are related to whether vacancy rates (for apartment units) are influenced (in the short or long run) by rent controls. Where feasible, tests of no effect have been based on the alternative of a negative effect -- a reduction of vacancy rates as a result of controls.

Taking the results of Table 16 at face value, it appears that controls of type B tend to be associated with lower vacancy rates, while those of type C do not. That can be seen from a comparison

of tests C7 and C8 with C9 and C10. Further evidence that the effects of the two regimes are different is provided by tests C5 and C6. However, we continue to be sceptical about the results for particular types of regime, given especially the concentration of type C observations in only two provinces. We feel more comfortable with the results for tests in which the regimes are combined, in spite of the apparent indication of differences between the effects of the two regimes. Test results for the combined regimes (C1 to C4) do not indicate any significant effects.

Hypothesis Category D: Proportion of Renter Households (Table 17)

The tests of whether the proportion of renter households is affected by controls suggest that regime type B may have some effect (a positive one, based on the estimated regression coefficient), but there is no corresponding evidence that regime type C has. Compare the relatively small P-values for tests D7 and D8, which relate to B regimes, with the relatively high ones for D9 and D10, which relate to C regimes. The tests for effects when the regimes are combined also suggest some effect (again positive); see tests D1 to D4. The combined effect would appear to be attributable to the contribution of regime type B, given the results of the other tests -- but once again we are sceptical about the ability to distinguish between regimes, given the available data.

It is well to keep in mind that these tests do not refer to household preferences, as such. The proportion of renter households is determined by the availability or supply of rental units, as well as factors on the demand side of the market.

Hypothesis Category E: Conversions (Table 18)

A glance at Table 18 suggests immediately that there is no evidence of any rent control effects on rates of conversion (from singles to multiples). The P-values are generally quite high. There is no indication that conversions are influenced by controls of either type. It is to be emphasized, however, that because suitable data were not available, it was not possible to test for the (possibly more important) effect of rental controls on conversions from multiples to singles (especially rented apartments to owner-occupied condominiums).

Hypothesis Category F: Rental Units in Need of Major Repairs (Table 19)

Again there is no evidence that rent controls have any appreciable effect: the P-values are again generally high, or at least above conventional levels of significance. We note that the observation period is much shorter (only five years) and the number of observations available for estimating the equation on which the tests are based much smaller here than in the case of the other test categories. However, on the basis of the data that are available it seems safe to say that no evidence exists to the effect that a larger proportion of rental units are in need of major repairs when there are controls (of either kind) than when there are not.

A word of caution is warranted. With the data source used, whether buildings are recorded as in need of major repair reflects the perceptions that tenants have. A possible concern is that tenants might not be aware of the need for some types of major repairs; one might expect landlords to be better informed about

structural repairs, for example. However, the important point for present purposes is that there is no evidence that tenants' perceptions of the apparent need for such repairs differed in systematic ways between jurisdictions where rent controls were in effect and those where they were not.

## 12. SUMMARY INTERPRETATION OF THE RESULTS

We stand back now from the details of the tests reported in the previous section and conclude our report by giving a nontechnical summary of our interpretation of the results. In doing so we take account of the numerical values of the test statistics, of course, but also of our judgements as to the quality of the data and the reasonableness of the theoretical models on which the tests are based.

(1) The data that we have used and the tests that we have carried out provide no evidence to suggest that rent controls reduce the rate of increase of rents in the long run.

(2) There is some evidence to suggest that controls cause rents to rise more rapidly than they would otherwise in periods when the rental market is "soft," especially when the control regime is of type B, "rent control with (mandatory) review." If the long-run rate of increase is unaffected, that would imply that under controls rent increases must be less rapid in periods of market "tightness." However, there is no significant evidence that that is the case. We are inclined therefore to discount the evidence of "soft" period effects, and emphasize the finding of no long-run effects. Given the practical difficulties of defining market "softness" and

"tightness," that seems to us the more credible finding.

(3) There is no evidence that the responsiveness of apartment unit starts to vacancy rates is reduced by the imposition of controls.

(4) There is no evidence that the responsiveness of apartment unit starts to the level of rents (relative to the general price level) is reduced by the imposition of controls.

(5) There is some apparent evidence that the responsiveness of apartment unit starts to changes in rents is reduced by the imposition of controls of type C, "(voluntary) rent arbitration." However, that evidence is based on what we judge to be an implausible theoretical model of the determination of starts. Also, the model does not fit the data well, and the data do not make it possible to establish with confidence the distinction between control effects and effects associated simply with the characteristics of the two provinces in which type C controls are currently in force, namely Quebec and Newfoundland. We are therefore of the opinion that (4) is the more credible conclusion with regard to the effect of rents on starts.

(6) The formal statistical evidence suggests that type B controls tend to be associated with lower apartment unit vacancy rates. However, there is no evidence of that for type C controls, or when types B and C are combined into a single rent control regime. Given the practical statistical difficulties in trying to establish the effects of a particular type of regime, we are inclined to emphasize the lack of effects when the distinction between B and C is ignored. (It

may be noted that the lack of effects on vacancy rates is consistent with the findings of a study using longitudinal data from the New York City Housing and Vacancy Survey, as reported by Rapaport (1992)).

(7) There is some evidence that controls are associated with a higher proportion of renter households. (That should not be taken simply as evidence that household preferences for renting have increased, although that may be the case; the proportion of renters depends also on the availability of rental units.) The evidence is present for type B controls and for types B and C combined, but not for type C alone. In light of our scepticism about the reliability of distinctions between the two types we would again attach greater emphasis to the evidence obtained when the two are combined.

(8) There is no evidence that rent controls affect the rate of conversion of single housing units into multiple units.

(9) There is no evidence that rent controls increase the proportion of occupied rental dwellings that are in need of major repairs.

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## LIST OF TABLES

TABLE 1: STATEMENT OF HYPOTHESES IN A FORM SUITABLE FOR STATISTICAL TESTING

| Category | Null hypothesis | Alternative hypothesis |
|---|---|---|
| 1. Rents | (a) The long-run average rate of increase of rents is unaffected by regulations. | The long-run average rate of increase of rents is lowered by regulations. |
| | (b) The rate of increase of rents is unaffected by regulations in periods of either strong or weak upward pressure on rents. | The rate of increase of rents is lower in periods of strong upward pressure and/or higher in periods of weak upward pressure under regulations. |
| | (c) The rate of increase of rents is unaffected by regulations in periods of strong upward pressure on rents. | The rate of increase of rents is lower in periods of strong upward pressure under regulations. |
| 2. Housing Starts | (a) The responsiveness of rental-unit housing starts to the levels of vacancy rates and rents is unaffected by regulations. | The responsiveness of rental-unit housing starts is diminished with respect to the levels of vacancy rates and/or rents under regulations. |
| | (b) The responsiveness of rental-unit housing starts to the level of vacancy rates is unaffected by regulations. | The responsiveness of rental-unit housing starts is diminished with respect to the level of vacancy rates under regulations. |
| | (c) The responsiveness of rental-unit housing starts to the level of rents is unaffected by regulations | The responsiveness of rental-unit housing starts is diminished with respect to the level of rents under regulations. |
| | (d) The responsiveness of rental-unit housing starts to changes in vacancy rates and rents is unaffected by regulations. | The responsiveness of rental-unit housing starts is diminished with respect to changes in vacancy rates and/or rents under regulations. |
| | (e) The responsiveness of rental-unit housing starts to changes in vacancy rates in unaffected by regulations. | The responsiveness of rental-unit housing starts is diminished with respect to changes in vacancy rates under regulations. |
| | (f) The responsiveness of rental-unit housing starts to changes in rents is unaffected by regulations. | The responsiveness of rental-unit housing starts is diminished with respect to changes in rents under regulations. |
| 3. Vacancy Rates | (a) Rental vacancy rates are unaffected by regulations. | Rental vacancy rates are lowered by regulations. |
| 4. Property Values | (a) The long-run rate of increase of rental property values is unaffected by regulations. | (1) The long-run rate of increase of rental property values is lowered by regulations; (2) the rate of increase is altered (increased or decreased) by regulations. |
| 5. Tenure Preferences | (a) Household preferences for renting are unaffected by regulations. | (1) Household preferences for renting are increased by regulations; (2) household preferences are altered (increased or decreased) by regulations. |
| 6. Conversions | (a) Rates of conversion of rental units to owner units are unaffected by regulations. | (1) Rates of conversion of rental units to owner units are higher under regulations; (2) rates of conversion are different (higher or lower) under regulations. |
| 7. Maintenance and Repairs | (a) Maintenance, repairs, and the provision of services in rental units are unaffected by regulations | Maintenance, repairs and the provision of services in rental units are decreased under regulations. |

TABLE 2: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, NEWFOUNDLAND

| | |
|---|---|
| Current System | Voluntary review |
| Rent Regulation First Effective | 1973 |
| Most Recent Legislation | Residential Tenancies Act, 1989<br>Revised June, 1992 |
| Expiry Date | Permanent |
| Exemptions | Public housing; employee/employer relationship |
| Controls on Demolition or Conversion | Court approval required for group eviction - provision never used |
| Increases Allowed per Year | One automatic, further on application |
| Notice of Previous Rent Required? | No |
| Rent Registry? | No |
| Rent Control | |
| Guideline Increase | Not applicable |
| Based on Index? | Not applicable |
| Rent Review | |
| Mandatory or Voluntary | Voluntary |
| Tenant Can Initiate | For all increases |
| Landlord Can Initiate | Not applicable |
| Focus of Review | Rent level ("fair rent") |
| Factors Considered | Operating costs, maintenance, service and quality, return on investment, comparable rents, capital replacement reserve |
| Formula Applied? | Not regularly |
| Administration | |
| Agency | Residential Tenancies Board |
| Ministry | Justice |
| Appeals to | Supreme Court of Newfoundland |

Note: The information in Tables 2-11 has been compiled from a number of secondary sources, supplemented with personal communications with individuals in seven provinces. Rent regulation legislation is usually complex and these tables do not cover all the details and special cases. A definitive survey would require a search of each province's legal records.

TABLE 3: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, PRINCE EDWARD ISLAND

| | |
|---|---|
| Current System | Rent control plus review |
| Legislation | |
| Rent Regulation First Effective | 14 Oct 1975 |
| Current or Most Recent Legislation | Rental of Residential Property Act, 1988 |
| Expiry Date | Permanent |
| Exemptions | First rent on new construction, educational premises; community/nursing homes; innkeepers; therapeutic homes; coop housing; premises normally occupied by owner and rented for less than 7 months |
| Controls on Demolition or Conversion | No |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | No, but false information gives ground for appeal |
| Rent Registry? | No |
| Rent Control | |
| Guideline Increase | 1984-87: 3%; 1988-90:4%; 1991:4.5%; 1992: 3%;1993:1% |
| Based on Index? | No. Determined by Island Regulatory and Appeals Commission |
| Rent Review | |
| Mandatory or Voluntary | Mandatory |
| Tenant Can Initiate | For all increases |
| Landlord Can Initiate | For increases above guideline |
| Focus of Review | Rent increase, but return on equity is considered |
| Factors Considered | Operating costs; maintenance; mortgage interest and principal; capital improvements; financial loss; reasonable return on equity |
| Formula Applied? | No. Return on equity is inflation plus a factor determined by the Commission |
| Administration | |
| Agency | Land and Property Section of Island Regulatory and Appeals Commission |
| Ministry | None |
| Appeals to | The Commission; Supreme Court on matters of law |

Note: See note to Table 2.

TABLE 4: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, NEW BRUNSWICK

| | |
|---|---|
| Current System | No regulation |
| Legislation | |
| Rent Regulation first effective | October, 1975; 31 August, 1982 |
| Current or Most Recent Legislation | Residential Rent Review Act, 1983 |
| Expiry Date | [30 June, 1979]; 31 August 1985 |
| Exemptions | New construction after 1982; public, non-profit housing; boarding houses; business premises with attached accomodation; initial increase after major renovation |
| Controls on Demolition or Conversion | No |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | No |
| Rent Control | |
| Guideline Increase | 1982: 6%; 1983: 5%; 1984-85: 6% |
| Based on Index? | No |
| Rent Review | |
| Mandatory or Voluntary | Voluntary |
| Tenant Can Initiate | For increases above guideline |
| Landlord Can Initiate | Not applicable |
| Focus of Review | Rent increase |
| Factors Considered | Reasonable expenses |
| Formula Applied? | No |
| Administration | |
| Agency | Rentalsman |
| Ministry | Justice |
| Appeals to | Chief rentalsman; Queen's Bench on points of law |

Note: See note to Table 2.

TABLE 5: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, NOVA SCOTIA

| | |
|---|---|
| Current System | Control plus review |
| Legislation | |
| Rent Regulation First Effective | 1 Oct 1975 |
| Current or Most Recent Legislation | Rent Review Act, 1975<br>revisions effective 1 Jan 1990 |
| Expiry Date | Permanent |
| Exemptions | New construction for 4 yrs; public and non-profit; residential rentals with 2 units where owner occupies 1 (since 1990); single family dwellings (sfd) when owner has only 1 sfd for rent (since 1990) |
| Controls on Demolition or Conversion | No |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | No |
| Rent Registry? | No. File available to landlord and tenant only |
| Rent Control | |
| Guideline Increase | 1976: 8%; 1977-78: 6%; 1979-82: 4%; 1983-84: 6%; 1985: 5%; 1986: 4%; 1987-90: 3%; 1991: 4%; 1992: 3%; 1993: 0% |
| Based on Index? | No |
| Rent Review | |
| Mandatory or Voluntary | Mandatory |
| Tenant Can Initiate for Increases Below Guideline | No |
| Landlord Can Initiate | All rent increases must be filed. Any increase below guideline is automatically approved. |
| Focus of Review | Rent level |
| Factors Considered | Operating costs; maintenance; mortgage interest; service and quality; return on equity not permitted to fall below previous levels |
| Formula Applied? | Yes |
| Administration | |
| Agency | Rent review division |
| Ministry | Consumer Affairs and Housing |
| Appeals to | Rent Review Commission |

Note: See note to Table 2.

## TABLE 6: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, QUEBEC

| | |
|---|---|
| **Current System** | Voluntary review |
| **Legislation** | |
| Rent Regulation First Effective | 1951 |
| Current or Most Recent Legislation | Act respecting the Regie du Logement, 1979 |
| Expiry Date | Permanent |
| Exemptions | New construction for 5 years; dwellings erected under Acts to facilitate slum clearance in Montreal; low rental housing; cooperatives |
| Controls on Demolition or Conversion | Must be approved by Regie or local committee |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | No |
| **Rent Control** | |
| Guideline Increase | Not applicable |
| Based on Index? | Not applicable |
| **Rent Review** | |
| Mandatory or Voluntary | Voluntary |
| Tenant Can Initiate | Tenant may refuse rent increase |
| Landlord Can Initiate | Landlord then appeals to Regie for determination of rent |
| Focus of Review | Rent increase |
| Factors Considered | Components of operating cost and net income inflated adjusted separately to obtain permitted increase |
| Formula Applied? | Yes |
| **Administration** | |
| Agency | Regie du logement |
| Ministry | Municipal Affairs |
| Appeals (on matters of rent) | Review by Regie du logement |

Note: See note to Table 2.

TABLE 7: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, ONTARIO

| | |
|---|---|
| Current System | Control |
| Legislation | |
| Rent Regulation First Effective | 29 July 1975 |
| Most Recent Legislation | Rent Control Act, 1992 |
| Expiry Date | Permanent |
| Exemptions | 5 year exemption on buildings not occupied before 1 Nov 1991; dwellings where owner or family shares bathroom or kitchen with tenant; transient and seasonal; accommodation conditional on employment; institutional and educational; non-profit |
| Controls on Demolition or Conversion | Yes. Rental Housing Protection Act |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | Yes |
| Rent Control | |
| Guideline Increase | Up to Aug,1985: 6%; Dec.,1986:4%; 1987:5.2%; 1988: 4.7%; 1989:4.6%; 1990:4.6%; 1991: 5.4%; 1992:6.0%; 1993: 4.9% |
| Based on Index? | Yes |
| Rent Review | |
| Mandatory or Voluntary | Mandatory |
| Tenant Can Initiate for Increases Below Guideline | Yes; for reduction in service or illegal rent and charges |
| Landlord Can Initiate | Yes, for increases above guideline |
| Focus of Review | Rent increase |
| Factors Considered | Extraordinary increases in electricity, water, municipal taxes |
| Formula Applied? | No (?) |
| Administration | |
| Agency | Rent Control Program offices |
| Ministry | Housing |
| Appeals to | Divisional Court on points of law |

Note: See note to Table 2.

## TABLE 8: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, MANITOBA

| | |
|---|---|
| Current System | Control plus review |
| Legislation | |
| Rent Regulation First Effective | 1 July 1975 |
| Current or Most Recent Legislation | Residential Tenancies Act, 1992 (effective 1 Sept) |
| Expiry Date | Permanent |
| Exemptions | New construction for 5 yrs; educational, therapeutic, rehabilitative, penal, transient and seasonal; boarding houses; non-profit, co-op, limited dividend and public housing; businesses with single attached unit used for purposes of the business |
| Controls on Demolition or Conversion | No |
| Increases Allowed Per Year | One |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | Yes |
| Rent Control | |
| Guideline Increase | 1982: 9%; 1983: 8%; 1984: 6%; 1985: 4.5%; 1986-90: 3%; 1991: 4%; 1992: 3%; 1993: 1% |
| Based on Index? | No |
| Rent Review | |
| Mandatory or Voluntary | Mandatory |
| Tenant Can Initiate | Yes, for any rate increase |
| Landlord Can Initiate | Must file all increases above guideline |
| Focus of Review | Rent increase |
| Factors Considered | Operating costs, maintenance, mortgage interest; capital improvements; service and quality; economic adjustment factor (guideline or 1/3 of operating deficit) |
| Formula Applied? | Yes |
| Administration | |
| Agency | Residential Tenancies Branch |
| Ministry | Consumer and Corporate Affairs |
| Appeals to | Residential Tenancies Commission; Manitoba Court of Appeal on points of law or jurisdiction |

Note: See note to Table 2.

TABLE 9: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, SASKATCHEWAN

| | |
|---|---|
| Current System | No control |
| Legislation | |
| Rent Regulation First Effective | 19 October, 1974 |
| Current or Most Recent Legislation | Residential Tenancies Act, 1992 |
| Expiry Date | 1 October 1992 |
| Exemptions | Since 1984, all leaseholds. Monthly tenancies exempted in new construction for 4 yrs; centres under 2000 population; landlord occupied with 1 rental unit; educational and special care housing |
| Controls on Demolition or Conversion | No |
| Increases Allowed per Year | One under previous legislation<br>Current legislation allows any increases after 3 months notice |
| Notice of Previous Rent Required? | No. Previous legislation applied to tenancies, not dwellings |
| Rent Registry? | No |
| Rent Control | |
| Guideline Increase | Not applicable |
| Based on Index? | Not applicable |
| Rent Review | (Pre-1992 legislation) |
| Mandatory or Voluntary | Voluntary |
| Tenant Can Initiate | Yes, after receiving notice of increase |
| Landlord Can Initiate | No need |
| Focus of Review | Rent increase. Tenant could appeal level |
| Factors Considered | Operating costs, maintenance, capital improvements, service and quality |
| Formula Applied? | Yes |
| Administration | |
| Agency | Rentalsman, Provincial Mediation Board |
| Ministry | Justice |
| Appeals to | Rent Appeal Commission |

Note: See note to Table 2.

TABLE 10: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, ALBERTA

| | |
|---|---|
| Current System | No control |
| Legislation | |
| Rent Regulation First Effective | 1 January 1976 |
| Current or Most Recent Legislation | Rent Decontrol Act, 1977 |
| Expiry Date | July, 1980 |
| Exemptions | New construction after 1 Jan 1976; public housing. After 1 Jan 1977 rent level decontrol at $375 3 bed, $325 2 bed, $275 1 bed |
| Controls on Demolition or Conversion | Not available |
| Increases Allowed per Year | One |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | No |
| Rent Control | |
| Guideline Increase | 1976-77: 10%; 1978-80: 8% |
| Based on Index? | No |
| Rent Review | |
| Mandatory or Voluntary | Mandatory |
| Tenant Can Initiate for Increases Below Guideline | Apparently not |
| Landlord Can Initiate | Yes, for above guideline |
| Focus of Review | Rent increase |
| Factors Considered | Operating costs and maintenance; other details not available |
| Formula Applied? | Not available |
| Administration | |
| Agency | Rent Regulation Appeal Board, subsequently Rent Decontrol Appeal Board |
| Ministry | Attorney-General |
| Appeals to | Appeal Board; Supreme Court |

Note: See note to Table 2.

TABLE 11: SUMMARY OF PROVINCIAL RENT REGULATION, JANUARY 1993, BRITISH COLUMBIA

| | |
|---|---|
| **Current System** | No control |
| **Legislation** | |
| Rent Regulation First Effective | 1 January 1976 |
| Current or Most Recent Legislation | Rent Regulation Act, 1982 (amended 1983) |
| Expiry Date | Rent control: 31 Aug 1983; appeal of excessive increases: 31 August 1984 |
| Exemptions | New construction for 5 years (forever after 1977); public, non-profit, educational housing; landlord occupied 2 unit premises; rent level decontrol after 1979 |
| Controls on Demolition or Conversion | Not available |
| Increases Allowed per Year | One. Vacancy decontrol for units subject to review |
| Notice of Previous Rent Required? | Yes |
| Rent Registry? | |
| **Rent Control** | |
| Guideline Increase | 10% for controlled units plus up to 18% of capital improvements |
| Based on Index? | No |
| **Rent Review** | |
| Mandatory or Voluntary | Mandatory to 1983 for controlled units; voluntary to 1984 for reviewed units |
| Tenant Can Initiate for Increases Below Guideline | Yes |
| Landlord Can Initiate | Yes, must file all rent increases |
| Focus of Review | Rent increase |
| Factors Considered | Operating cost, maintenance, mortgage interest and principal, capital improvements, service and quality, comparable rents |
| Formula Applied? | Not available |
| **Administration** | |
| Agency | Rentalsman |
| Ministry | Consumer and Corporate Affairs |
| Appeals to | Rent Review Commission; Supreme Court |

Note: See note to Table 2.

**TABLE 12: OVERVIEW OF RENT REGULATIONS BY PROVINCE AND YEAR**

| | NFLD | PEI | NS | NB | QUE | ONT | MAN | SASK | ALTA | BC |
|---|---|---|---|---|---|---|---|---|---|---|
| 1971 | BB01 | AA0 | CC0 | AA0 | CC0 | AA0 | AA0 | AA0 | AA0 | AA0 |
| 1972 | BB01 | AA0 | CC0 | AA0 | CC0 | AA0 | AA0 | AA0 | AA0 | AA0 |
| 1973 | CC1 | AA0 | CC0 | AA0 | CC0 | AA0 | AA0 | AA0 | AA0 | AA0 |
| 1974 | CC1 | AA0 | CC0 | AA0 | CC0 | AA0 | AA0 | AB15 | AA0 | BB1 |
| 1975 | CC0 | AB1 | CB1 | AB1 | CC0 | AB1 | AB15 | BB15 | AA0 | BB1 |
| 1976 | CC0 | BB1 | BB1 | BB1 | CC0 | BB1 | BB15 | BB0 | BB1 | BB0 |
| 1977 | CC0 | BB0 | BB0 | BB02 | CC0 | BB0 | BB0 | BB12 | BB12 | BB02 |
| 1978 | CC0 | BB0 | BB0 | BB02 | CC0 | BB0 | BB02 | BB12 | BB02 | BB02 |
| 1979 | CC0 | BB0 | BB0 | BA12 | CC0 | BB0 | BB02 | BB02 | BB02 | BB02 |
| 1980 | CC0 | BB0 | BB0 | AA1 | CC0 | BB0 | BA1 | BB0 | BA12 | BB07 |
| 1981 | CC0 | BB0 | BB0 | AA0 | CC0 | BB0 | AA1 | BB0 | AA1 | BC17 |
| 1982 | CC0 | BB0 | BB0 | AB1 | CC0 | BB0 | BB1 | BB0 | AA0 | BB07 |
| 1983 | CC0 | BB0 | BB0 | BB1 | CC0 | BB0 | BB1 | BB0 | AA0 | BC17 |
| 1984 | CC0 | BB0 | BB0 | BB0 | CC0 | BB0 | BB0 | BA16 | AA0 | CA16 |
| 1985 | CC0 | BB0 | BB0 | BA1 | CC0 | BB0 | BB0 | AA16 | AA0 | AA1 |
| 1986 | CC0 | BB0 | BB0 | AA1 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1987 | CC0 | BB0 | BB0 | AA0 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1988 | CC0 | BB0 | BB0 | AA0 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1989 | CC0 | BB0 | BB0 | AA0 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1990 | CC0 | BB0 | BB0 | AA0 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1991 | CC0 | BB0 | BB0 | AA0 | CC0 | BB03 | BB0 | AA06 | AA0 | AA0 |
| 1992 | CC0 | BB0 | BB0 | AA0 | CC0 | BB04 | BB0 | AA06 | AA0 | AA0 |
| 1993 | CC0 | BB0 | BB0 | AA0 | CC0 | BB04 | BB0 | AA0 | AA0 | AA0 |

EXPLANATION OF CODES

XXXX

I I I I--- 4th place: Special Notes

I I I---- 3rd place: New Regime Indicator

I I----- 2nd place: Rent Regulation Regime at end of year

I------ 1st place: Rent Regulation Regime at beginning of year

RENT REGULATION CODES:

A: NO RENT REGULATION

B: RENT CONTROL WITH REVIEW (MANDATORY)

C: RENT ARBITRATION (VOLUNTARY)

NEW REGIME INDICATOR:

1: Regime change in current or previous calendar year

0: otherwise

SPECIAL NOTES

1: Rent Restriction Act, 1943. Details unknown.

2: Gradual Decontrol of some form during part or all of year

3: Major relaxation of controls under Liberal gov't

4: Major tightening of controls under NDP

5: Retroactive controls during part or all of year

6: Arbitration for month to month tenancies during all or part of year

7: Rent Control on units with low rent; appeal of excessive increases for others

8: No Control; appeal of excessive increases permitted

## NOTES FOR TABLE 13

HYPOTHESIS CATEGORY A: RENTS

FORM OF EQUATION USED FOR TESTING:

$$(8)\quad r_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2} + \beta_{20}y_{it} + \beta_{21}y_{i,t-1}$$

$$+ \beta_{22}y_{i,t-2} + \beta_{30}p_{it} + \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2}$$

$$+ \beta_{40}\pi_{t} + \beta_{41}\pi_{t-1} + \beta_{42}\pi_{t-2} + \gamma_{1}r_{i,t-1} + \gamma_{2}r_{i,t-2}$$

$$+ \phi_{1}B^{+}_{it} + \phi_{2}B^{-}_{it} + \theta_{1}C^{+}_{it} + \theta_{2}C^{-}_{it} + \varepsilon_{it}$$

METHOD OF ESTIMATION: Ordinary least squares

TYPE OF DATA: Pooled annual time-series for 16 metropolitan areas

NUMBER OF OBSERVATIONS: 192

SUMMARY DEFINITIONS OF VARIABLES:

| | |
|---|---|
| $r$ | -- annual rent increase (%) |
| $n$ | -- annual population growth (%) |
| $y$ | -- annual increase in real personal disposable income per capita (%) |
| $p$ | -- annual inflation rate (%) |
| $\pi$ | -- annual rate of interest |
| $B^{+}$ | -- control regime type B, strong upward pressure (zero-one variable) |
| $B^{-}$ | -- control regime type B, weak upward pressure (zero-one variable) |
| $C^{+}$ | -- control regime type C, strong upward pressure (zero-one variable) |
| $C^{-}$ | -- control regime type C, weak upward pressure (zero-one variable) |
| $\varepsilon$ | -- random error |

SUBSCRIPTS: i for metropolitan area, t for year

TABLE 13: TESTS OF HYPOTHESES RELATING TO CATEGORY A: RENTS

| Test | Restrictions Imposed | Restrictions Tested: Null | Restrictions Tested: Alternative | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| A1 | $\phi_1=\phi_2=\theta_1=\theta_2$ | $\phi_1=0$ | $\phi_1<0$ | S | t | .696 |
| A2 | $\phi_1=\phi_2=\theta_1=\theta_2$ | $\phi_1=0$ | $\phi_1<0$ | R | $\phi_1$ | .706 |
| A3 | $\phi_1^*=\phi_2^*=\theta_1^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*<0$ | S | t | .670 |
| A4 | $\phi_1^*=\phi_2^*=\theta_1^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*<0$ | R | $\phi_1^*$ | .705 |
| A5 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | S | F | .191 |
| A6 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | R | $\lvert\phi_1\rvert+\lvert\phi_2\rvert$ | .265 |
| A7 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | S | Wald | .173 |
| A8 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | R | $\lvert\phi_1^*\rvert+\lvert\phi_2^*\rvert$ | .254 |
| A9 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1<0$ | S | t | .221 |
| A10 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1<0$ | R | $\phi_1$ | .212 |
| A11 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*<0$ | S | t | .225 |
| A12 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*<0$ | R | $\phi_1^*$ | .209 |
| A13 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_2=0$ | $\phi_2>0$ | S | t | .104 |
| A14 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_2^*=0$ | $\phi_2^*>0$ | S | t | .096 |
| A15 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2$ | $\phi_1\neq\phi_2$ | S | t | .081 |
| A16 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2$ | $\phi_1\neq\phi_2$ | R | $\lvert\phi_1-\phi_2\rvert$ | .067 |

| | | | | | | |
|---|---|---|---|---|---|---|
| A17 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*$ | $\phi_1^*\neq\phi_2^*$ | S | t | .079 |
| A18 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*$ | $\phi_1^*\neq\phi_2^*$ | R | $\lvert\phi_1^*-\phi_2^*\rvert$ | .059 |
| A19 | $\phi_1=\phi_2;\theta_1=\theta_2$ | $\phi_1=\theta_1$ | $\phi_1\neq\theta_1$ | S | t | .764 |
| A20 | $\phi_1^*=\phi_2^*;\theta_1^*=\theta_2^*$ | $\phi_1^*=\theta_1^*$ | $\phi_1^*\neq\theta_1^*$ | S | t | .765 |
| A21 | none | $\phi_1=\phi_2=\theta_1=\theta_2$ | any violation of null | S | F | .335 |
| A22 | none | $\phi_1^*=\phi_2^*=\theta_1^*=\theta_2^*$ | any violation of null | S | Wald | .326 |
| A23 | $\phi_1=\phi_2;\theta_1=\theta_2$ | $\phi_1=\theta_1=0$ | $\phi_1$ and/or $\theta_1\neq0$ | S | F | .838 |
| A24 | $\phi_1^*=\phi_2^*;\theta_1^*=\theta_2^*$ | $\phi_1^*=\theta_1^*=0$ | $\phi_1^*$ and/or $\theta_1^*\neq0$ | S | Wald | .833 |
| A25 | $\phi_1=\phi_2;\theta_1=\theta_2$ | $\phi_1=0$ | $\phi_1<0$ | S | t | .720 |
| A26 | $\phi_1^*=\phi_2^*;\theta_1^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*<0$ | S | t | .723 |
| A27 | $\phi_1=\phi_2;\theta_1=\theta_2$ | $\theta_1=0$ | $\theta_1<0$ | S | t | .618 |
| A28 | $\phi_1^*=\phi_2^*;\theta_1^*=\theta_2^*$ | $\theta_1^*=0$ | $\theta_1^*<0$ | S | t | .619 |
| A29 | none | $\phi_1=\theta_1=0$ | $\phi_1$ and/or $\theta_1\neq0$ | S | F | .621 |
| A30 | none | $\phi_1^*=\theta_1^*=0$ | $\phi_1^*$ and/or $\theta_1^*\neq0$ | S | Wald | .630 |
| A31 | none | $\phi_2=\theta_2=0$ | $\phi_2$ and/or $\theta_2\neq0$ | S | F | .443 |
| A32 | none | $\phi_2^*=\theta_2^*=0$ | $\phi_2^*$ and/or $\theta_2^*\neq0$ | S | Wald | .413 |
| A33 | none | $\phi_1=\phi_2=\theta_1=\theta_2=0$ | any violation of null | S | F | .453 |
| A34 | none | $\phi_1^*=\phi_2^*=\theta_1^*=\theta_2^*=0$ | any violation of null | S | Wald | .424 |

| | | | | | | |
|---|---|---|---|---|---|---|
| A35 | none | $\phi_1=0$ | $\phi_1<0$ | S | t | .298 |
| A36 | none | $\phi_1=0$ | $\phi_1<0$ | R | $\phi_1$ | .292 |
| A37 | none | $\phi_1^*=0$ | $\phi_1^*<0$ | S | t | .300 |
| A38 | none | $\phi_1^*=0$ | $\phi_1^*<0$ | R | $\phi_1^*$ | .292 |
| A39 | none | $\theta_1=0$ | $\theta_1<0$ | S | t | .166 |
| A40 | none | $\theta_1=0$ | $\theta_1<0$ | R | $\theta_1$ | .156 |
| A41 | none | $\theta_1^*=0$ | $\theta_1^*<0$ | S | t | .170 |
| A42 | none | $\theta_1^*=0$ | $\theta_1^*<0$ | R | $\theta_1^*$ | .156 |
| A43 | none | $\phi_2=0$ | $\phi_2>0$ | S | t | .118 |
| A44 | none | $\phi_2=0$ | $\phi_2>0$ | R | $\phi_2$ | .111 |
| A45 | none | $\phi_2^*=0$ | $\phi_2^*>0$ | S | t | .112 |
| A46 | none | $\phi_2^*=0$ | $\phi_2^*>0$ | R | $\phi_2^*$ | .110 |
| A47 | none | $\theta_2=0$ | $\theta_2>0$ | S | t | .152 |
| A48 | none | $\theta_2=0$ | $\theta_2>0$ | R | $\theta_2$ | .143 |
| A49 | none | $\theta_2^*=0$ | $\theta_2^*>0$ | S | t | .143 |
| A50 | none | $\theta_2^*=0$ | $\theta_2^*>0$ | R | $\theta_2^*$ | .140 |

## NOTES FOR TABLE 14

HYPOTHESIS CATEGORY BA: Housing starts (tests for "levels" effects)

FORM OF EQUATION USED FOR TESTING:

$$(9)\quad (S_a/N)_{it} = \alpha + (\beta_1+\phi_1 B_{it}+\theta_1 C_{it})v^*_{it} + (\beta_2+\phi_2 B_{it}+\theta_2 C_{it})(R/P)_{it}$$

$$+ \gamma(S_a/N)_{i,t-1} + (\text{MA terms}) + \varepsilon_{it}$$

where $v^* = \ln(v/(1-v))$.

METHOD OF ESTIMATION: Instrumental variables

TYPE OF DATA: Pooled annual time series for 15 metropolitan areas

NUMBER OF OBSERVATIONS: 182

SUMMARY DEFINITIONS OF VARIABLES:

| | |
|---|---|
| $S_a$ | -- starts of apartment units |
| N | -- population |
| v | -- vacancy rate |
| R | -- rent index |
| P | -- general price index |
| B | -- control regime type B (zero-one variable) |
| C | -- control regime type C (zero-one variable) |
| MA | -- metropolitan area (zero-one variable; one for each area) |
| $\varepsilon$ | -- random error |

SUBSCRIPTS: i for metropolitan area, t for year

TABLE 14: TESTS OF HYPOTHESES RELATING TO CATEGORY BA: HOUSING STARTS (BASED ON "LEVELS" EQUATION)

| Test | Restrictions Imposed | Restrictions Tested | | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| | | Null | Alternative | | | |
| BA1 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1>0$ | S | t | .411 |
| BA2 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1>0$ | R | $\phi_1$ | .414 |
| BA3 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*>0$ | S | t | .590 |
| BA4 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*>0$ | R | $\phi_1^*$ | .603 |
| BA5 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_2=0$ | $\phi_2<0$ | S | t | .589 |
| BA6 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_2=0$ | $\phi_2<0$ | R | $\phi_2$ | .581 |
| BA7 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_2^*=0$ | $\phi_2^*<0$ | S | t | .396 |
| BA8 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_2^*=0$ | $\phi_2^*<0$ | R | $\phi_2^*$ | .382 |
| BA9 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | S | Wald | .975 |
| BA10 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | R | $\lvert\phi_1\rvert+\lvert\phi_2\rvert$ | .815 |
| BA11 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | S | Wald | .951 |
| BA12 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | R | $\lvert\phi_1^*\rvert+\lvert\phi_2^*\rvert$ | .787 |
| BA13 | none | $\phi_1=\theta_1$ | $\phi_1\neq\theta_1$ | S | t | .926 |
| BA14 | none | $\phi_1=\theta_1$ | $\phi_1\neq\theta_1$ | R | $\lvert\phi_1-\theta_1\rvert$ | .923 |
| BA15 | none | $\phi_1^*=\theta_1^*$ | $\phi_1^*\neq\theta_1^*$ | S | t | .907 |

| | | | | | | |
|---|---|---|---|---|---|---|
| BA16 | none | $\phi_1^*=\theta_1^*$ | $\phi_1^*\neq\theta_1^*$ | R | $\lvert\phi_1^*-\theta_1^*\rvert$ | .901 |
| BA17 | none | $\phi_2=\theta_2$ | $\phi_2\neq\theta_2$ | S | t | .801 |
| BA18 | none | $\phi_2=\theta_2$ | $\phi_2\neq\theta_2$ | R | $\lvert\phi_2-\theta_2\rvert$ | .785 |
| BA19 | none | $\phi_2^*=\theta_2^*$ | $\phi_2^*\neq\theta_2^*$ | S | t | .750 |
| BA20 | none | $\phi_2^*=\theta_2^*$ | $\phi_2^*\neq\theta_2^*$ | R | $\lvert\phi_2^*-\theta_2^*\rvert$ | .728 |
| BA21 | none | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1\neq\theta_1$ and/or $\phi_2\neq\theta_2$ | S | Wald | .884 |
| BA22 | none | $\phi_1=\theta_1;\theta_2=\theta_2$ | $\phi_1\neq\theta_1$ and/or $\phi_2\neq\theta_2$ | R | $\lvert\phi_1-\theta_1\rvert+\lvert\phi_2-\theta_2\rvert$ | .838 |
| BA23 | none | $\phi_1^*=\theta_1^*;\theta_2^*=\theta_2^*$ | $\phi_1^*\neq\theta_1^*$ and/or $\phi_2^*\neq\theta_2^*$ | S | Wald | .822 |
| BA24 | none | $\phi_1^*=\theta_1^*;\theta_2^*=\theta_2^*$ | $\phi_1^*\neq\theta_1^*$ and/or $\phi_2^*\neq\theta_2^*$ | R | $\lvert\phi_1^*-\theta_1^*\rvert+\lvert\phi_2^*-\theta_2^*\rvert$ | .881 |
| BA25 | none | $\phi_1=0$ | $\phi_1>0$ | S | t | .391 |
| BA26 | none | $\phi_1^*=0$ | $\phi_1^*>0$ | S | t | .522 |
| BA27 | none | $\theta_1=0$ | $\theta_1>0$ | S | t | .453 |
| BA28 | none | $\theta_1^*=0$ | $\theta_1^*>0$ | S | t | .573 |
| BA29 | none | $\phi_1=\theta_1=0$ | $\phi_1$ and/or $\theta_1\neq0$ | S | Wald | .960 |
| BA30 | none | $\phi_1^*=\theta_1^*=0$ | $\phi_1^*$ and/or $\theta_1^*\neq0$ | S | Wald | .983 |
| BA31 | none | $\phi_2=0$ | $\phi_2<0$ | S | t | .608 |
| BA32 | none | $\phi_2^*=0$ | $\phi_2^*<0$ | S | t | .469 |

| | | | | | | |
|---|---|---|---|---|---|---|
| BA33 | none | $\theta_2=0$ | $\theta_2<0$ | S | t | .483 |
| BA34 | none | $\theta_2^*=0$ | $\theta_2^*<0$ | S | t | .321 |
| BA35 | none | $\phi_2=\theta_2=0$ | $\phi_2$ and/or $\theta_2\neq0$ | S | Wald | .958 |
| BA36 | none | $\phi_2^*=\theta_2^*=0$ | $\phi_2^*$ and/or $\theta_2^*\neq0$ | S | Wald | .897 |
| BA37 | none | $\phi_1=\theta_1=\phi_2=\theta_2=0$ | any violation of null | S | Wald | .991 |
| BA38 | none | $\phi_1^*=\theta_1^*=\phi_2^*=\theta_2^*=0$ | any violation of null | S | Wald | .971 |

## NOTES FOR TABLE 15

HYPOTHESIS CATEGORY BB: Housing starts (tests for "changes" effects)

FORM OF EQUATION USED FOR TESTING:

$$(11)\quad (S_a/N)_{it} = \alpha + (\beta_1+\phi_1 B_{it}+\theta_1 C_{it})\Delta v^*_{it} + (\beta_2+\phi_2 B_{it}+\theta_2 C_{it})\Delta(R/P)_{it} + \gamma(S_a/N)_{i,t-1} + (\text{MA terms}) + \varepsilon_{it}$$

where $v^*=\ln(v/(1-v))$, $\Delta v^*_{it} = v^*_{it} - v^*_{i,t-1}$, $\Delta(R/P)_{it} = (R/P)_{it} - (R/P)_{i,t-1}$

METHOD OF ESTIMATION: Instrumental variables

TYPE OF DATA: Pooled annual time series for 15 metropolitan area

NUMBER OF OBSERVATIONS: 182

SUMMARY DEFINITIONS OF VARIABLES:

- $S_a$ -- starts of apartment units
- N -- population
- v -- vacancy rate
- R -- rent index
- P -- general price index
- B -- control regime type B (zero-one variable)
- C -- control regime type C (zero-one variable)
- MA -- metropolitan area (zero-one variable; one for each area)
- $\varepsilon$ -- random error

SUBSCRIPTS: i for metropolitan area, t for year

TABLE 15: TESTS OF HYPOTHESES RELATING TO CATEGORY BB: HOUSING STARTS (BASED ON "CHANGES" EQUATION)

| Test | Restrictions Imposed | Restrictions Tested: Null | Restrictions Tested: Alternative | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| BB1 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1>0$ | S | t | .649 |
| BB2 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=0$ | $\phi_1>0$ | R | $\phi_1$ | .680 |
| BB3 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*>0$ | S | t | .849 |
| BB4 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=0$ | $\phi_1^*>0$ | R | $\phi_1^*$ | .857 |
| BB5 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_2=0$ | $\phi_2<0$ | S | t | .094 |
| BB6 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_2=0$ | $\phi_2<0$ | R | $\phi_2$ | .071 |
| BB7 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_2^*=0$ | $\phi_2^*<0$ | S | t | .885 |
| BB8 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_2^*=0$ | $\phi_2^*<0$ | R | $\phi_2^*$ | .894 |
| BB9 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | S | Wald | .308 |
| BB10 | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1=\phi_2=0$ | $\phi_1$ and/or $\phi_2\neq0$ | R | $\lvert\phi_1\rvert+\lvert\phi_2\rvert$ | .152 |
| BB11 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | S | Wald | .365 |
| BB12 | $\phi_1^*=\theta_1^*;\phi_2^*=\theta_2^*$ | $\phi_1^*=\phi_2^*=0$ | $\phi_1^*$ and/or $\phi_2^*\neq0$ | R | $\lvert\phi_1^*\rvert+\lvert\phi_2^*\rvert$ | .205 |
| BB13 | none | $\phi_1=\theta_1$ | $\phi_1\neq\theta_1$ | S | t | .546 |
| BB14 | none | $\phi_1=\theta_1$ | $\phi_1\neq\theta_1$ | R | $\lvert\phi_1-\theta_1\rvert$ | .517 |
| BB15 | none | $\phi_1^*=\theta_1^*$ | $\phi_1^*\neq\theta_1^*$ | S | t | .654 |

| | | | | | | |
|---|---|---|---|---|---|---|
| BB16 | none | $\phi_1^*=\theta_1^*$ | $\phi_1^*\neq\theta_1^*$ | R | $\lvert\phi_1^*-\theta_1^*\rvert$ | .632 |
| BB17 | none | $\phi_2=\theta_2$ | $\phi_2\neq\theta_2$ | S | t | .000 |
| BB18 | none | $\phi_2=\theta_2$ | $\phi_2\neq\theta_2$ | R | $\lvert\phi_2-\theta_2\rvert$ | .000 |
| BB19 | none | $\phi_2^*=\theta_2^*$ | $\phi_2^*\neq\theta_2^*$ | S | t | .000 |
| BB20 | none | $\phi_2^*=\theta_2^*$ | $\phi_2^*\neq\theta_2^*$ | R | $\lvert\phi_2^*-\theta_2^*\rvert$ | .000 |
| BB21 | none | $\phi_1=\theta_1;\phi_2=\theta_2$ | $\phi_1\neq\theta_1$ and/or $\phi_2\neq\theta_2$ | S | Wald | .001 |
| BB22 | none | $\phi_1=\theta_1;\theta_2=\theta_2$ | $\phi_1\neq\theta_1$ and/or $\phi_2\neq\theta_2$ | R | $\lvert\phi_1-\theta_1\rvert+\lvert\phi_2-\theta_2\rvert$ | .000 |
| BB23 | none | $\phi_1^*=\theta_1^*;\theta_2^*=\theta_2^*$ | $\phi_1^*\neq\theta_1^*$ and/or $\phi_2^*\neq\theta_2^*$ | S | Wald | .001 |
| BB24 | none | $\phi_1^*=\theta_1^*;\theta_2^*=\theta_2^*$ | $\phi_1^*\neq\theta_1^*$ and/or $\phi_2^*\neq\theta_2^*$ | R | $\lvert\phi_1^*-\theta_1^*\rvert+\lvert\phi_2^*-\theta_2^*\rvert$ | .000 |
| BB25 | none | $\phi_1=0$ | $\phi_1>0$ | S | t | .930 |
| BB26 | none | $\phi_1^*=0$ | $\phi_1^*>0$ | S | t | .950 |
| BB27 | none | $\theta_1=0$ | $\theta_1>0$ | S | t | .695 |
| BB28 | none | $\theta_1^*=0$ | $\theta_1^*>0$ | S | t | .816 |
| BB29 | none | $\phi_1=\theta_1=0$ | $\phi_1$ and/or $\theta_1\neq0$ | S | Wald | .262 |
| BB30 | none | $\phi_1^*=\theta_1^*=0$ | $\phi_1^*$ and/or $\theta_1^*\neq0$ | S | Wald | .136 |
| BB31 | none | $\phi_2=0$ | $\phi_2<0$ | S | t | .941 |
| BB32 | none | $\phi_2^*=0$ | $\phi_2^*<0$ | S | t | .998 |

| | | | | | | |
|---|---|---|---|---|---|---|
| BB33 | none | $\theta_2=0$ | $\theta_2<0$ | S | t | .004 |
| BB34 | none | $\theta_2^*=0$ | $\theta_2^*<0$ | S | t | .066 |
| BB35 | none | $\phi_2=\theta_2=0$ | $\phi_2$ and/or $\theta_2\neq0$ | S | Wald | .001 |
| BB36 | none | $\phi_2^*=\theta_2^*=0$ | $\phi_2^*$ and/or $\theta_2^*\neq0$ | S | Wald | .000 |
| BB37 | none | $\phi_1=\theta_1=\phi_2=\theta_2=0$ | any violation of null | S | Wald | .003 |
| BB38 | none | $\phi_1^*=\theta_1^*=\phi_2^*=\theta_2^*=0$ | any violation of null | S | Wald | .004 |

## NOTES FOR TABLE 16

HYPOTHESIS CATEGORY C: Vacancy rates

FORM OF EQUATION USED FOR TESTING:

$$(12)\quad v^*_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2} + \beta_{20}y_{it} + \beta_{21}y_{i,t-1}$$

$$+ \beta_{22}y_{i,t-2} + \beta_{30}p_{it} + \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2}$$

$$+ \beta_{40}\pi_t + \beta_{41}\pi_{t-1} + \beta_{42}\pi_{t-2} + \gamma_1 v^*_{i,t-1} + \gamma_2 v^*_{i,t-2}$$

$$+ \phi B_{it} + \theta C_{it} + \varepsilon_{it}$$

METHOD OF ESTIMATION: Ordinary least squares

TYPE OF DATA: Pooled annual time series for 15 metropolitan areas

NUMBER OF OBSERVATIONS: 182

SUMMARY DEFINITIONS OF VARIABLES:

| | |
|---|---|
| v | -- vacancy rate |
| n | -- annual population growth (%) |
| y | -- annual increase in real personal disposable income per capita (%) |
| p | -- annual inflation rate (%) |
| $\pi$ | -- annual rate of interest |
| B | -- control regime type B (zero-one variable) |
| C | -- control regime type C (zero-one variable) |

SUBSCRIPTS: i for metropolitan area, t for year

TABLE 16: TESTS OF HYPOTHESES RELATING TO CATEGORY C: VACANCY RATES

| Test | Restrictions Imposed | Restrictions Tested | | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| | | Null | Alternative | | | |
| C1 | $\phi=\theta$ | $\phi=0$ | $\phi<0$ | S | t | .575 |
| C2 | $\phi=\theta$ | $\phi=0$ | $\phi<0$ | R | $\phi$ | .562 |
| C3 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*<0$ | S | t | .574 |
| C4 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*<0$ | R | $\phi^*$ | .549 |
| C5 | none | $\phi=\theta$ | $\phi\neq\theta$ | S | t | .001 |
| C6 | none | $\phi^*=\theta^*$ | $\phi^*\neq\theta^*$ | S | t | .000 |
| C7 | none | $\phi=0$ | $\phi<0$ | S | t | .077 |
| C8 | none | $\phi^*=0$ | $\phi^*<0$ | S | t | .067 |
| C9 | none | $\theta=0$ | $\theta<0$ | S | t | .947 |
| C10 | none | $\theta^*=0$ | $\theta^*<0$ | S | t | .942 |
| C11 | none | $\phi=\theta=0$ | $\phi$ and/or $\theta\neq0$ | S | F | .003 |
| C12 | none | $\phi^*=\theta^*=0$ | $\phi^*$ and/or $\theta^*\neq0$ | S | Wald | .001 |

## NOTES FOR TABLE 17

HYPOTHESIS CATEGORY D: Proportion of renter households

FORM OF EQUATION USED FOR TESTING:

$$(13)\quad (H_r/H)_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2}$$

$$+ \beta_{20}y_{it} + \beta_{21}y_{i,t-1} + \beta_{22}y_{i,t-2} + \beta_{30}p_{it}$$

$$+ \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2} + \beta_{40}\pi_t + \beta_{41}\pi_{t-1}$$

$$+ \beta_{42}\pi_{t-2} + \gamma(H_r/H)_{i,t-1} + \phi B_{it} + \theta C_{it}$$

$$+ (\text{PROV terms}) + \varepsilon_{it}$$

METHOD OF ESTIMATION: Ordinary least squares

TYPE OF DATA: Pooled annual time series for 10 provinces

NUMBER OF OBSERVATIONS: 135

SUMMARY DEFINITIONS OF VARIABLES:

- $H_r$ -- number of renter households
- H -- total number of households
- n -- annual population growth (%)
- y -- annual increase in real personal disposable income per capita (%)
- p -- annual inflation rate (%)
- $\pi$ -- annual rate of interest
- B -- control regime type B (zero-one variable)
- C -- control regime type C (zero-one variable)
- PROV -- province (zero-one variable; one for each province)
- $\varepsilon$ -- random error

SUBSCRIPTS: i for province, t for year

TABLE 17: TESTS OF HYPOTHESES RELATING TO CATEGORY D: PROPORTION OF RENTER HOUSEHOLDS

| Test | Restrictions Imposed | Restrictions Tested | | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| | | Null | Alternative | | | |
| D1 | $\phi=\theta$ | $\phi=0$ | $\phi\neq0$ | S | t | .058 |
| D2 | $\phi=\theta$ | $\phi=0$ | $\phi\neq0$ | R | $\lvert\phi\rvert$ | .043 |
| D3 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*\neq0$ | S | t | .074 |
| D4 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*\neq0$ | R | $\lvert\phi^*\rvert$ | .033 |
| D5 | none | $\phi=\theta$ | $\phi\neq\theta$ | S | t | .327 |
| D6 | none | $\phi^*=\theta^*$ | $\phi^*\neq\theta^*$ | S | t | .325 |
| D7 | none | $\phi=0$ | $\phi\neq0$ | S | t | .056 |
| D8 | none | $\phi^*=0$ | $\phi^*\neq0$ | S | t | .072 |
| D9 | none | $\theta=0$ | $\theta\neq0$ | S | t | .751 |
| D10 | none | $\theta^*=0$ | $\theta^*\neq0$ | S | t | .750 |
| D11 | none | $\phi=\theta=0$ | $\phi$ and/or $\theta\neq0$ | S | F | .104 |
| D12 | none | $\phi^*=\theta^*=0$ | $\phi^*$ and/or $\theta^*\neq0$ | S | Wald | .126 |

## NOTES FOR TABLE 18

HYPOTHESIS CATEGORY E: Conversions

FORM OF EQUATION USED FOR TESTING:

$$(14)\quad (C/K_s)_{it} = \alpha + \beta_{10}n_{it} + \beta_{11}n_{i,t-1} + \beta_{12}n_{i,t-2}$$

$$+ \beta_{20}y_{it} + \beta_{21}y_{i,t-1} + \beta_{22}y_{i,t-2} + \beta_{30}p_{it}$$

$$+ \beta_{31}p_{i,t-1} + \beta_{32}p_{i,t-2} + \beta_{40}\pi_t + \beta_{41}\pi_{t-1}$$

$$+ \beta_{42}\pi_{t-2} + \gamma_1(C/K_s)_{i,t-1} + \gamma_2(C/K_s)_{i,t-2} + \phi B_{it}$$

$$+ \theta C_{it} + \varepsilon_{it}$$

METHOD OF ESTIMATION: Ordinary least squares

TYPE OF DATA: Pooled annual time series for 10 provinces

NUMBER OF OBSERVATIONS: 135

SUMMARY DEFINITIONS OF VARIABLES:

- $C$ -- conversions (single units to multiples)
- $K_s$ -- stock of single units (beginning of year)
- $n$ -- annual population growth (%)
- $y$ -- annual increase in real personal disposable income per capita (%)
- $p$ -- annual inflation rate (%)
- $\pi$ -- annual rate of interest
- $B$ -- control regime type B (zero-one variable)
- $C$ -- control regime type C (zero-one variable)
- $\varepsilon$ -- random error

SUBSCRIPTS: i for province, t for year

TABLE 18: TESTS OF HYPOTHESES RELATING TO CATEGORY E: CONVERSIONS

| Test | Restrictions Imposed | Restrictions Tested: Null | Restrictions Tested: Alternative | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| E1 | $\phi=\theta$ | $\phi=0$ | $\phi<0$ | S | t | .357 |
| E2 | $\phi=\theta$ | $\phi=0$ | $\phi<0$ | R | $\phi$ | .339 |
| E3 | $\phi=\theta$ | $\phi=0$ | $\phi\neq0$ | S | t | .713 |
| E4 | $\phi=\theta$ | $\phi=0$ | $\phi\neq0$ | R | $\lvert\phi^*\rvert$ | .701 |
| E5 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*<0$ | S | t | .761 |
| E6 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*<0$ | R | $\phi^*$ | .936 |
| E7 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*\neq0$ | S | t | .479 |
| E8 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*\neq0$ | R | $\lvert\phi^*\rvert$ | .123 |
| E9 | none | $\phi=\theta$ | $\phi\neq\theta$ | S | t | .740 |
| E10 | none | $\theta^*=\theta^*$ | $\phi^*\neq\theta^*$ | S | t | .840 |
| E11 | none | $\phi=0$ | $\phi<0$ | S | t | .339 |
| E12 | none | $\phi=0$ | $\phi\neq0$ | S | t | .677 |
| E13 | none | $\phi^*=0$ | $\phi^*<0$ | S | t | .710 |
| E14 | none | $\phi^*=0$ | $\phi^*\neq0$ | S | t | .581 |

| | | | | | | |
|---|---|---|---|---|---|---|
| E15 | none | $\theta=0$ | $\theta<0$ | S | t | .476 |
| E16 | none | $\theta=0$ | $\theta\neq0$ | S | t | .952 |
| E17 | none | $\theta^{*}=0$ | $\theta^{*}<0$ | S | t | .808 |
| E18 | none | $\theta^{*}=0$ | $\theta^{*}=0$ | S | t | .384 |
| E19 | none | $\phi=\theta=0$ | $\phi$ and/or $\theta\neq0$ | S | F | .885 |
| E20 | none | $\phi^{*}=\theta^{*}=0$ | $\phi^{*}$ and/or $\theta^{*}\neq0$ | S | Wald | .682 |

## NOTES FOR TABLE 19

HYPOTHESIS CATEGORY F: Rental units in need of major repairs

FORM OF EQUATION USED FOR TESTING:

$$(15)\quad (H_r^*/H_r)_{it} = \alpha + \gamma(H_r^*/H_r)_{i,t-1} + \phi B_{it} + \theta C_{it} + \varepsilon_{it}$$

METHOD OF ESTIMATION: Ordinary least squares

TYPE OF DATA: Pooled annual time series for 10 provinces

NUMBER OF OBSERVATIONS: 50

SUMMARY DEFINITIONS OF VARIABLES:

- $H_r^*$ -- number of rented dwelling units in need of major repairs
- $H_r$ -- total number of rented dwelling units (equal to number of renter households)
- B -- control regime type B (zero-one variable)
- C -- control regime type C (zero-one variable)
- $\varepsilon$ -- random error

TABLE 19: TESTS OF HYPOTHESES RELATING TO CATEGORY F: RENTAL UNITS IN NEED OF MAJOR REPAIRS

| Test | Restrictions Imposed | Restrictions Tested | | Type of Test | Test Statistic | P-value |
|---|---|---|---|---|---|---|
| | | Null | Alternative | | | |
| F1 | $\phi=\theta$ | $\phi=0$ | $\phi>0$ | S | t | .201 |
| F2 | $\phi=\theta$ | $\phi=0$ | $\phi>0$ | R | $\phi$ | .195 |
| F3 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*>0$ | S | t | .635 |
| F4 | $\phi^*=\theta^*$ | $\phi^*=0$ | $\phi^*>0$ | R | $\phi^*$ | .659 |
| F5 | none | $\phi=\theta$ | $\phi\neq\theta$ | S | t | .449 |
| F6 | none | $\phi^*=\theta^*$ | $\phi^*\neq\theta^*$ | S | t | .980 |
| F7 | none | $\phi=0$ | $\phi>0$ | S | t | .319 |
| F8 | none | $\phi^*=0$ | $\phi^*>0$ | S | t | .633 |
| F9 | none | $\theta=0$ | $\theta>0$ | S | t | .131 |
| F10 | none | $\theta^*=0$ | $\theta^*>0$ | S | t | .594 |
| F11 | none | $\phi=\theta=0$ | $\phi$ and/or $\theta\neq0$ | S | F | .528 |
| F12 | none | $\phi^*=\theta^*=0$ | $\phi^*$ and/or $\theta^*\neq0$ | S | Wald | .940 |

## APPENDIX I

## CONTACTS FOR RENT REGULATION UPDATE, 1993

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## APPENDIX II

## ESTIMATED EQUATIONS (UNRESTRICTED) CORRESPONDING TO HYPOTHESIS CATEGORIES

(Note: t-ratios in brackets; $\bar{S}$ is standard error of estimate; $\bar{R}^2$ is coefficient of determination, corrected for degrees of freedom.)

### A. RENTS

$$r_{it} = \underset{(-1.94)}{-1.3743} + \underset{(6.18)}{1.4591}n_{it} - \underset{(-2.19)}{0.7285}n_{i,t-1} - \underset{(-1.77)}{0.4212}n_{i,t-2}$$

$$+ \underset{(1.46)}{0.0741}y_{it} + \underset{(1.67)}{0.0867}y_{i,t-1} - \underset{(-2.06)}{0.1012}y_{i,t-2} + \underset{(1.24)}{0.1141}p_{it}$$

$$+ \underset{(3.01)}{0.3391}p_{i,t-1} - \underset{(-0.86)}{0.0718}p_{i,t-2} - \underset{(-0.43)}{0.0270}\pi_{t} + \underset{(0.89)}{0.0691}\pi_{t-1}$$

$$+ \underset{(0.15)}{0.0129}\pi_{t-2} + \underset{(9.88)}{0.7389}r_{i,t-1} - \underset{(-2.97)}{0.2116}r_{i,t-2} - \underset{(-0.53)}{0.2098}B^{+}_{it}$$

$$+ \underset{(1.19)}{0.4114}B^{-}_{it} - \underset{(-0.97)}{0.4359}C^{+}_{it} + \underset{(1.03)}{0.3917}C^{-}_{it}$$

$\bar{S} = 1.437$ $\qquad \bar{R}^2 = 0.736$

### B. HOUSING STARTS

$$(S_a/N)_{it} = \underset{(-2.77)}{-4.6695} + (\underset{(-2.40)}{-1.2764} + \underset{(0.28)}{0.2280}B_{it} + \underset{(0.12)}{0.1191}C_{it})v^{*}_{it}$$

$$+ (\underset{(0.01)}{0.0176} + \underset{(0.27)}{0.8650}B_{it} - \underset{(-0.04)}{0.1390}C_{it})(R/P)_{it}$$

$$+ \underset{(7.35)}{0.4930}(S_a/N)_{i,t-1} + \underset{(1.12)}{1.8386}MA1 + \underset{(2.29)}{1.6280}MA3$$

$$+ \underset{(1.10)}{0.8982}MA4 + \underset{(1.50)}{2.6166}MA5 + \underset{(1.41)}{2.4402}MA6 + \underset{(0.85)}{0.5729}MA7$$

$$- \underset{(-0.84)}{0.5787}MA9 + \underset{(1.16)}{0.8490}MA10 + \underset{(0.90)}{0.7832}MA11 + \underset{(2.32)}{2.2046}MA12$$

$$+ \underset{(1.47)}{1.1278}MA13 + \underset{(1.39)}{1.0665}MA14 + \underset{(0.68)}{0.5729}MA15 - \underset{(-0.17)}{0.2248}MA16$$

$\bar{S} = 1.562$ $\qquad \bar{R}^2 = 0.529$

$$(S_a/N)_{it} = \underset{(3.47)}{2.6237} + (\underset{(0.92)}{0.6985} - \underset{(-1.48)}{2.2166}B_{it} - \underset{(-0.51)}{0.8177}C_{it})\Delta v^*_{it}$$

$$+ (\underset{(1.45)}{18.700} + \underset{(1.57)}{30.948}B_{it} - \underset{(-2.67)}{48.904}C_{it})\Delta(R/P)_{it}$$

$$+ \underset{(5.22)}{0.4728}(S_a/N)_{i,t-1} - \underset{(-2.71)}{2.8068}MA1 - \underset{(-0.15)}{0.1252}MA3 - \underset{(-1.32)}{1.3371}MA4$$

$$- \underset{(-1.14)}{1.0230}MA5 - \underset{(-1.47)}{1.3528}MA6 - \underset{(-0.05)}{0.0483}MA7 - \underset{(-0.12)}{0.1063}MA9$$

$$- \underset{(-1.37)}{1.2311}MA10 - \underset{(-0.03)}{0.0388}MA11 + \underset{(1.12)}{1.4090}MA12 - \underset{(-1.52)}{1.4332}MA13$$

$$- \underset{(-1.50)}{1.4511}MA14 + \underset{(0.36)}{0.3328}MA15 - \underset{(-0.90)}{1.0415}MA16$$

$\bar{S} = 2.066$ $\qquad \bar{R}^2 = 0.177$

## C. VACANCY RATES

$$v^*_{it} = \underset{(-4.94)}{-1.6593} - \underset{(-4.52)}{0.3952}n_{it} + \underset{(2.14)}{0.2445}n_{i,t-1} + \underset{(0.99)}{0.0810}n_{i,t-2}$$

$$- \underset{(-1.54)}{0.0293}y_{it} + \underset{(1.52)}{0.0297}y_{i,t-1} + \underset{(0.57)}{0.0107}y_{i,t-2} - \underset{(-0.03)}{0.0011}p_{it}$$

$$- \underset{(-0.50)}{0.0189}p_{i,t-1} - \underset{(-1.27)}{0.0383}p_{i,t-2} - \underset{(-0.49)}{0.0115}\pi_t + \underset{(0.55)}{0.0158}\pi_{t-1}$$

$$+ \underset{(1.40)}{0.0425}\pi_{t-2} + \underset{(10.76)}{0.8945}v^*_{i,t-1} - \underset{(-3.88)}{0.3265}v^*_{i,t-2} - \underset{(-1.44)}{0.1686}B_{it}$$

$$+ \underset{(1.63)}{0.1866}C_{it}$$

$\bar{S} = 0.523$ $\qquad \bar{R}^2 = 0.711$

## D. PROPORTION OF RENTER HOUSEHOLDS

$$(H_r/H)_{it} = \underset{(5.42)}{0.1859} - \underset{(-1.28)}{0.0052}n_{it} - \underset{(-0.78)}{0.0038}n_{i,t-1} + \underset{(0.09)}{0.0003}n_{i,t-2}$$

$$+ \underset{(1.62)}{0.0010}y_{it} + \underset{(2.29)}{0.0015}y_{i,t-1} + \underset{(1.34)}{0.0009}y_{i,t-2} - \underset{(-0.72)}{0.0008}p_{it}$$

## APPENDIX III

## SUMMARY STATEMENT OF DATA AVAILABILITY

The estimated equations reported in Tables 13 through 16 are based, in part, on annual data for sixteen metropolitan areas. Insofar as possible, data are drawn from the period 1971-1991. Hence there are twenty-one annual observations for each of the metropolitan areas. However, one year is lost because the estimated equations involve first difference terms and a further two years are lost to allow for two years of lagged variables, leaving eighteen observations for each area, or 288 observations (18 years times 16 metropolitan areas) for all areas combined. Further observations are lost because some series are unavailable. In the case of Table 13, the rent index is missing for the Charlottetown metropolitan area from 1974 through 1981 inclusive, and the price index is missing for the Victoria metropolitan area for the period 1976 through 1986 inclusive, thereby accounting for a further reduction of twenty-one observations. Finally, the use of data set (4), which excludes observations for years in which a change in rent control regime had occurred as well as the year following, accounts for the loss of a further seventy-five observations. (For further description of data set (4), see section 8.) In total, then, the equations reported in Table 13 are based on 192 observations. A further ten observations are lost in the estimation of the equations in Tables 14 through 16 because starts data are not available for the Charlottetown metropolitan area. (Eight observations were already lost in the

estimation of the equations in Table 13, and a further ten here.)

The estimation of the equations associated with Tables 17 and 18 is based on provincial data. Again, an eighteen-year period (1974-1991) is used, yielding 180 observations (eighteen years, ten provinces). Forty-five observations are lost because of the restrictions on data set (4).

Finally, the estimation of the equations in Table 19 is based on six years of observations (the period 1987-1992). One year is lost in order to allow for the inclusion of lagged variables. Hence there are fifty observations, in total (five years for each of ten provinces).

# TERMS OF REFERENCE

## Follow-up to Study on "Testing Hypotheses About Rent Controls" by Quantec Research Ltd.

**OBJECTIVES**

1. The objectives of this project is to provide CMHC with an assessment of **HOW** the research carried out by Quantec can be improved or built upon. This assessment should focus on i) better methodologies and/or techniques that can be used to test hypotheses about rent controls; ii) how additional evidence regarding the impact of rent controls on the rental market can be generated.

   Rent regulation is a subject of interest in many quarters and it has been widely studied. The research carried out by Quantec Research Ltd. is unique in that it is the first to apply statistical methods (a para-metric and non-parametric approach) in the testing of a range of hypotheses about the effects of rent regulations on the rental market.

   The terms of the CMHC contract with Quantec called for the development of formal statistical testing methods and the application of those methods to such data as were available and relevant. It is important to note that in carrying out the study, the consultant considered and documented the implications of data availability as well as a range of issues related to statistical or econometric theory and procedure.

**WORK**

2. The Contractor is required to carry out the following tasks:

   i) The Contractor will review the methodologies and techniques employed in the study "Testing Hypotheses About Rent Controls", with a view to making substantive and practical suggestions specifically related to **HOW** the study can be improved or built upon. It is underscored, that in addition to providing your rationale, a key aspect will be the Contractor's indication how the alternative methodologies and techniques that can be employed in a practical way.

   ii) The Contractor will also present practical ideas for further work that could be carried out in order to

generate additional evidence regarding the impact of rent controls on the rental market.

iii) The Contractor shall submit the written comments to the Corporation no later than Monday, 28 February, 1994.

iv) The Contractor shall include in his submission a resumé which clearly outlines his qualifications and experience in carrying out statistical analysis and research related to the rental market.

TIME FRAME

3. The work shall be performed between 31 January, 1994 and 28 February 1994.

# Follow-Up Study On:

## *"Testing Hypotheses About Rent Controls"*

**Andersen Economic Research Ltd.**
**February 1994**

### 1.0 Terms of Reference

Andersen Economic Research Ltd. has responded to a proposal call by C.M.H.C. to review research undertaken by Quantec Research Ltd. on rent controls in Canada and to provide written commentary as to how the research could be improved or built upon. Respondents have also been asked to comment as to how additional evidence can be generated about the impact of rent controls on the rental market.

### 1.1 Organization of Paper

Section 2 of the paper will provide a critique of the methodologies and techniques employed by Quantec with the view that further statistical and econometric techniques can be employed in addressing the issue of the impact of rental control on rental markets in Canada. Section 3 will provide an outline of how further additional evidence of how the impact of rent control can be generated. Key recommendations found in Section 2 are closely linked with the suggestions for additional work in Section 3.

### 1.2 Summary of Conclusions

With regard to the first objective, Andersen Economic Research Ltd. finds and recommends the testing of a set of hypotheses derived from the specification of a structural model of the impact of rent control in Canada. Further, the structural model would be applied to only three large urban markets in Canada - Toronto, Montreal and Vancouver. The structural model would place more emphasis on estimating the supply effects in markets with and without rent control regimes. Greater attention would be directed towards estimating the impact of investment in for-profit rental units in markets with and without rent controls. A structural model would more accurately account for unique differences in the rental markets in each of the three large urban areas.

In addressing the second objective, which is how additional evidence of the impact of rent controls may be gathered, it is recognized that there currently exists a paucity of data in regards to this issue. Andersen Economic Research Ltd. suggests that there is a great need for the development of data on the investment component of rental housing. The fundamental issue on subsequent supply effects is seen as whether or not capital employed in for-profit rental housing is more or less profitable in a market where some form of rent control exists. If in fact, there was variance in the rate of return on capital across markets with and without controls, it can reasonably be assumed that this would be evident in variations in the investor profiles in these

markets. Markets with controls may be more concentrated in terms of investor activity as a smaller number of investors would own a greater proportion of the rental stock.

It is recommended that further work in this area be conducted by carrying out a number of surveys which would focus on the investment aspect of rental housing. More evidence also needs to be gathered on issues affecting the rental market such as differences in turnover between buildings under rent control and those that are not.

## 1.3 Summary of Quantec Research Ltd. Study

It is our understanding that Canada Mortgage and Housing Corporation contracted Quantec Research Ltd. to undertake formal statistical testing on a range of hypotheses pertaining to the effects of rent controls on the rental housing market in Canada.

The terms of the contract required Quantec to test seven null hypotheses that suggested that rent regulations:

1) over the long run have no impact on rents;
2) cause no differences in the responsiveness of supply with respect to vacancy rates and rents;
3) are associated with lower vacancy rates;
4) decrease the relative attractiveness of investment in rental housing;
5) decrease the relative financial advantages of home ownership;
6) encourage conversions from rental to owner-occupied housing; and,
7) discourage maintenance and repairs.

Quantec's approach to the study was to first state the above seven hypotheses into a set of testable null and alternative hypotheses which could be more effectively used in their statistical approach. The key components of their statistical approach included a detailed analysis of the various provincial rent control regimes for the period 1971-1991 and the selection of data related to the housing market and other pre-defined exogenous variables at a provincial and metropolitan level. Statistical methods employed included ordinary least squares regression analysis as well as instrumental variables estimation combined with non-parametric testing which was used to confirm the results of the original estimation procedures.

Data limitations resulted in testing a reduced number of hypotheses than were originally stated. Quantec tested seven equations relating to rents, housing starts (level and change effects), vacancy rates, proportion of renter households, conversions and rental units in need of major repairs.

Quantec concluded that:

1) there is no evidence that controls influence the long-run rate of increase of rents;
2) there is no evidence that controls affect the responsiveness of apartment unit starts to either vacancy rates or rents;

3) in general, controls do not tend to lower vacancy rates;
4) there is some, albeit limited, evidence that controls tend to be associated with higher proportions of renter households;
5) there is no evidence that controls affect the rate of conversions; and,
6) there is no evidence that controls increase the proportion of occupied rental dwellings in need of major repairs.

In effect, the results supported hypotheses one, two and five as stated by C.M.H.C. and did not support the remainder of the hypotheses.

In summary, the results of the statistical study under investigation would suggest that the various rent regulations in existence over a period of time in various provinces in Canada have had virtually no impact on the normal functioning of the rental markets observed in major cities in the time period under investigation.

## Section 2.0 Critique of Methodology

While the authors are to be given credit for the elaborate statistical methods used in applying non-parametric estimation techniques to the set of hypotheses, our critique must begin with an overall assessment of the results of the study. The authors are suggesting that the imposition of rent controls at various periods of time in markets in Canada have had virtually no impact on key elements including the demand, the price and the subsequent supply of rental market housing. In other words, in spite of the imposition of an elaborate series of controls similar, but not identical to, price controls, applied to the price of rental housing, the rental market in Canada has operated as if these controls were not in place.

Although the body of econometric literature on the rental housing market in either Canada, or other countries where the market is similar to that of Canada's, is not as broad as other areas of study on the housing market, the results reached in this study under investigation appear in contrast to the existing body of literature where similar hypotheses have been tested. Further, the conclusions are also in contrast to an intuitive pre-judgment about the impact of rent control in Canada. The key conclusions reached in the study would suggest that the price elasticity of rental housing is the same in the absence of rent regulations as when regulation exists. If this is indeed the case, the dynamics of the rental market in Canada would have to be determined by something other than basic factors of demand and supply.

As our critique first begins with the conclusions reached by the study, we must look at how the hypotheses were first stated. The area where we would suggest further econometric work be undertaken is the development of a structural framework, beginning with the original specification of the hypotheses. In establishing seven different categories for examination including rents, housing starts, vacancy rates, property values, tenure preferences, conversions and maintenance and repairs, the various null hypothesis associated with each category assume mutual exclusivity of categories. Thus, for example, the null hypothesis associated with vacancy rates, (that rental vacancy rates are unaffected by regulation), is assumed to be separable from

supply (housing starts), where for example, a null hypothesis is that the responsive of rental-unit housing starts to changes in vacancy rates and rents is unaffected by regulations.

A structural model would recognize the dependency of the price of rental housing (the rental rate) on the integration of factors affecting the demand for rental housing (income, tenure choice, household characteristics as well as underlying demographic and economic influences) and the supply of rental housing (opportunity cost for investors, cost of financing, land availability, other legislation regarding the investment opportunities in this sub-sector, degree of demand as exhibited by the vacancy rate). A set of hypotheses would then be generated which would logically connect sub-components for the market and result in the test of relationships between, for example, vacancy rates and rents, and in turn housing starts, as a series of dependencies rather than separable factors.

Other than the binary variables and lagged dependent variables used in the equations, the explanatory exogenous variables are assumed to be variables which would have been selected, if a structural model had been identified. The equations are all assumed to be reduced-form equations which are the result of an implicit structural model. In Section 6, entitled Testing the Hypotheses: A Parametric Approach (p. 20), the authors rationalize the absence of a structural model by stating that "we are not interested in the effects of the x-variables, as such, but merely with controlling for those effects so that we can identify the effects, (if any) of the rent control variables." As a result, similar exogenous variables are used in each of the equations, including the rate of population growth, the annual per cent increase in real disposable income per capita, a three-month Tbill rate and the rate of inflation.

However, misspecification of the exogenous variables combined with the reliance upon lagged dependent variables and binary rent regulation variables increases the potential for bias in the results in favor of supporting each of the null hypothesis. In by-passing a properly specified structural model, it is a necessary condition that the explanatory variables which are used are properly specified. Misspecification in a reduced-form equation will introduce bias in the statistical results arising from the problem of omitted variables. By definition, the statistics on the lagged dependent variables and rent regulation binary variables will be weighted as these variables pick up the systemic error introduced from the condition of omitted variables.

The estimation results would suggest that some misspecification has occurred. The R-squared statistics on only three of the seven equations are within bounds of reasonability. The t-statistics on many of the coefficients in most of the equations are also low although if misspecification has occurred there would be bias in these results. While it appears that serial correlation has been scanned visually, appropriate test results appear to be lacking.

In this context, the application and use of non-parametric estimation techniques as a method of verifying the original estimation results loses its significance as the procedure is simply confirming the results of what may have been originally misspecified variables.

In addition to the introduction of bias into the results due to the specification of what are assumed to be reduced-form equations, the second major concern relates to the selection of the

exogenous variables to begin with. Population growth, the generalized rate of inflation, a representative rate of interest and a proxy for change in household income serve for the most part as scale variables which provide an explanation for underlying long-term trends, but do not provide much explanatory power in terms of variances in the dependent variables. Tenure preference on one hand and rental supply response on the other, is a more complicated process than would be suggested by simply looking at the trend over time in the above exogenous variables.

In suggesting an alternative methodology which would address some of the concerns herein identified, we would not undertake a further refinement of the statistical methodology employed in the study under investigation. Rather, we would recommend econometric work focus first on the development of an adequately specified structural model. While a series of reduced-form equations would be tested, greater weight would be given to the explanatory power of the exogenous variables.

The development of a structural model would necessarily result in a different set of hypotheses to be tested owing to the simultaneity of the variables. In the original set of hypotheses identified by C.M.H.C., with the exception of the issue of tenure preference, the remaining six hypotheses relate to the effects of rent regulations on supply of rental housing. Regarding the effects on the supply of rental housing to the market, the key question becomes whether or not the rate of return on capital invested in for-profit rental housing is greater or less in the presence of rent controls.

A lower rate of return on investment in rental housing stock in markets with rent controls would over time, result in a reduction in the number of units to the market, other things being equal. Measures of disequilibrium in the market, such as vacancy rates, would be assumed to be greater in these conditions. In turn, conversions as well as quality of existing stock would also be adversely affected.

In accounting for tenure preference, factors other than the proportion of renter households to total households would be measured. Factors affecting tenure choice, including specific household characteristics such as age and income levels, as well as general economic conditions, would be tested in the presence and absence of rent controls. As well, factors specific to rental housing, such as the probability of vacancy as well as the rate of turnover in buildings under rent control and those not under control, could be examined in the model.

The application of the structural model would be limited to three large urban markets - Toronto, Montreal and Vancouver. Together, these market account for the majority of the population in Canada. Rent control regimes have also varied in each of these markets. In limiting the analysis to these markets, greater attention would be paid to adequately specifying a structural model which would take into account unique features in these markets. While Quantec did attempt to account for differences in markets by estimating the ratio of starts to population, it was not broad enough to account for all major structural differences in these markets.

The structural model would be estimated using standard econometric techniques including the two-stage least squares, a method used to estimate simultaneous equations.

## 3.0 Areas for Further Investigation of Additional Evidence

A well-specified structural model is heavily dependent upon the development and broadening of existing data sources. In particular, the area of focus would be on investigating alternative sources of data which would provide some indication of the rate of return accruing to investors in the rental market and whether or not it varies according to control regimes. As noted in Section 2.0, the Quantec study fails to adequately account for the impact of rent controls on the rate of return on investment in for profit rental housing. While some attempt was made by Quantec to investigate data sources on rental property values, in our view this approach was not sufficient.

Theoretically, the supply of rental housing is linked to the willingness of private-sector investors to invest in this component of the market. The imposition of rent controls is assumed to affect the rate of return negatively on that investment, not only through the reduction in rents, other things being equal, but also through the imposition of other types of controls.

Further research would entail 1) measuring the rate of return accruing to investors over time and looking at 2) variations in rates of return in markets with and without controls 3) whether or not this has resulted in attitudinal shifts among investors 4) whether the class of investors has changed 5) whether or not there has been a structural change in the concentration of investment in the industry i.e., have rent controls resulted in an exodus by investors from the market, resulting in fewer investors owning more of the rental stock and 6) does investor behavior change with the housing market cycle, independently of the rate of return on investment. While these statements appear unconnected from the original hypotheses, they are in fact, intrinsically related.

If indeed, rent controls have had an impact on the rate of return on rental stock, one would assume a substitution away from private sector to public sector involvement in the rental market. This, in turn, would over time, have an impact on the general level of rents in a market where rent controls are present.

The recommended approach in addressing the questions would be twofold. First, some elements of these questions would have to be incorporated within an econometric framework into the structural model. This in turn, would be dependent upon the existing data series available to test some of these statements. As noted by Quantec, information on the value of rental properties does not readily exist. However, what can be developed are proxies representative of value. Series on the yield associated with various types of real estate do exist. Although these generally tend to measure rate of return on non-residential real estate, it may be possible to use a similar methodology to generate a yield series on rental housing for each of the three major markets in Canada.

In addition to investigating and perhaps developing data series which would serve as proxies of investor behavior, a survey of the three key markets can be undertaken which would provide information on the differences among Toronto, Montreal and Vancouver in the ownership of for-profit rental stock present. It is significant that all three markets have been subjected to different rent control regimes and would therefore quite naturally lend themselves to such analysis. Some of this analysis within the survey could be combined with C.M.H.C.'s semi-annual vacancy survey.

A survey of the characteristics of the investor participation in the rental markets in the three cities could be supplemented by an analysis of whether or not controls cause substitution towards other non-recognized types of rental accommodation. For example, if controls reduce the return on rental stock, which in turn, reduces the supply of private sector stock, does this affect preferences of the renter population towards other forms of housing? Conversions of single-family dwellings to multiple-units would be the most obvious impact. While the Quantec study did examine the impact on conversions and concluded that there was no impact, it is suggested that further evidence be gathered in this area. A survey could be undertaken which would try to estimate the proportion of non-traditional types of rental accommodation available in Toronto, Montreal and Vancouver.

With regard to the demand side, further evidence is required in terms of tenure preference and the rate of turnover by tenants in controlled versus uncontrolled buildings. Household choice between renting and owning is to a large extent dependent upon specific household characteristics such as current and expected income level, number within the household, mobility status and employment status. The presence of rent controls is likely to generate changes in the distributional effects of tenure status. Further evidence should be gathered as to whether or not rent controls generate changes in tenure choice amongst the various quartiles of household income classes. Further evidence is also needed in terms of examining the rate of turnover in buildings with and without controls. A lower rate of turnover in controlled buildings would more likely cause market distortions over time. Much of this analysis could be conducted using the information available from the annual H.I.F.E. survey.

The focus of these recommendations, in terms of area of further study, is on broadening the collective understanding of some of the underlying characteristics of the rental market in Canada. If markets are intrinsically different due to rent controls, this will have long-term implications for housing policy. In particular, if there has indeed been a shift away from private to public rental stock in markets where rent controls are present, this could have significant long-term consequences. Related to this is the issue of whether investment in for-profit rental housing is a more or less profitable venture as a result of controls.

Toronto, Vancouver and Montreal are three key markets which lend themselves to both econometric and survey analysis, as rent control regimes have varied in each market. The fundamental questions with regard to rent controls are whether or not controls have made investment in rental housing more or less attractive over time and in turn, have created substitution and dislocation in the rental market. These issues still require further examination.

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# Clayton Research

February 28, 1994

Memorandum to: Sharon Olm, Senior Analyst, Market Housing Policy Group, Strategic Planning and Policy Development Division, CMHC

From: Frank A. Clayton, President

Re: Follow-up to Study on "Testing Hypotheses About Rent Controls" by Quantec Research Ltd.
Our File: P-1968

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Pursuant to the terms of our contract letter, this memorandum: (a) reviews the methodologies and techniques employed in the above study and makes suggestions as to how the study could be improved or built upon and (b) presents ideas for additional work that could be carried out to generate additional evidence regarding the impact of rent controls. A resume of our qualifications and experience relating to statistical analysis and research related to the rental market is enclosed, as requested.

## REVIEW OF QUANTEC STUDY

### General Observations

- Our first reaction to the study was astonishment with its conclusion that there appears to be no convincing evidence that rent controls (regulations) have had significant effects on rents, on the construction of rental units, or on vacancy rates - we hoped the report would be making a convincing case supporting its findings.

- However, after a thorough review, we have doubts about the validity of the study's findings since we have serious reservations about the study's approach, some of the statistical techniques used, and many of the data series used.

- Our recommendations for improving the study include: development of an analytical framework, formulate models that encompass both the demand and supply sides of the market, and devoting resources to the creation of a better statistical base.

### Comments on the Approach and Models

- Instead of applying models globally to all the metropolitan areas/provinces considered and reviewing these results, in our opinion there are convincing arguments to focus on specific market

areas, e.g., Toronto, over time. This approach was used in an econometric study of the rent control impacts in Ontario done in the early 80s.

- If there is one thing we have learned from 25 years of housing market analysis experience, it is that individual market areas have important differences. Thus, for instance, a given change in average rents could trigger different responses in rental construction activity depending upon the specific market area under consideration. These differences include affordability, land costs, role of the provincial government in providing or subsidizing rental production, and the structure of the local development industry.

- The separation of rent control regimes into three categories does not adequately reflect differences at the local (provincial) level. The rent control schemes in Manitoba and Ontario are categorized as staying the same over the past decade. However, the Ontario scheme is much more restrictive than Manitoba's and became more so over the period.

- There is often unique data at the local level. For example, the Toronto CMHC office has been tabulating rental starts back to the early 1970s. Calgary has data from its annual enumeration on the rental housing stock and vacancies by unit type.

• P.16. In theory, a full blown structural model of the rental market is not required if one has all the relevant explanatory variables from both the supply and demand sides of the market and the correct functional form of the relationships. But how does one know if you do? You don't.

We do not believe a detailed model is "unnecessary for our purposes". One should begin with models of both the demand and supply sides of the market to determine what the important variables are and how they interact (i.e., functional form of the model) to determine rents, vacancies and starts. This provides a justification for the choice of variables and warns us ahead of time about the potential limitations of the econometric work. At this stage one sees what information may be impossible to obtain, even though in theory it may be important.

Before testing for the effects of rent control one should have a reduced form model (equation) that explains changes in rents in locations that do not have rent control. Otherwise, by adding in dummy variables for rent control one does not know what one is picking up.

• P.16 bottom and top of P. 17

(i) we do not know why Quantec imposes a two year lag. This should come either from a model or can be determined by appropriate testing.
(ii) Quantec assumes a linear functional form. Why? This is very restrictive. Later on the text tells us that parametric methods are inferior if the residuals are non-normally distributed (see more later). One reason why it might appear that the residuals are non-normal is incorrect functional form. A natural logarithm transformation of the dependent variable often induces normality into the residuals and at the same time stabilizes the variance.

- P.19 Equation (7). We do not see why there is any reason to believe that the coefficients on the variables should have the same values for all regions in the country. This is related to the above discussion about the uniqueness of local markets.

  It is also related to recent work that shows that different urban markets have different natural vacancy rates. Thus a comparison of vacancy rates across urban areas is not necessarily useful in itself - it depends on rates of change of rents among other variables.

- P.20 Housing Starts Model (Model is given on p.30). Housing starts measure a rate of change in the stock of housing and thus are related to changes in the number of households. As such, the relevant explanatory variable should be the rate of change of rents not the level of rents as in the Quantec mode. Quantec does experiment with this later, but it should be the focus we believe. Also, should we not have construction costs in the model? Or maybe derive a measure of profitability? Land costs? Competition from the condo sector?

  Quantec uses current rents to explain current starts. Starts, however, will be driven by expected rents. They could estimate a model with different types of expectations mechanisms assumed: rational, adaptive, extrapolative.

  Finally, the expected profitability of new rental housing investment is related to risk. Maybe one could proxy risk somehow.

  Risk enters the analysis in another way as well. In the absence of rent control the risk that controls may be imposed in the future is a cost to builders/developers - this might go part way in explaining Quantec's results that controls do not affect new construction.

- Nonparametric vs parametric: Quantec correctly states that standard regression techniques only produce meaningful parameter estimates and tests of their significance if the residuals are normally distributed. The authors then jump to a nonparametric method that does not require this because it does not make any distributional assumptions about the error terms.

  From what we could see, Quantec never tests the normality of the residuals in the standard models - there are very simple tests available. Given the nature of the dependent variables used in the analysis we suspect non-normality is not a problem. Thus it seems to us that the use of a nonparametric method is not of any use.

**Comments on the Data Series**

- The report states that Quantec's mandate was to work with readily available data. This is unfortunate given the lack of time series information for several key series. Fortunately, it is possible to create reasonable estimates of some of the key data - we have done it for various projects over the years. However, such estimates can be time-consuming so this is another argument for concentrating on a small number of local market areas. Specific comments include:

- Rental Housing Starts: The use of all apartment starts is of course an imperfect proxy for private sector rental starts. A basic problem is that there is not a consistent overstatement factor - it will vary by time period and by market area. Questions arise about how to treat private sector units subsidized directly or indirectly by the federal or provincial governments (which would include much of the new rental housing built in the later 70s and early 80s for example). CMHC data for private sector starts are available through the 1980s. Rental starts for earlier years can be estimated by using a combination of Census of Canada period of construction and CMHC information.

- Rents: It is well known that the rent component of the CPI is not a very accurate reflection of changes in average market rents in a market area. It tends to be too smooth relative to true rent price fluctuations (i.e., understates the actual variance of rent changes) and there seems to be a consensus that it is downward biased as well. Thus, it has very little variation (i.e., there is not much to explain in terms of rent price changes) and this may partially explain why rent control indicators are not significant.

  The study employs average rent inflation. Quantec never tells us (at least we could not find a reference) if the rent variable uses average rents during the whole year from year-to-year of if they have chosen a specific month with which to measure year-to-year changes. If it is the former, then this further artificially smooths the rent changes.

  It would be nice if they could plot the rent data and indicate on the graphs when rent control programs were initiated, changed or removed.

  An approach we have used in the past is to use the Census of Canada average rents as benchmarks and to pro rate the changes between Census benchmarks according to relative changes in the rent component during the same period. For the 1980s in several market areas rents are available from the apartment vacancy surveys done by CMHC.

- Conversions: The conversion process as related to rent controls is more related to conversion of existing rental buildings to condos than to the conversion of single-detached houses. Again at the local level there are some data here. Several CMHC offices, I believe, keep track of rental conversions as part of the updating of the apartment vacancy survey universes.

- State of Repairs - We do not see how five years of data on rental dwellings in need of major repairs can be helpful here since the need for major repairs resulting from rent controls would occur over a lengthy time. Even if a longer time series was available it would appear more reasonable to focus on units in need of minor reports or minor plus major repairs, not just units needing major repairs.

- Population is an inaccurate measure of demand - one needs a measure of renter households.

**IDEAS FOR ADDITIONAL WORK TO IMPROVE UNDERSTANDING OF THE IMPACT OF RENT CONTROLS**

- We think that case studies have the potential to produce much more meaningful results of the impacts of rent control than the global approach utilized by Quantec. While data availability will be a decisive consideration in the case study urban centres chosen, the role of expectations should not be ignored. How long did it take prospective investors in Vancouver after rent controls were removed there to be convinced (if they were) that rent controls would not be reimposed. Perhaps consideration should be given to pairing Canadian urban areas with closeby U.S. urban areas, in addition to or instead of, pairing with other Canadian urban areas, to get case study comparables for urban marketing without rent controls.

- Ideally for the case studies one would like to obtain a time series of property level data (rents, age, units, expenses, etc.) on a sample of properties in a small number of urban areas with and without rent control so we could construct hedonic (quality-adjusted) rent indexes. With the resulting data we could analyze the independent effect of rent controls on changes in rents.

- Whatever models or geographic areas are eventually adopted, it seems critical that a lot of effort be made to generate data series which have sufficient reliability to be meaningful inputs into the models.

- If CMHC wishes to continue to utilize the global approach, then we believe a complete structural model of the rental housing market should be developed. This would provide a framework in which to establish approximate regression models. Location-specific parameters would allow for unique adjustments by area. As background, it would be useful to review both theoretical and empirical work on rental market adjustments, new construction determinants, etc. In this regard, a lot of the empirical work being done in the commercial sector (e.g., natural vacancy rates) could have relevance to the task at hand.

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# Comments on: Testing Hypotheses About Rent Controls, By Quantec Research

Hickling Corporation
March 1, 1994

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# Executive Summary

The Report, "Testing Hypotheses About Rent Controls," by Quantec Research makes the first real attempt to apply formal statistical techniques to Canadian data with a view to testing hypotheses relating to the effects of rent controls on the rental housing market.

The comments presented here conclude that while the Report provides the basis for further research it does not end the debate as to whether the effects of rent controls have been sufficiently disentangled from other housing policy and macro-economic factors that affect the operation of the housing market. The comments point specifically to problems of model specification and using time series analysis to estimate the effects of a short run "structural impact."

The comments come to the conclusion that in order to gather further evidence with respect to rent controls a multi-faceted analytical approach should be adopted in order to consider the full range of effects. This approach includes:

- More appropriate testing of the short run effects of rent controls on rents and other factors could provide further evidence regarding whether rent control polices have been successful in meeting there initial objectives.

- Comprehensive analysis of the costs and benefits of rent controls, for a major metropolitan centre, would provide further evidence regarding rent controls that would be useful for housing policy makers.

- Evidence currently exists relating to the disincentives for investment in the rental housing market. While most rate of return studies relating to rent controls focus on specific metropolitan centres, further research that considers the full range of tax incentives and other government policies which favour other investments over rental housing, may shed light on some of the persistent supply problems that exist in some rental housing markets in Canada.

## 1.0 Introduction

The comments that follow are divided into three sections. Section 2.0 focuses on the methodological approach of the Report and the theoretical foundations for the hypotheses that were tested. The authors tested a pre-defined set of hypotheses and, as a result, the basis for the hypotheses is not provided. By taking a step back from the technical details of the Report, the comments address whether an alternative approach could be taken to develop the hypotheses relating to the effectiveness of rent controls as an instrument of public policy. Alternative methodologies are suggested in order to apply the current statistical techniques to additional hypotheses and to use alternative analytical procedures to evaluate the effects of rent controls on the rental housing market.

Section 3 briefly addresses some of the statistical and econometric methods used in the Report to suggest possible limitations of the results. Finally, the comments suggest how the approach of the Report could be improved with a view to providing more evidence on the effects of rent controls on the rental housing market in Canada.

## 2.0 The Methodological Approach of the Report

In the Report "Testing Hypotheses About Rent Controls," Denton et. al., assess the effects of rent controls on a number of key factors that determine the demand and supply for rental accommodations in Canada. The Report makes the first attempt to apply econometric techniques to metropolitan area time series data in Canada, relating to the rental housing market, and test hypotheses dealing with the effects of rent controls.

The Report tests seven hypotheses relating to the housing market. It will be useful for later discussion to summarize those hypotheses here:

1) The long run rate of increase in rents is unaffected by regulations either in periods of strong or weak upward pressure on rents;

2) The responsiveness of rental-unit housing starts to levels of vacancy rates and rents is unaffected by regulations;

3) Rental vacancy rates are unaffected by regulations;

4) The long run rate of increase in real property value is unaffected by regulations;

5) Household preferences for renting are unaffected by regulations;

6) Rates of conversion of rental units are unaffected by regulations; and

7) Maintenance, repairs, and the provision of services in rental units are unaffected by regulations.

The paper contains an exhaustive discussion of the technical details relating to the statistical and econometric methods that were used to test the hypotheses. The comprehensive statistical and econometric treatment of the rent control hypotheses takes place in the absence of a discussion of the formulation of the hypotheses themselves. The Report would benefit from an initial statement of the arguments both in favour and against using rent controls as an instrument of public policy.

In order to do this it is necessary to consider two central questions; what were the objectives of rent controls when initially implemented? and, have rent controls had the desired effects? A related and useful extension of these questions is; were rent controls the most efficient means to meet the stated objectives of rent control policies? Hypotheses centred around these questions provide the correct basis for assessing the effects of rent controls on the Canadian housing market.

*Identifying Policy Objectives*

With respect to the first of the above questions regarding public policy objectives; rent controls were initially implemented in the mid 1970s as part of the general price and wage controls that were intended to reduce the distributional impacts of rising inflation. In the case of housing, the argument was, that for low income groups for which housing represents a much more significant proportion of household income, the effects would be particularly deleterious.

Rent controls were also viewed as part of a much broader trend of pervasive government (all levels) involvement in Canadian housing. This involvement included large subsidy programs for home buyers and intrusive tax incentive programs. Direct government involvement in the form of subsidies and social housing provision has since been drastically reduced. However, tax incentives favouring owner occupied housing remain an important part of housing policy. As noted later in the comments, considering rent controls in isolation from other pervasive forms of government involvement could lead to biased results.

Initially, all provinces imposed rent controls on a "temporary" basis to dampen the effects of inflation on rental housing prices for individuals in low income groups. However, the initial intentions of the policies have been extended in some provinces and rent controls persist in six provinces, with Ontario having the most stringent controls. The "extended" objective of rent controls is to provide an affordable and stable supply of rental accommodation (Fallis, 1985).

The basic policy question therefore is; have rent controls been successful in meeting the initial and extended objectives of rent controls? And, if they have been successful, then at what cost have they been successful?

The Report considers only part of the question in the estimates of the effects of rent controls on rents, housing starts and vacancy rates (hypotheses 1-3). The Report addresses the long run question of price and supply stability in rental housing but does not address the question of whether rent controls were successful in meeting there initial objectives.

In order to do this it is necessary to consider rent control policies as a shock to the housing market when they were implemented; initially placing the rental housing market in a state of disequilibrium. However, if landlords considered that controls would be temporary, behaviour would be unlikely to change in the short run. Landlords would forgo current increases in the interest of perceived future increases.

The Report does not adequately consider the short terms effects in its analysis of rent controls to determine whether this in fact was the case. By using time series analysis

(rather than a cross section analysis) it in effect concentrates on long run analysis and cannot be used to judge the effectiveness of rent controls in terms of their initial objectives of correcting a short run market phenomena. So while the report may be successful in considering the "extended" objectives of rent controls it offers no evidence as to whether the initial objectives were successfully met.

*The Distortionary Effects of Rent Controls*

Determining the effects of rent controls on rents is the most important hypothesis in terms of meeting initial objectives, but the other hypotheses give some indication as to the possible distortionary effects of rent controls on the housing market. However, without a thorough discussion of the theory relating to these effects it is not clear why the hypotheses are being tested.

The long run hypotheses considered in the Report can be divided into two groups. The first group relates to indicators of long run equilibrium levels. The relevant hypotheses are; housing starts, vacancy rates, and the long run rate of increase in real property value.

The theoretical basis for considering the long run effects of rent controls is in the context of the disincentives for investing in rental housing. Rent controls in the economics literature are viewed as an income transfer from landlord to renter. As a result, rates of return on investment are lower than they would otherwise be; housing starts decline and vacancy rates are lowered. The end result is a negative impact on the long run real property values in the case of rental properties. Unfortunately, the Report is not able to offer evidence regarding the trend in real property rates due to data limitations.

All of the above relationships rely on rent controls being effectively restrictive i.e., rent controls which are successful in preventing landlords from circumventing regulations. Landlords, have many ways to escape the grips of rent controls to increase the returns on their investment. The degree to which landlords can take part in this activity differs according to the strictness and comprehensiveness of rent control regimes which varies considerably across provinces.

Stanbury and Todd (1990), identify a series of measures taken by landlords to compensate for income transfers to renters. These include, converting rental units to apartment hotels, demolitions, shirking, cheating, selling and exploiting. This type of activity is partially captured in the Report in the evaluation of conversions and maintenance and repairs however the full range of effects is not considered.

In an effective rent control regime you would expect rates of conversion to increase as landlords attempt to increase their rates of returns by avoiding the restrictions of rent controls. Reducing maintenance and repairs (shirking) is an alternative way for landlords to improve their rate of return by reducing costs. Over time, renters begin to suffer as conditions decline and rents remain the same, or increase by the regulated amount.

An additional point regarding possible means of securing further evidence is warranted here. One method used to evaluate the effects of maintenance and repairs is to evaluate "rental prices" versus "rental value." Using data relating to rental prices and a range of quality factors (e.g., location, security) for regulated and non-regulated areas "hedonic" price indexes can be established to determine the "true" difference between regulated and non-regulated rents. The difference can be interpreted, in part, as a measure of the discrepancy in rental quality associated with landlord maintenance and repair efforts.

By engaging in a discussion, such as the above, the econometric results of the Report can be considered within the context of the recognized theory in the area of rent controls. The discussion also provides the necessary information to link the results of the statistical tests relating to the hypotheses. This link is missing from the Report, in its current form, although on a couple of occasions the authors point out that some of the hypotheses would be better tested in a structural model of the housing market which specifically identifies these links (p. 26).

### 2.1 Alternative Methodologies

The above discussion also points to three major areas where more information could be gathered in the area of rent controls. The first area is including rent controls as part of a much larger range of policies in the analysis of what factors affect the determination of rental housing demand and supply i.e., taxation, subsidies. As the authors of the

Report acknowledge this would involve the development of a large scale structural model of the housing market. The authors also acknowledge that data limitations may prevent the accurate specification of such a model.

The second area requiring further study is the short term effects of rent controls and their relationship to the initial policy objectives. The proper analytical approach to consider this issue is cross sectional analysis. By adopting an analysis of this type the true impact of the rent control "shock" to the market is quantified and the success of the initial policy is determined.

Perhaps the most fruitful area for further research is in the determination of the distortionary effects of rent controls. This could be accomplished it two ways. The first is to use the current estimation techniques to test hypotheses that cover the full range of possible effects of rent controls and landlord behaviours (e.g., the "premature" selling of rental properties to take advantage of allowed increases in rents).

The second approach is micro-economic in nature and investigates the long term costs and benefits of implementing rent controls. In this case, it would be most appropriate to select a jurisdiction having the most stringent regulations and develop an analytical framework for assessing the total costs of rent controls since they were imposed and compare these costs to the benefits that have accrued. Empirical evidence regarding the benefits of rent controls is limited but all such literature relates to the transfer of income from landlords to renters which is discussed earlier on in the comments. Linneman (1987), provides an analytical basis for considering these benefits.

In the case of Ontario, the costs have been substantial. Direct costs include: administration costs of rent reviews, legal costs for landlords and renters, and the extensive costs of lobbying (on behalf of both landlords and renters). Indirect costs are the total of all the negative distortionary effects described above. The quantification of these costs needs to be considered in the context of a disequilibrium analysis i.e., the long run inefficient use of resources caused by distorted investment incentives. In this context, and given the results of the Report, it is unlikely that the benefits of rent controls have outweighed the costs over the twenty year period.

The alternatives suggested above use a combination of approaches to investigate the effects of rent controls on the rental housing markets. In the interests of providing as much information on rent controls as is possible the alternatives suggest that a further specification of hypotheses is needed. Once this is accomplished, the appropriate tests can be applied using the existing estimation techniques in the Report. Complementing this approach with the development of a cost benefit analysis framework to consider the costs of rent control policies, particularly in Ontario, is needed in the ongoing debate on this issue.

The extensive research on rent controls has provided limited insight to date regarding the effects of rent controls on the rental housing market. Developing further evidence on the effects of rent controls is best accomplished with a multi-faceted approach until such time as econometric models can sufficiently account for many of the non-market activities (e.g., lobbying) which characterize the rental housing sector.

**3.0 Statistical and Econometric Methods in the Report**

The comments to follow concentrate on the application of the econometric methods in the Report to the analysis of rent controls, rather than on the details of the application themselves i.e., correct use of the F statistic. There is no indication that the report is lacking in the latter case.

There are two issues that relate to the suitability of the econometric methods in the Report. The first issue relates to the application of OLS in the determination of the impact of rent controls on rents. By using pooled time series and representing rent control regimes with binary variables it is not clear that the estimation will yield unbiased results. Rent controls when first implemented represent a price shock to the rental housing market. Jurisdictions that subsequently remove controls (with corresponding changes to the dummy variable), in the presence of uncertainty, experience another shock. Therefore, the timing involved in turning the dummy variable on or off is imperative in determining the effects of rent controls on rents since it is unclear what the state of the rental housing market is at the time the policy is reversed.

While the authors attempt to rectify this problem in their choice of data set (i.e., which years of the data set to include in the analysis), the choice is arbitrary and can make a difference depending on the province. As mentioned earlier, time series analysis is not the most appropriate means of testing the short run implications of such a policy.
The second issue relates to model specification. Given the form of the equations estimated, model specification is not a major concern. However, the analysis of the long run effects of rent controls should consider other government policy variables which affect both the supply and demand for rental housing. In particular there are several policies that have been used during the time of rent controls which favour owner occupied housing over rental units, i.e., housing subsidies. Subsidy programs have been reduced continuously over the past twenty years but they played an important role in determining the nature of housing stocks in Canada in the 1970s.

Furthermore, there are drastic differences in the tax treatment of owner occupied housing versus rental housing that should be considered in the analysis. The marginal effective tax rate on owner occupied housing (using 1985 data) was estimated to be 21.4 percent compared to 44.7 percent for investment in rental properties (Economic Council of Canada 1987). In fact, rental housing had one of the highest marginal tax rates of all forms of investment estimated. Considering this, the Report could have given more consideration to non-rent control factors which determine the supply and demand of rental housing supply in Canada.

One further comment relating to the statistical and econometric methods used in the Report deals with the use of the non-parametric methods to test the hypotheses. As the authors point out, results of hypotheses tests using these methods serve as further evidence that the findings of the parametric tests produce valid results. The application of these methods is definitely useful in this regard but more information regarding the limitations of this approach would be useful. Given that the methods have been limited to date in their application to economic analysis a more complete discussion is warranted. In particular, it should be noted that the non-parametric models do not provide support for the efficiency of the parametric equation estimations but only support the tests regarding the variable coefficients.

## 4.0 Summary of Comments

One of the main reasons for developing alternatives to the methodology of the Report is that the results of the econometric analysis are in some cases counter-intuitive. In all cases, with a minor exception relating to vacancy rates, the analysis shows that: rent controls do not reduce the rate of increase in rents in the long run, there is no evidence that the responsiveness of rental unit starts is reduced by the imposition of controls; there is no strong evidence that controls are accompanied by reduced vacancy rates; there is "some" evidence that rent controls are associated with a higher proportion of renter households; no evidence that rent controls affect the rate of conversion of single housing units to multiple units; and that there is no evidence that rent controls increase the proportion of rental accommodations that are in need of repair.

So, while the Report finds that rent controls have no long term effect on rents it also shows no evidence of supply effects (vacancy rates) or of adverse landlord behaviour (maintenance and repairs). Considering the persisting low vacancy rates in the 1970's and into the late 1980s, in major centres in Ontario such as Toronto and Ottawa leads to the following; what explains the apparent and continuing disequilibrium in the rental housing market in Canada?

As Arnott (1987) states in a major international review of rent controls, "There have been no econometric studies which have succeeded in disentangling the effects of controls from those of the many other housing market policies and of the many other factors -- notably demographic and macro-economic -- which influence the markets operations.". Whether the current Report has managed to solve this dilemma is unclear. However, it is clear that it makes considerable headway. Questions remain as to whether the specification of the models in the Report do sufficiently "disentangle" rent controls from other policies and factors. Including a more comprehensive specification of the equations regarding other housing policies that affect rental housing may answer that question.

With regard to the most important questions relating to the inefficiency of rental housing markets, several points of the above discussion may be useful.

- More appropriate testing of the short run effects of rent controls on rents and other factors could provide further evidence regarding whether rent control polices have been successful in meeting there initial objectives.

- Comprehensive analysis of the costs and benefits of rent controls, for a major metropolitan centre, would provide further evidence regarding rent controls that would be useful for housing policy makers.

- Evidence currently exists relating to the disincentives for investment in the rental housing market. While most rate of return studies relating to rent controls focus on specific metropolitan centres, further research that considers the full range of tax incentives and other government policies which favour other investments over rental housing, may shed light on some of the persistent supply problems that exist in some rental housing markets in Canada.

In many instances the above comments suggest that econometric limitations persist in capturing the full range of effects of rent controls. However, the Report does, for the first time, establish building blocks for continuing analysis of the subject using both econometric methods as well as other well established analytical techniques. By continuing a multi-faceted approach to analysis, evidence can continue to be gathered to determine whether rent controls have been successful in meeting their objectives, and at what cost.

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Jan. 18, 1994

Sharon Olm,
Senior Analyst,
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CMHC
Ottawa

Dear Ms. Olm,

Enclosed is a review of the study "Testing Hypotheses About Rent Controls," which you requested in your letter of Dec.10, 1993.

I suspect that the reason you asked me to participate in this exercise is because I authored a paper on the use of randomization tests in econometrics, referred to by the authors. It is certainly not because I am knowledgeable about the housing or rent control literatures; my comments on the study should be read bearing this in mind. I do have extensive experience in econometrics, however. I am the author of a popular book in the area, A Guide to Econometrics (3rd edition, MIT Press), I have published several econometric papers in academic journals, I regularly teach econometrics at both the undergraduate and graduate level, and I edit the research section of the Journal of Economic Education, which means I spend a lot of time evaluating applied econometric work in the economic education area.

I have no quarrel with the empirical techniques employed in this study; many are rough and ready, but this is common and not likely to lead to any great injustice. My main objection is that the specification of the regression model is unsatisfactory, for a variety of reasons, so much so that I am quite reluctant to view the empirical results as having any meaning. My suggestions for improvement are twofold: improve the specification and gather new data. In some instances I have spelled this out, but for the most part they are implicit in my commentary.

Should you decide to include me in your early March colloquium I must warn you that I have previous commitments on March 7, 8, 9 and 14.

I enclose an invoice re this review.

Yours sincerely,

Peter Kennedy
Professor of Economics

**Review of "Testing Hypotheses About Rent Controls" by Quantec Research**

This study uses available data to examine via a regression model several hypotheses regarding rent controls in Canada. The authors are to be commended for writing the document so clearly - with only a few exceptions (due to my ignorance of terminology in this area) I was able to understand what had been done and what the results were, and throughout I was left with the impression that these authors are competent researchers, concerned about what can legitimately be concluded from their empirical results.

My remarks below stem primarily from my ignorance of the housing market and rent control literature. It is unfortunate that the authors chose not to review this literature, or indicate which parts of their specification were influenced by that literature; in my opinion the most crucial ingredient of any analysis based on a regression model is the regression specification, usually heavily influenced by prior work in the area. As my remarks below indicate, I do not feel comfortable with the specifications employed in this study.

**Specifying Reduced Forms**

I am always nervous when researchers state that they are not going to spell out a structural model but instead are going to move directly to a reduced form. My reason for this is that all too often this is used as an excuse to throw all sorts of variables into an equation without much thinking about whether the specification makes sense. I fear that this may be the case in this study: My generic problem here is that I can't envision a sensible structural model that could give rise to the authors' reduced forms.

Let's look at the logic of the eqn 7 specification. Several things strike me as strange.

a) Rent controls are assumed exogenous. The decision to apply rent controls, and the levels at which they are set, seem to me to be endogenous. Is it a common assumption in the literature that they are exogenous?

b) There is an odd mixing of real and nominal variables. The dependent variable is the nominal rate of change of rents, with the rate of inflation included as an explanatory variable, presumably to capture the fact that in equilibrium the nominal rate of change of rents should roughly match the rate of inflation. Why not make the dependent variable measure real rents or real rent changes? What is relevant here? Does interest focus on the impact of controls on nominal rents or on real rents? Why is real income an explanatory variable when nominal rent change is the dependent variable? Why is the nominal interest rate used rather than the real rate?

c) There does not appear to be a sensible equilibrium. Consider for example the logic of the connection between the nominal rate of change of rents and the rate of population growth. In the short run, a sudden jump in the rate of population growth should increase the demand for the stock of rental housing and because supply cannot respond immediately, there should be a higher real (and thus nominal) rental price. Over time this higher real price should bring forth extra supply, both to eliminate the excess demand, and to increase the flow of supply to meet the higher flow of demand. In short, the price moves over time to a new, higher real equilibrium price. This implies that in the long run the nominal price <u>change</u> falls back to its normal level (equal to the rate of inflation, say). But the authors' specification has the higher rate of population growth leading to a <u>permanently</u> higher rate of change of rents. A similar argument can be made re growth in real personal disposable income.

This lack of a sensible equilibrium plagues eqns 12, 13 and 14, variants of the generic eqn 7. And in eqn 11 surely the levels of v* and R/P should be allowed to influence starts, in addition to the dynamic (disequilibrium) influence of their rates of change?

**The Role of Relative Price**

In eqn 9 appears a real relative price. Is it the relevant price? Yes and no. What is happening at the economists' infamous margin? It seems to me that there are two generic things happening:
a) people switch from rental housing to home ownership and vice versa, for which the relevant price is the relative price of renting versus home ownership; and
b) people switch from rental housing to living in the streets, with friends or with relatives, for which the relevant price is the price of rental housing relative to the consumer price index.
Why is the latter relative price present, but not the former? What is thought to be going on in this market to lead to this choice of relative price measure?

**Defining Rent Control Dummies**

The authors never define/explain the difference between a rent control program with mandatory review and a program with voluntary rent arbitration, so it is difficult for a reader not familiar with the associated literature to pass judgement on what difference this distinction is likely to make in the empirical work. This is unfortunate, because the rent control dummies are key ingredients in this empirical work; the way they are specified (defined/measured) can have a substantive impact on the results. A major characteristic of a rent control program, which for this study is crucial to know, is how the rent control levels are determined. Have they deliberately been set at a level designed to keep them below the long-run market price? Have they deliberately been set to "smooth" changes over time but allow the long-run market prices ultimately to prevail? Have they been designed to affect only low-rent rentals but not high-rent rentals? Do they have built into them some safeguard to ensure "fair" returns to landlords? Do they impose effective penalties on landlords who ignore repairs? Etc.

What is needed for this study is some measure of the degree of tightness of the rent control programs - existence of a program doesn't imply much, since the controls imposed may not be binding, or may barely be binding. The authors have done a lot of work to investigate past and present rent control programs, and all they have to show for it is a couple of dummies. I can understand the reluctance on the part of researchers to shy away from concocting subjective measures, but there are times, and I believe this is one of them, when a subjective measure may be more meaningful than available objective measures.

The relevance of this to specification is evident from examining the way in which the data period is broken into subperiods of strong vs weak upward pressure on rents. Although the authors explain clearly how this was done, they do not explain the rationale behind their choice of method. Their method identifies years in which an area is experiencing strong versus weak upward pressure on rents relative to the average for that area. This is not a measure of strong versus weak upward pressure on rents in the usual sense: if, for example, one area has little or no upward pressure on rents in all years, despite this roughly half of these years will be coded by this method as having strong upward pressure on rents. Why does this make sense?

One guess as to the logic here is that this is an effort to measure whether the upward pressure on rents exceeds or falls short of the control level in that area, set for that year by the rent control program, and these authors are assuming that this control level is set in all years at that area's long-run area average. Is it true that the control level is fixed in an area? If so, is it reasonable to assume that it is fixed at a long-run average? Don't the authors have data on what the control level is in each area for each year?

The bottom line here, as with earlier remarks, is that I think a much better measure/specification could be produced.

**Measurement Problems**

It is unfortunate that the authors were tied to using only available data; this creates problems which in some cases prevented the examination of hypotheses of interest and in other cases caused the authors to use data which may not allow legitimate tests of hypotheses of interest. The one that worries me the most is the substitution of apartment unit starts and vacancy rates for rental unit starts and vacancy rates. Although as the authors note it is true that economists often must make use of data that is not ideally suited to the task at hand, some assumptions in this regard are more serious than others.

The ideal measure of rents was not available because the available figures included rents in the not-for-profit market. Ignoring this problem seems reasonable - changes in the rent indicator over time should reflect adequately rent changes in the for-profit market. The ideal measure of new construction of for-profit rental units was not available for three reasons. First, the available figure omits row and detached housing. Ignoring this seems reasonable since there is no reason to believe that rental row and detached housing starts should differ in any fundamental way from other for-rental starts. Second, the available figure includes social-housing starts. This is unfortunate. If social-housing starts are increased in times of housing shortages (as one might expect to be the case), when rental controls are likely to be imposed, this inclusion should tend to bias upward any estimate of the effect of rent controls on rental starts. Third, the available figure includes apartment starts that are for sale as condominiums, not just apartment starts that are to be rental units. This is most unfortunate. If when rent controls are applied apartment builders switch to building condominium apartments (as one might expect to be the case), then an empirical analysis using this measure should find no influence of rent controls on apartment starts.

The seriousness of these latter two data problems is a matter of subjective judgement; they seem not to bother the authors much, but they bother me a lot - both cause a bias, in my opinion substantial, toward finding the results that the authors actually find. What can be done about this? Here my ignorance of the housing literature is a disadvantage. Perhaps in this literature there are studies concluding that social housing starts are unaffected by things that might prompt imposition of rent controls? And perhaps it is common knowledge in this literature that the fraction of apartment starts that is condominium vs rental is unaffected by rent controls? Failing this, some legwork would have to be done either to verify these results, or to construct a proper measure of for-rental housing starts. This would involve constructing new data - selecting a random sample of apartments and investigating whether they were rental or condo when built, etc.

**Miscellanea**

1. The nonparametric testing approach should be called a distribution-free testing approach; the model and what is being tested are still parametric.

2. Some equations allow the intercept to differ from area to area, but some do not. Eqn 9 does, but eqn 8 does not, for example. Is there some reason for this? For the latter case should you test that the intercepts are insignificantly different from one another?

3. Do most rent control systems have in place mechanisms to force landlords to do repairs? If so, this might explain the results. Maybe you could classify the control programs into two types, those ignoring the repair problem and those with specific legislation re repairs, and see if the major repairs incidence is greater for the one than for the other.

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March 2, 1994

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Dear Ms. Ohm:

Please find following our comments on the study "Testing Hypotheses About Rent Controls." As I indicated in a phone conversation, I did this jointly with my colleague Professor A.E. Myatt, also at the University of New Brunswick.

Both my colleague and I have extensive experience in the econometric modelling of provincial phenomenon and I have also undertaken research on modelling the housing market. Indeed, recently one of my M.A. students completed his thesis on the interaction between the housing market and interprovincial migration.

I am also sending these comments by mail together with our cv's.

Please do not hesitate to contact me should you require further information or clarification of these comments.

Yours sincerely,

William J. Milne,
Professor of Economics and
Vaughan Chair in Regional Economics.

Comments on

# "TESTING HYPOTHESES ABOUT RENT CONTROLS"

## 1. Introduction

This report examines the effect of rent controls through a series of hypotheses related to a number of variables including:

(1) rent
(2) housing starts
(3) vacancy rates
(4) property values
(5) tenure preferences
(6) conversions
(7) maintenance and repairs

While CMHC had identified the hypotheses, the authors of the report were unable to explicitly test these hypotheses due to data problems; consequently, a different set of hypotheses were set out and evaluated in the report.

In these comments, we begin by noting the good features of the report. We then turn, in Section 3, to some general discussion about the way that the effect of rent control regimes (or other regimes like rent controls) should be measured including a discussion of appropriate econometric techniques. Next we consider, in broad terms, the way that the exercise should have been carried out through the use of a structural model. In Section 5 we provide some comments on the data used in the analysis and make some general comments on the results. Section 6 sets out some comments on the individual results presented in the report while Section 7 considers the notion of classification of rents. A final report summarizes our comments.

## 2. Good Features of the Report

Certainly a useful piece of the report is the articulation of the rent controls in the different Canadian provinces. This review of the situation could have gone further, however. The authors themselves point out (page 15), "... it should be recognized that there can be considerable differences in the details and practical applications of controls within each type of regime, either from period to period or from province to province. Indeed, the variation within a given type may be as great as the variation between types." However, they do not spell out in sufficient detail the differences within regime. Thus, it is hard for the reader to evaluate the seriousness of the problem here. On balance, taking into account both the econometric results and the review of the legislation, we would suggest that when using pooled data only the B-type regimes be included. (That is, focus only on those provinces where the rent control is mandatory.) However, as noted below, we have reservations about using pooled data.

In terms of the econometric analysis, the use of non-parametric tests was interesting. The non-parametric randomization tests seem to provide a useful way of corroborating the evidence found in the standard econometric analysis. We also like the approach (see page 41) of obtaining the standard errors associated with the long run parameters using the procedure developed by Bewley (1979). We also take note of the logit transformation of the vacancy rate when it is used as a dependent variable. Since the vacancy rate lies between zero and one, the logit transformation allows the dependent variable to lie over the entire number range.

## 3. Measuring the impact of rent controls

To begin, rent controls are a type of price ceiling. Any price ceiling will be irrelevant when it is imposed above the equilibrium (free market) price. In the rental market situation, the control takes the form of a specified rate of price change (rather than a price level). Thus, the analyst must estimate the free market rate of change in rental prices, and compare this with the control regime. Thus, the fundamental question is not "What is the effect of different types of rent controls?" but rather "In this situation, is the control rate of price change below the equilibrium (free market) rate of price change?". This more fundamental question is not addressed in the report. This issue is related to the correct modelling of the rental market -- a topic we return to later.

A second critical issue is whether it makes sense to examine the impact of the controls using pooled data. After all, the legislation is different in each province, and, therefore, the potential effect of the control is likely to be different. By pooling the data it is assumed that the effect on the rate of change of rents, for example, is the same regardless of the province.

Third, the authors of the report have taken a standard approach to measuring the impact of rent controls. That is, they have introduced dummy variables to reflect the type of rent control (either Type B or C). Most often, these dummy variables have been introduced so that the intercept of the equation changes. Only in the case of apartment unit starts do they allow for slope changes. We find this to be too restrictive. We would have preferred allowance for both intercept and slope effects throughout the report.

We think the first test that should have been undertaken is a simple Chow Test to see if the structure of the equation had changed when the regime was in place. That is we would have thought a useful starting place for the analysis would have been to undertake an F-Test on the equality of all the coefficients with and without the controls.

In cases where the regime is changing, it might be useful to consider the use of spline functions. Spline functions allow for different slopes for different periods of time. In this sense, the response of parts of the rental market can be different for different periods and types of rent control. This technique has also been used as an alternative to including non-linearities through identification of certain "knots". In the case of the spline function, these "knots" must be known a priori.

Finally, we suggest the possibility of using a switching regression regime. These models are most useful when changes in regimes are possible (as is the case here where rent regulations are changing). In this switching regression scheme, the point of the "switch" can be determined through a search procedure.

## 4. The need for a disaggregated model

To begin, we should note that we do not feel that pooling the metropolitan areas is particularly appropriate. If there is one thing that is known with certainty, it is that the housing market is a regional phenomenon. Different regional housing markets may react to events in remarkably different ways. One need only consider the effect of immigration on Vancouver, Toronto and Montreal to understand that it would not make sense to pool these CMAs together with markets that have not seen such large demographic effects.

This leads to our second point. We believe the authors should have built a structural model of demand and supply, and in particular, included variables that have a large impact on the housing market. For example, there can be little doubt that demographics have played a large part on the demand side of the housing market -- both in terms of the baby-boom generation moving from rental units to single dwelling units, and as noted above, the effect of immigration. Furthermore, there can be little doubt of the impact of interprovincial migration in some markets. For example, rents increased in Calgary during the energy boom in the late 1970s and early 1980s and this can, at least in part, be attributable to interprovincial migration.

Therefore, we believe that a structural model consisting of a demand for and supply of rental units is the appropriate way to proceed. On the demand side, besides the demographic variables noted above, the authors should have taken account of the price of substitutes. Variables from the housing market (as substitutes) in addition to real per capita income should be included. On the supply side, in addition to interest rates, variables related to the costs of building rental units could have been explicitly included. In addition, provincial policies, such as tax policies, should have been explored.

Overall, then, we think a better way to tackle this problem would be through a better specified structural model of the rental market.

## 5. Comments on Data and Other General Comments

We think it would have been useful if there had been more discussion of problems with the data in Section 4 of the report. While the authors do discuss the availability of data, we wonder about the appropriateness and quality of the data they have used. In particular, we raise the following points.

1. personal disposable income - the use of provincial income rather than metropolitan area

income seems like it could introduce serious biases especially for metropolitan areas like Toronto or in largely rural provinces like Saskatchewan or New Brunswick. More effort should have been put into developing measures of real disposable income in the CMA, perhaps through partly using information available from the census.

2. when deflating the provincial income, the authors use the provincial consumer expenditure deflator. However, when analysis was at the metropolitan level deflation was undertaken with the metropolitan CPI. We wonder if there are advantages to using the consumption deflator or if it would not have been better to use the provincial CPI at the provincial level for consistency reasons.

3. interest rates - the authors have used the three-month treasury bill as the relevant interest rate. On the supply side of the rental market it is likely that a longer term interest rate is more appropriate. We wonder if the authors experimented with other interest rates to see if the results were sensitive to this choice.

4. rental prices - we wonder if there are any drawbacks with using the rental part of the CPI as a measure of rents. It is likely, however, that the authors had no choice in this regard.

5. when trying to identify the impact of rent controls one must always be concerned about the level of aggregation. The issue here is the vintage of the units. In Metro Toronto, for example, the rules are different depending on the age of the apartment building. Table 2 (of the report) documents that there are exemptions in most provinces associated with the vintage of the unit. Of course, this is not an issue that the authors can do much about since the data are simply not available. Nevertheless, it is an important issue that should be discussed in their report.

6. much of the rest of the data for the housing market is not of high quality. This seems to be a common problem in undertaking studies of the housing market. The only issue we would raise is for the authors to suggest potential effects on their results as a result of data choice.

We note that the discussion of the time period of the data used for the analysis (pages 27 and 28) seems odd. In particular, how is it possible to drop some years in a dynamic model such as that constructed here. That is, does it not create holes in the data set. The authors acknowledge that it invalidates standard tests for serial correlation. We believe that this procedure creates more problems than it solves.

We also think that it is worth noting that whenever time series data are used there is concern about spurious correlation. Consequently, it is common for time series data to be checked for unit roots and to ensure that the relevant series are stationary. While it may have been beyond

the scope of the project to consider co-integrated series, we believe the authors should at least have undertaken standard time series tests (such as Dickey-Fuller or augmented Dickey-Fuller tests) to shed some light on the potential for spurious regression in the results.

## 6. Comments on Specific Equations

In this section, we provide comments on each of the equations estimated.

### A. Rents

We have several concerns about this equation:

1. Why is the dependent variable not the real percent change in the rent rather than the nominal percent change? To put the point another way, the equation should be made homogeneous of degree one in the inflation rate (in the long run) -- that is, the sum of the coefficients on the inflation rate terms should be one.

2. On a related issue, why not include the real interest rate rather than the nominal interest rate and the inflation rate. Then by using the percentage change in the real rent as the dependent variable, homogeneity of degree one is imposed. This economises on degrees of freedom.

3. It seems wrong to leave out demographic variables. Over this sample period (1971-1993) the baby boom entered the labour market and this surely had an impact on the demand for both housing and rental accommodation. Variables could have been constructed to capture this effect. For example, the proportion aged between 25 and 35 in the total population.

4. Another explanatory variable that seems to be missing is the percent change in house prices (since this is an obvious substitute for rental accommodation).

5. The only role for B and C type rent control regimes is to change the intercept of the estimated equation. In fact, as noted above, there must be other ways that the rent control regimes have an effect. In particular, they should allow for changes in the slope coefficients. The authors could have included some interactive terms.

6. The authors point out that they cannot determine a-priori expectations about signs of coefficients since the equation is a reduced form. However, surely some of the signs can be determined. For example, population growth must enter positively. All of this suggests that the authors might have been better served by building a structural model rather than using a reduced form.

7. Following along on the idea of building a structural model, it would seem appropriate to consider the impact on rents (or other parts of the rental market) of tax breaks associated with owning rental accommodation. (For example, repairs are tax deductible.)

8. The authors note that the dummy variables for the metropolitan areas were not significant in these equations. It would have been useful if the complete results were included in an appendix. In this particular case, since a form of this equation is used to classify rent periods, a feel for the insignificance of these dummy variables would have been useful. We wonder if a test of the **joint** significance of the "MA terms" in all equations was undertaken.

B. Housing Starts

1. The authors seem to suggest that the appropriate specification is

$$S = f(v, r, \Delta v, \Delta r)$$

If this is the appropriate model, then why not estimate it? That is, why should two separate equations be estimated, one on levels and one on changes?

2. Surely starts of apartment units should be related to other parts of the housing market (for example, house prices).

3. The dynamic structure used here is more restrictive than that in the equation for rent changes. It would seem more appropriate to allow for a more general dynamic structure (at least as large as that allowed for elsewhere).

4. In these equations, the regime changes are allowed to affect the slope terms only. It seems like there should also be intercept effects. This brings us back to the more general point about testing for the effect of the controls. In all cases both intercept and slope changes should be considered.

C. Vacancy Rates

1. As noted above, we like the use of the logit transformation in this equation

D. Proportion of Renter Households

1. It would seem more appropriate to estimate a demand function directly. Once again, the decision to estimate a reduced form model is causing problems. The original CMHC hypothesis concerning a shift in renters preferences could have been tested. It is not

necessary to have data on preferences. Preferences are revealed in the market. All that is necessary is to identify the demand equation for rental accommodation, and then test to see it is shifted by the imposition of rent controls.

2. Once again we feel a structural approach would have been preferable since there can be little doubt that demographics strongly influence tenure choice. In addition, one might expect that general economic conditions would have an impact on the choice of dwelling type.

E. Conversions

1. Obviously the issue of conversions is especially important in large metropolitan centres, like Toronto, where conversion of rental units into condominiums was common in the mid 1980s.

2. The data are not very good here. As a result, further research on this issue would not appear to be fruitful.

F. Major Repairs

1. The specification seems far too simple. There are still 50 observations and therefore some type of structural model should be attempted.

## 7. Classification of Rents

The authors try to classify rent increases according to whether there was weak or strong pressure on rents. They form these periods by first estimating a pooled equation for the percentage change in the nominal rent based on rates of change and first differences of rates of change of: population, real personal disposable income, the interest rate and the inflation rate (page 39). Upon estimation, the first differences and the dummies for the rent controls were set equal to zero and a mean $r^*$ was computed for each metropolitan area.

This does not seem to make a great deal of sense to us for several reasons. First, the dependent variable is in nominal terms, and taking an average over the period 1974-91, seems misguided. Second, there were no dummy variables included for the different metropolitan areas and this is a case where these definitely seem to be warranted. Third, the real issue here is not if the rent controls had a different effect in different periods but whether they had an effect at all.

## 8. Summary

This report has provided an attempt to explore certain hypotheses about the effect of rent controls on the various parts of the rental market. The impact of rent controls is, of course, important to understand -- particularly in terms of their long run effect on rental accommodation.

The results in this report are somewhat worrisome since, in general, they suggest that rent controls have not had a major impact on rental market variables. This is contrary to very stronly held prior beliefs, and very strong anecdotal evidence. These results, of course, are based on the specification chosen (in this case reduced forms) and the data used.

We have three broad concerns about the results in this report:

1. it would have been more useful to build a structural model of the rental market so that obvious variables which influence the market and which could be correlated with the timing of the controls can be taken into account;

2. more extensive econometric procedures should be used to look for the impact of the rent controls;

3. greater discussion of data issues and the potential impact of the data used on the results.

February 1994

# "TESTING HYPOTHESES ABOUT RENT CONTROLS"

## BY FRANK T. DENTON ET AL.

Reviewed by Soo-Bin Park

The primary objective of the above study by Dr. Denton and others of Quantec Research Ltd. was to test for a range of hypotheses about the effects of rent controls on the Canadian rental housing market. The study finds "no evidence" that rent controls influence key endogenous variables of the rental housing market including (1) the long-run rate of change in rents, (2) apartment units starts, (3) rental vacancy rates, (4) the rate of conversion of single-family homes into multi-family units, and (5) major repairs of rental units. The study also finds "some evidence" for rent controls to increase proportions of renter households. These finding are interesting. Section 5 on Survey of Rent Controls is also highly informative.

It is unknown, however, how large the probabilities of not rejecting false null hypotheses would be. Null hypotheses of no effects may not have been rejected because the power of the tests employed is low or because the models used for testing purpose may be misspecified. Hypothesis testing calls for sufficient care and thought in the initial model specification and a thorough diagnostic checking of an estimated model. This review provides some comments on the final report of the study.

## 1. The Conceptual Framework

Although the "construction of an econometric model of the rental housing market in each province or metropolitan area was beyond the scope of the project" (p.16), the report could have discussed the conceptual framework of a rental housing market. The authors briefly describe how the rental housing market operates to justify their choice of exogenous variables to be included in their models. (p.19) An in-depth discussion on the conceptual framework of the rental market would have enabled the readers to know how the authors see the interdependence of the endogenous variables they have modelled in the reduced form equations and what the appropriate exogenous or predetermined variables would be.

## 2. Pooling Time-Series Data of Cross Sectional Units

Pooling data from sixteen metropolitan areas (or ten provinces) into a single sample for estimation is valid only if the models of sixteen metropolitan (or ten provincial) rental housing markets have a common structure. By a common structure one means not only that the sixteen metropolitan area (or ten provincial) models include the same list of endogenous and predetermined variables but also that their model parameters are identical in value. Clearly, the assumption of a common structure for the sixteen models is highly restrictive. It is very unlikely, in this reviewer's opinion, that the rental housing market of, for example, St. John's has a common structure with that of Toronto. Whether pooling is appropriate or not is a serious issue in the study. If pooling is not appropriate, conclusions derived from the analysis of the pooled data are not valid.

The data analyzed consists of at most 22 annual observations from each metropolitan area (or province). The degrees of freedom problem suggests that some kind of pooling is unavoidable. The authors decided to pool the data into one single sample. They could check if pooling into a single sample was appropriate by pooling the data into several subsamples, estimating the models for each subsample, and examining to what extent the parameter estimates would vary across the subsamples.

The issue of a common structure has partly been recognized in the study. The authors introduce dummy variables for metropolitan areas (or provinces) to allow for possible differences in the intercept terms in Eq.(9) on housing starts and also in Eq.(13) on the proportion of renter households. It is quite plausible that the "slope coefficients" also differ from one metropolitan area to another.

## 3. Reduced Form Representation

Of the six equations that the authors have estimated, five are meant to be in the reduced form. A well-known characteristic of the reduced form of a simultaneous equation model (SEM) is that the reduced form equations have a common set of explanatory variables consisting of all predetermined variables of the SEM. An odd feature of Eqs. (8), (12), (13) and (14) that the authors consider to be in the reduced form is that they do not have the same set of predetermined variables. They do have the same set of exogenous variables. But they include only the lagged left-hand-side endogenous variables and omit all other lagged endogenous variables of the model that appear in other reduced form equations. Specification errors of

excluding relevant predetermined variables from the reduced-form equations would render the test statistics used in the study inconsistent and the conclusions invalid.

## 4. Specification of the Equation for Starts of Rental Units

Eq.(9) for starts of rental (or apartment) units is "in the nature of 'structural equations" and includes the rental vacancy rate, real rents and the lagged starts as the explanatory variables. Since the expected profitability of rental housing construction given the rents and the costs of new construction is the primary determinant of the rental housing supply, this reviewer finds Eq.(9) to be misspecified in that the cost of capital for the owner of rental housing and construction costs are omitted from the equation.

## 5. Exogenous Variables

The authors have "chosen" four exogenous variables: (1) the rate of population growth, (2) the rate of change in per capita real disposable income, (3) the rate of inflation, and (4) the three-month Treasury bill yields. This reviewer is not sure if there are no other relevant exogenous variables. Other possible exogenous variables include (1) the beginning of the period stock of rental units, (2) construction costs and (3) consumer price index for owner-occupied housing. Perhaps one could appropriately argue that the price of services from owner-occupied housing should be treated as endogenous. There will then be even more exogenous variables that should have been included in the study.

Another related and equally important issue is how predetermined variables should enter in the reduced form equations. This reviewer does not understand why three exogenous variables enter the reduced form equations in the rate-of-change form and not in the level form.

## 6. Effect of Rent Controls

In the context of a structural or reduced-form equation for an endogenous variable of the rental housing market, the effect of rent controls on the endogenous variable may be represented by three different types: (1) a shift in the intercept term, (2) changes in the slope coefficients, and (3) changes in both the intercept and slope coefficients. The authors have considered Type (2) for housing starts and Type (1) for all other endogenous variables. Why did they a priori specify different type of effect for different endogenous variables? Why was Type (3) ruled out?

## 7. Proportions as an Endogenous Variable

Three endogenous variables are in proportions and limited in value from 0 to 1: the proportion of the renter households out of the total households in Eq.(13), the proportion of conversions from singles to multiples out of the total single units in Eq.(14), and the proportion of the occupied rental dwellings classified as in need of major repairs out of the total occupied rental dwellings in Eq.(15). When the dependent variable in the regression equation is a proportion, the error term in the model is known to be heteroscedastic. Moreover, the proportions in Eqs. (14) and (15) should often be close to or equal to zero. The report does not

say what special care, if any, has been taken to accommodate the special nature of the dependent variables in the three regression equations.

## 8. **Bootstrapping**

It is interesting to see that the so-called bootstrapping method has been used in the study to supplement the conventional t or F tests of hypotheses. Eq.(9) is a structural equation. This reviewer wonders if a reduced form equation for the starts variable was estimated and used to generate the bootstrapped data.

## 9. **Data on Maintenance and Repairs**

For testing in the "maintenance and repairs" category, the authors could have used the annual data on expenditures on maintenance and repairs of tenant occupied housing. (CANSIM Matrix 0439).

## 10. **Typos**

The following appears to have been omitted from Eq.(16) by mistake when typing:

$$+ \gamma_1 \Delta r_{it} + \gamma_2 \Delta r_{i,t-1} .$$

# REPORT ON
# Frank T. Denton, Christine H. Feaver, R. Andrew Muller, A. Leslie Robb and Byron G. Spencer, "TESTING HYPOTHESES ABOUT RENT CONTROLS"

by
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February, 1994

## *I INTRODUCTION*

In this report, I first put forward comments on the study. I make general comments and then comment on the economic analysis of housing markets in the study, technical aspects and finally the data. In III I suggest possible improvements, some minor and some major. In IV I discuss an alternative method of assessing the effects of rent control.

## *II COMMENTS*

### II.1 *GENERAL*

There is much to applaud in this report. The careful systematic setting out of hypotheses is impressive. The information on rent control regimes in the various provinces is well organized and valuable. The application of the hottest set of techniques in econometrics, nonparametric methods, is noteworthy.

The modelling in this report is the first, following the pioneering work of Muller (1990) I have seen to use CMA level data, over time, for a substantial set of housing market equations. The pooling of time series and cross-section data means that the authors have more observations than is usual in time series models. In addition, it recognizes and takes advantage of the fact that housing markets in different parts of the country vary greatly. Using such data places a substantial burden on the authors because it means much more data investigation is required. Because of the disaggregation into CMA's there also will be more noise in the model than is the case in a national model, and so a lower adjusted $R^2$ should be expected.

While the econometrics in this report in general is very strong, the housing market analysis in not, and unfortunately, this weakness has econometric implications. The evidence of the test statistics is less strong than it would be if the housing market analysis had been stronger. Nonetheless, I am inclined to take the study's non-rejection of most hypotheses (that in various respects rent control does not have an effect) as a good first approximation; while I make various suggestions for improvement of the study, I do not think that the results of an improved study would be much different.

My suggestions must be read keeping in mind the fact that time series or pooled data modelling is a difficult art, especially of the housing market,[1] and it is much

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[1] Patrick Grady, in his review of macroeconomic modelling in Canada (1985) comments that the housing market is "...extremely difficult to model..." (p. 237) and in the final

easier to suggest improvements to models than to implement them. It is also important to keep in mind the budget and time constraints binding this study.

One reason that I do not expect a revised study to come to different conclusions is that the authors' results are so consistent: in not just one case but in practically every case, they find no effect. Whatever the modelling and data details one can complain about, it is difficult to discount such a strong result. If rent control had the strong effects it is popularly believed to have, they would surely show up very strongly in *several* equations. Here, in equation after equation rent control does not have *any* effect or has an effect only when an undemanding level of significance is used.

Another reason that I expect a revised study to come to no different conclusions is that other recent studies have found it to have no effect.[2] I believe part of the reason for this is that rent control regimes in Canada and the United States in recent decades have been much less constraining than the classic textbook version of rent control--so much so that they are often referred to as second generation rent control. In addition, they generally have controlled only part of the market; thus, even if rent control has affected *some* units in the market, that effect is apt to be offset in the remaining part of the market, so that *overall* there is no effect.

## *ECONOMIC ANALYSIS OF HOUSING MARKETS*

### *General comment*

The authors bravely test hypotheses about housing markets without doing much more than a quite casual analysis of these markets. The authors are entirely correct to say that for their purposes, a complete structural model of the housing market is not required (as they note, they need only reduced form equations) and that collinearity among variables, other than those which are the focus of the hypotheses tested, does not matter.

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paragraph of his report singles out the housing sector, commenting "...the housing sector of most models is generally unsatisfactory..."(p. 250)

[2] The authors cite Rapaport (1992). In addition Gyourko and Linneman, for the New York Style regime applying to quite recent buildings (and quite like the typical Canadian regime) find no effect. Honig and Filer (1993) find that rent control has no effect on crowding, doubling up and homelessness. Also, in a study of the Toronto and Ottawa rental markets (1993b), I found, like the authors, that rent control, if anything had the effect of raising rent.

Nonetheless, the correctness of the specification of the variables other than rent control variables will, in general, matter. To the extent that variables which should be included in the reduced form are not included, the estimates of the coefficients of the variables of interest will be biased (assuming collinearity is present). Measurement without theory is problematic even when hot econometric techniques are used.

Let me illustrate the point by commenting on one aspect of the specification. First, consider the demographic variable. The study uses, simply, total population as an indicator of demographically driven demand. This is surprising in view of the fact that for years CMHC has been using weighted sum (and the change in this weighted sum) of the population in various age and sex groups in its projections of total households (i.e. dwelling units) and components such as rental dwelling units. In addition, in the econometric housing literature are variables closely related to those in the projection literature (e.g. for Canada, see Grady, 1985 and for the U. S., Hendershott, 1980). A sum with weights based on amount of housing stock demanded was introduced in Mankiw and Weil's famous piece (1989) and was applied to Canadian data by Englehardt and Poterba (1991).

To illustrate why this specification might matter, consider the fact that the population under 20 has virtually no impact on demand. In the 1970s this population was proportionately more important than in the 1980s. Thus total population understates demographically driven demand in the 1980's relative to the 1970's, especially the early 1970s. Whether this matters or not depends on the kind and extent of collinearity between demographic variables and independent variables of policy interest.[3]

*The rent equation*

Standard analysis of the rental housing market (e.g. Follain, Hendershott and Ling, 1988; Steele, 1992)) assumes that in long run equilibrium rent net of tax must equal net of tax user cost; equivalently, the risk adjusted rate of return on housing must equal the rate of return on another asset. Thus, nominal rent in the long run depends on user cost, which in turn depends on the nominal price of housing stock, the nominal interest rate, expected rate of capital gain, the marginal tax rate of the investor, other tax variables, and the cost of utilities etc.

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[3] Englehardt and Poterba (1991) find their demographic variable has no statistically significant effect on house prices.

In the short run rent depends (see, e.g. Smith, 1974a, 1974b) on the demand for rental housing (which in turn depends on variables such as income, weighted population) relative to the existing stock, and the vacancy rate.

The study's rent equation is then missing

short run variables:
- rental housing stock
- vacancy rate

long run variables:
- components of user cost:
  - expected rate of capital gain
  - price of the housing stock
  - marginal tax rate
  - other tax variables

It might appear that the study's specification A does allow for the effect of expected capital gain because it includes the rate of inflation, an often used proxy for expected capital gain (Englehardt and Poterba,1991; DiPasquale and Wheaton, 1994), . Unfortunately, however, since the dependent variable in A is in terms of percentage change, all the independent variables should be entered as percentage change, so that the percentage change in the rate of inflation is required, not simply the rate of inflation.

The interest rate, which is included in the authors' rent equation should be transformed in the same way that rent is (i.e. percentage change). The authors do not indicate why they use the treasury bill rate rather than a (longer term) mortgage interest rate.

*The Apartment Starts equation*

A quite standard starts equation would include

supply variable:
- stock of rental units

investor demand variables:
- price of existing stock relative to price of new units (or, in more modern specifications, rent, and components of user cost (see above)
- change in weighted population as an indicator of change in demand for stock

The authors do include a short-run variable of importance, the vacancy rate. They also include real rent. Real rent might be regarded as a proxy for the rent-to-user-cost

ratio, but it is not a very good proxy; in their starts equation there is no role for interest rates, MURBs, the price of new units. There is also no role for the change in weighted population.

## II.3 *TECHNICAL COMMENTS*

1. As noted earlier, the omission of relevant variables does not matter for the purpose of the hypothesis testing only so long as the omitted relevant variables are not correlated with the critical variables $B^+$, $B^-$, $C^+$, $C^-$. Assuming that no such correlation exists is a strong assumption.

2. There is no need to use Intrumental Variable estimation instead of OLS to estimate the starts equation. OLS is a superior method because, contrary to the authors, the vacancy rate and rent are predetermined in this equation. Given that apartments typically take a year or more to build, why should apartment *starts* in 1980 affect rents and vacancy rates in 1980?

3. A problem with the large number of lagged variables in the specifications is that it increases multicollinearity. This does not matter, so long as (see point 1) these variables are not collinear with the critical variables. If these not-very-well-justified variables are collinear with $B^+$, $B^-$, $C^+$, $C^-$, however, their inclusion may increase the standard errors of the estimated coefficients and thus degrade test statistics, making rejecting null hypotheses very difficult. *In sum, the non-rejection of the null hypotheses might be the consequence of the inclusion of extraneous variables in the specifications,* because of the effect of this inclusion on standard errors.

## *II.4 DATA*

*General*

The authors have taken great care with some data. For example, in the nonhousing category, personal disposable income, they adjust personal disposable income for fluctuations in farm income. Some housing data, however, seems to have been uncritically accepted at face value--probably because the work required to investigate and adjust data is time-consuming, and impossible within the budget and the time frame for the study. In some cases, for example, in the case of rental starts, their choice of simplifying assumption seems to me to be the best one, but it would be worth spending the small amount of funds and time spent required to produce a better proxy.

*Vacancy rate*

The CMHC rental vacancy rate deteriorated *as an indicator of market looseness* over the period studied (Steele, 1993a, 1993b), especially in Ontario and British Columbia (Clayton and Associates et al, 1991). Account should be taken of this fact in the estimation.

*Conversions*

The conversion estimation method used by Statistics Canada, although possibly satisfactory to obtain estimates of stock, is one which does not yield very useful estimates of conversions. Statistics Canada simply assumes that conversions consists of official conversions (those for which building permits are issued). Many conversions of singles into double or triplex will not be covered because much conversion activity proceeds without the use of a permit.

It is not clear that the authors are aware of the basic source of the conversion data. They note without comment (p.12) that "series of conversions from multiple unit to single unit dwellings ...were not available," but building permit sources cannot *in principle* provide such series because permits are not required for such conversions (often called deconversions or mergers). It is also not clear that the authors understand the context in which Statistics Canada uses the data--the estimation of the stock. They note "the actual conversions series represent the numbers of units *after* conversions, less one in each case. (If a single unit is converted into three multiple units, the number of conversions is counted as two; why that is so is not clear to us, but that appears to be the way the calculation is made by Statistics Canada.)" (p. 37) The reason for this procedure on the part of SC is pretty certainly that SC wishes to add to its stock estimate only the *net* number of units created by conversion. If a single detached unit is converted into a triplex, three units are added to the multiple stock and one unit is lost from the single stock, and so the net addition to the stock as a result of conversions is two. SC's "conversions" series is *not* an estimate of the net addition to *multiples* because of conversions but instead the *net addition to total stock* because of conversions.

For the purpose of Denton et al, the deductions should be added back. Denton et al seem not to have done so. This will not matter much, for hypothesis testing purposes, *so long as the number of units added per conversion permit is the same over time and over provinces*. Otherwise it will matter. For example, if typically each conversion permit in Quebec represents a conversion in which a single unit becomes a triplex, while each conversion in B. C. represents a conversion in which a single

becomes a double, the SC series for Quebec will be 2/3 the true series while the SC for B.C. will be 1/2 the true series.[4]

Even when an appropriate method is used to estimate conversions, however, estimates are subject to very great error. There is no reason to believe errors will be iid.

The authors make the simplifying assumption that conversions to single from multiple is equivalent to conversions to owner-occupancy from rental occupancy. This assumption, for Ontario, is extraordinarily at variance with estimation I have done for Ontario (Steele, 1993a, Tables 3, 6). In Ontario over the 1976-1981 there was an estimated large loss to multiple units because of conversions but an estimated (very small) *gain* to the *rental* stock because of conversions.[5] How could this have happened? In substantial part because duplexes and row housing changed substantially from ownership to rental and because of the ongoing phenomenon that single detached housing tends to shift into rental tenure as it ages (see Steele, 1992, p. 109)

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[4] Of course there are other possibilities. Consider the following.

| Nature of Change to Structure | Implications for Housing Stock (Change in number of dwelling units) | |
|---|---|---|
| | Singles | Multiples |
| 1. Single detached changed to duplex | -1 | +2 |
| 2. Single detached changed to triplex | -1 | +3 |
| 3. Duplex changed to triplex | 0 | +1 |
| 4. Duplex changed to single detached | +1 | -2 |
| 5. Fourplex changed to duplex | 0 | -2 |
| 6. Single changed to lawyers' offices | -1 | 0 |
| 7. Single changed to halfway house | -1 | 0 |
| 8. Triplex changed to restaurant with two apartment units | 0 | -1 |
| 9. School house changed to house | +1 | 0 |
| 10. Warehouse changed to fiveplex | 0 | +5 |
| 11. Seasonal cottage winterized | +1 | 0 |
| 12. Addition of basement apartment to house, where owner-occupier continues to classify his dwelling as single detached (correct classification is duplex) | 0 | +1 |

[5] Note that the definition of conversion used in Steele (1993a) is more general than that used by Statistics Canada, and the estimation is fundamentally different in method.

*Rental starts*

The authors assume, because no published series exists, that private rental starts equal apartment starts. As they are aware, this means that their proxy for rental starts includes social housing starts and owner-occupier starts. If these two latter components are highly correlated with rental starts, as the authors expect them to be (p. 11), this problem will not matter.[6]

Unfortunately, they are not. For example, social housing starts as a ratio of apartment starts are far higher in the early 1980s than in the late 1980s. This will not matter for their hypothesis testing puposes, however, so long as a unit of social housing substitutes for a unit of private housing. Econometric estimates suggest this is not far from the truth for earlier periods, and its seems reasonable to suppose that substitutability would, if anything, be even greater recently, because of the dominance of nonprofit and co-op housing.

The inclusion of condominium starts in apartment starts seems likely to create few problems. HIFE data imply that the proportion of apartment starts which are condominium has little effect on the proportion actually rented.

In sum, for the authors' purposes, their assumption that private rental starts equals apartment starts is relatively appropriate. It would not be very expensive, however, to estimate a better proxy, and at the same time remodel the starts equation to include the number of social housing starts.

## *II SUGGESTED IMPROVEMENTS ARISING FROM ABOVE COMMENTS*

1. Report the variance inflation factor for the dummy variables of interest, so that the reader can assess the extent to which the non-rejection of hypotheses may be associated with collinearity of these dummies with extraneous variables in the specifications. Also, report the matrix of zero order correlation coefficient--and perhaps other collinearity diagnostics--for at least two of the more important equations (rent equation and apartment starts equations).

2. Redo the modelling, drawing on the housing literture. In particular,

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[6] More precisely, the authors expect that rental starts will be highly correlated with apartment starts. As noted earlier, if a social housing start results in a one-for-one reduction in private rental starts, the inclusion of social housing starts does not matter.

incorporate user cost variables, and use the weighted sum of population by age, instead of, simply, population. Omitted variables may bias estimates of the coefficients which are involved in hypothesis test, and accordingly, bias test statistics.

3. Rerun the apartment starts equation using OLS and report the results, or explain why it is believed that rents and vacancy rates in t are affected by new buildings which are not completed until (t+1).

4. Include the unemployment rate in specifications instead of, or in addition to, income. The unemployment rate is available by CMA while income is not. The authors' use of provincial income, only, means that income for Toronto vs Ottawa, and Vancouver vs Victoria, is not well captured.

5. Estimate rental starts by deducting social housing starts and using HIFE rental proportions for recently built stock, by type of unit to guide transformation of starts by type into rental starts.

6. Estimate equations involving vacancy rates for the period ending in 1986, as well as the entire period, because of the deterioration of vacancy rates as indicators of market looseness in Toronto and Vancouver in the late 1980s.

7. Make the suggested minor adjustment to the SC conversion series, or estimate conversions from scratch.

## *IV ALTERNATIVE WAYS TO ESTIMATE THE EFFECT OF RENT CONTROLS*

One way to measure the effects of rent controls would be to split up the stock, according to categories specified in legislation and then attempt to measure effects by category. For example, in Ontario, legislation treats buildings first occupied as rental prior to (about) 1976 differently from other stock. Hedonic rent regressions could be run on this stock and compared with other stock. See Gyourko and Linneman (1989) for a clever application of this idea for New York City.

There are major problems with this procedure however. It is clearly very difficult to distinguish between the effect of rent controls and age of dwelling, and between the effect of rent controls and the effect of length of tenure. The evidence in Fallis and Smith is flawed for this reason (see Steele and Miron, 1984, for further discussion).

Further, this procedure would not tell us anything about the *overall* effect of rent controls, but rather merely about the differential effects within a single market. For

example, rent controls might have no effect on the average market rent, but simply increase the rent of recently built dwellings relative to old buildings.

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# COMMENTS ON "TESTING HYPOTHESES ABOUT RENT CONTROLS"

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## 1. INTRODUCTION

These comments are provided pursuant to the contract with Canada Mortgage and Housing Corporation calling for a review of the study "Testing Hypotheses About Rent Controls" and suggestions for possible further work. It will be appreciated that we are in the somewhat odd position of being authors of the study we have been asked to review. With that in mind, we shall concentrate principally on how the study could be extended, or complemented by work along different lines. We shall make a number of suggestions, the most promising of which -- and the one to which we give the most attention -- being an experimental investigation that would take advantage of the recently created laboratory for experimental economics at McMaster University. First, though, some observations on the constraints under which the original study was carried out.

## 2. CONSTRAINTS ON THE ORIGINAL STUDY

There were three types of constraints: (1) the terms of reference set by CMHC; (2) time limitations; and (3) data limitations.

### CMHC Terms of Reference

The terms of reference were rather rigidly defined by CMHC in its request for proposals and the subsequent contract. They called for the formal statistical testing of hypotheses under seven headings. The specific statement of hypotheses, as set forth by CMHC, was as follows:

(1) Rents

Over the long run, rent regulations have no impact on rents. Rent regulations act to restrict rents from sharply increasing during periods of strong economic growth, but also inhibit sharp rent decreases in recessionary periods.

(2) Housing Starts

There are no significant differences in the responsiveness of rental supply with respect to vacancy rates and rents, and with respect to changes in vacancy rates and rents.

(3) Vacancy Rates

Rent regulations are associated with lower vacancy rates, other things equal.

(4) Property Values

Rent regulations decrease the relative attractiveness of investment in rental housing. This is reflected in the fact that percentage changes in rental property values are smaller in regulated markets than in unregulated markets.

(5) Tenure Preferences

Under rent regulations, the relative financial advantages of homeownership are lower. This increases the preference for renting.

(6) Conversions

Rent regulations encourage conversions from rental to owner-occupied housing (particularly condominiums).

(7) Maintenance and Repairs

Rent regulations discourage maintenance and repairs, and reduce the services (e.g., cleaning) landlords provide to tenants.

While we considered the formal testing of these hypotheses, as requested by CMHC, it immediately became evident that the available data restricted quite severely what could actually be done. Also, we found it necessary to adapt and convert the hypotheses into more precise forms in order to apply statistical testing procedures. In addition, we saw some opportunities for extending the range of hypotheses, and while some of the ones originally specified were not testable because of a lack of data the total number that we tested exceeded considerably the number originally proposed. Our reworked and extended versions of the original hypotheses are provided in Table 1 of the report and discussed in Section 3. The results of the tests (parametric and nonparametric) are set forth in Tables 13-19.

The point that we would emphasize here is that the framework for the project -- the general statements of hypotheses and the requirements for formal statistical testing -- originated with CMHC. There was thus only limited scope for influencing the methodology to be applied and the questions to be asked.

Time Limitations

All contract work has limited time for completion, of course; it could hardly be otherwise. However, it is worth noting that had there been more time for the one that we are discussing, it is quite possible that other model specifications could have been explored as a basis for the hypothesis testing. We took a "reduced form" approach, leaving in the background the concept of an underlying structural model. Had time permitted we might have attempted to construct an explicit structural model of the provincial and/or CMA housing markets. It is not clear that we would have been better off had we done so, but in any event the possibility of pursuing that line of investigation was eliminated by scheduling restrictions.

Data Limitations

The data limitations for a project of the kind that we carried out are really quite severe. We went to considerable effort to extend and make use of the time-series taxonomy of rent control regimes developed previously by one of us (Muller); the work in that regard is described in Section 5 of the report. We doubt that there is much scope for further improvement, although it should be noted that even within the categories identified in the taxonomy there can be considerable variation in practice in the ways in which rent controls are applied. Control regimes in different provinces, or in different years within a province, may appear to be essentially the same, but in fact may differ substantially through differences in procedure, enforcement, interpretation of regulations, and the like. However, a classification scheme is essential for statistical analysis, and that means that some of the variation must be ignored and a broad categorization imposed on the data.

The story is rather different with regard to some of the other types of data required for testing the CMHC hypotheses. Bearing in mind that to be useful the data must relate to different rent control regimes -- different provinces in different years -- it was not possible to find useful data for property values or tenure preferences. In the case of tenure preferences we were able to substitute tenure realizations -- proportions of households actually renting or owning their homes -- but (as we said in the report) that was clearly a second-best alternative to attitudinal data relating directly to preferences. With regard to "maintenance and repairs," we made use of data from the Statistics Canada Household Facilities and Equipment Survey on "housing in need of repairs." However, those data were available in a continuous form for only six years, starting in 1987, and their reliability is somewhat in question. With regard to conversions, we

were unable to find any series relating directly to the CMHC hypothesis about conversions of rental to owner units. The best that we could find were series of annual conversions from single-unit to multiple-unit dwellings, from Statistics Canada. There was very little in the way of documentation of sources and methods for those series, and we are rather uncertain about their quality; however, it appears that nothing else of use exists under the heading of conversions.

In short, then, the availability and quality of data represented a major constraint on what we were able to do in carrying out the rent control hypothesis testing project.

## 3. POSSIBLE FUTURE WORK

We discuss, in this section, some possibilities for future work to extend or complement what has already been done.

### An Experimental Investigation

This seems to us the most promising of the several possibilities that we shall note. We merely mention it at this point and then return to it later for a detailed discussion.

### Extension or Modification of the Previous Models

There are no doubt many ways in which the models that we used in our earlier study could be modified or extended, while remaining within the same general framework. The variables and lag structures used in what we regard as the reduced form equations for rental price increases, vacancy rates, tenure proportions, conversions, and proportions of units in need of major repairs could be reviewed in detail and alternative specifications tried out. The same is true of the equation for rental housing starts, which is not really a reduced form equation since

vacancy rates and rents, which enter as explanatory variables, cannot be regarded as exogenous variables. (To accommodate the endogeneity of those variables we used an instrumental variables method for estimating the starts equation.) We could certainly think up alternative specifications for those equations. However, we doubt very much that the results would turn out to be helpful. That is to say, we doubt that different models estimated using aggregated data would advance the cause of understanding the effects of rent controls beyond what our previous study was able to accomplish. Deficiencies of data, aggregation problems, de facto heterogeneity over both time and space in apparently uniform categories of controls, laxity of enforcement of controls and the availability of ways to circumvent them, differences in the effects of controls in different segments of the rental market -- all of these things make it difficult to discern the consequences of control regimes with clarity. What we tried to do was to make good use of the data that were available to us, and to employ what we believed to be reasonable models and procedures, to determine whether the available evidence weighed one way or another in the debate about the effects of rent controls. Our conclusion was that the available evidence did not indicate that there were significant effects. We doubt that that conclusion would be overturned by further work of the kind that we undertook using the same aggregate data that was available to us.

Development of a Comprehensive Structural Model

In principle, one might like to work with a comprehensive model of the structural kind -- one in which the actual workings of the rental housing market were identified in detail, as opposed to a reduced form model in which the relationships are essentially statistical and the underlying structural relationships exist only in the background. We considered the possibility of a

structural model but rejected it on the grounds that the scope of the project was too limited to permit the building of such a model, and that the aims of the project could be achieved by other means. One could certainly take up again the possibility of a structural model but we still doubt that that would be a fruitful direction to follow. Again, we doubt that there is much more to be achieved using aggregate data of the kind that are available.

Improvement of the Data Base

We drew attention in our report to the lack of or weakness of data for investigating the questions posed by CMHC. A recommendation that we can make with some enthusiasm is that a full-scale investigation be initiated into the adequacy of the existing data base and how improvements could be effected. If answers to the questions about rent controls are wanted quickly it is unlikely that such an investigation would be helpful; it is unlikely to bear fruit for a considerable time. Nevertheless, having worked on the rent control project and a number of others for CMHC over the years we are of the opinion that improvements in data would eventually enhance greatly the effectiveness of econometric studies of the housing market.

One example that is perhaps worth mentioning has to do with conversions. The only conversion series that were available to us related to conversions from single-unit to multiple-unit dwellings. We used those series but they were clearly inferior in nature to what were originally called for in CMHC's hypothesis about conversions from rental to owner-occupied housing (in particular, condominiums). Also, we had considerable doubt about the accuracy of the single-to-multiple series: there seemed to be anomalies in the series that would be hard to explain, the concepts were somewhat unclear, and there was no documentation of sources and methods available. Other examples of data deficiencies have to do with property values (no

series at all were available), state of repair (the Household Facilities and Equipment sample series are of short duration and the quality of the estimates is in some doubt), attitudinal data relating to preferences for rented vs. owned dwellings (not available), and housing prices. Some series that are available at the provincial level are not available at the Census Metropolitan Area level, and conversely. In short, there appears to be considerable scope for improving upon the existing data base so that studies of the kind that we carried out can be built on a firmer foundation of statistical information in the future.

Use of Micro-Data to Investigate Tenure Patterns

We used macro-data exclusively in our earlier study. A possibility for the future is to use micro-data to investigate the effects of rent controls on patterns of tenure -- on the probabilities of renting and owning. We have in mind the use of data from the Statistics Canada Family Expenditure Survey (FAMEX). FAMEX micro-data are available for a number of years extending back into the 1970s and include provincial identifiers, so that the data could be sorted by type of rent control regime (including the no-controls regime). It might then be possible to see whether the relationships between tenure probabilities and income, household composition, etc., were different under different regimes.

A model of this type could be estimated using probit or logit techniques and tests for structural differences among regimes could then be carried out. Ideally one would like to be able to identify demand-for-housing relations by this approach. However, observed tenure status is determined by the supply of rental and owner-type units as well as by the demand for such units. The model to be estimated would therefore probably have to be regarded as a reduced form model. Nevertheless, it would be of interest to see whether there was any evidence that

the model structure varied from one regime to another. (To the extent permitted by the data, supply-related macro-variables could be included in the micro-equations to allow for variations in the availability of rental and owner-type units.) Alternative approaches to the estimation of the model would include the pooling of data from different regimes, with dummy variables to represent types of regimes, and the fitting of the model separately for each regime.

Tenure status is undoubtedly highly serially correlated in the age dimension: a household that owns its home at age 35 (the age of the parents, let us say) is likely to own its home also at 40, 45, and so on. Thus, current tenure status may be the result in part of rent controls in previous periods, perhaps going back for a considerable length of time. To get closer to the impact of current or recent controls one might fit the model only to data for persons in the age range in which decisions to buy or not to buy are most common -- say the range from 30 to 40 years of age. A further possibility would be to restrict incomes to a range in which the financing of a purchase was a practical possibility. (That would require the setting of different ranges at different survey dates in order to allow for inflation; perhaps the range would be based on percentiles of the income distribution rather than dollar limits.)

The foregoing is intended mainly to be suggestive. Details for a micro-data investigation would have to be worked out with care.

## 4. LABORATORY EXPERIMENTS AND RENT CONTROL

Most econometric testing of hypotheses about rent control is limited by the nature of the field data available. These data are essentially the outcome of an uncontrolled, historical experiment, in which the extent and nature of rent controls are varied at the same time as

income, population, and other factors affecting rents. The independent effect of rent controls can only be determined if there is considerable independent variation in rent control across time and regions and if the extent of rent controls can be measured with little error. Such is not the case with most aggregate data available. This suggests that the techniques of controlled laboratory experimentation might allow a fuller understanding of how second generation rent controls of the kind found in Canada interact with the market institutions found in Canadian urban rental markets.

CMHC might wish to consider laboratory experimentation as a complement to econometric testing of field data. In this section we first provide a brief rationale for using experimental techniques. We then discuss how experimental techniques might be applied to the issue of rent controls. Finally, we point out that McMaster University, as the home of Canada's first Experimental Economics Laboratory, is well suited to conduct such experiments.

A Rationale for Experimental Economics

As a sub-discipline of economics, experimental economics attempts to test economic theories and institutions in a simplified environment fully under the control of an investigator. In a typical market experiment, subjects (sellers and buyers) exchange a fictitious commodity (a "token") which has value only because it can be purchased from or redeemed by the experimenter according to a given schedule of values.[1] Sellers earn laboratory profits by selling

[1] Most experiments are conducted with university students. Davis and Holt (1993, 17) note a frequently expressed concern that students may be unrepresentative of decision makers that would actually be found in the field. They cite some evidence suggesting that the behaviour of sophisticated decision makers "has typically not differed from that exhibited by more standard (and far less costly) subject pools". Nevertheless, when resources permit, sophisticated subject pools provide valuable controls on the experimental findings.

tokens at a price greater than the cost of acquiring them from the experimenter. Buyers earn laboratory profits by purchasing tokens at a price less than the price for which they can redeem them from the experimenter. At the end of the experiment, laboratory profits are converted to real dollars at a preannounced exchange rate and the subjects are paid in cash. The experimenter controls the rules under which trading occurs (the market "institution") and studies the effect of different institutions on the convergence of the market to equilibrium and the properties of the equilibrium, such as stability, efficiency and distribution of gains.

Experimental economics is a relatively new sub-discipline of economics, although Roth (1988, 974) points out that informal laboratory experimentation dates back at least to 1738 and that formal reports of laboratory experimentation have been appearing since 1938. Nevertheless, attention to experimental methods and results greatly accelerated during the 1980s. Useful reviews of the experimental literature may be found in Roth (1988) and Plott (1989) and in the recent text by Davis and Holt (1993).

The literature suggests that experimental techniques may be particularly useful in three ways: in screening and testing economic theories; in discovering new facts requiring explanation; and in designing and demonstrating new approaches to problems of public policy. The first point is perhaps obvious. Many theoretical explanations can be found for almost any economic phenomenon. If these theories are to explain market phenomena in general, they should explain behaviour in the highly simplified world of the laboratory. Experimentation can be used to screen out theories which simply do not work or which perform less well than alternatives (Plott, 1989, 1166). The second point is less obvious. Roth (1988) describes how experimentation has discovered that markets in which sellers post "take-it-or-leave-it" prices perform significantly

less well than some forms of auction market, how the payoff to other agents influences bargaining behaviour, how experience in prisoner's dilemma games first increases then decreases cooperation, and how an agent's willingness to pay for a commodity may be significantly less than the same agent's willingness to accept a payment in exchange for it. None of these results were fully anticipated by economic theory.

Experimental methods have been fruitfully applied to many policy questions. Plott (1987) reviews a number of approaches that have been used. These range from ex post analysis of the effect of voting agendas on final outcomes through testing proposed market institutions and demonstrating their value to people unimpressed with pure economic theory. For example, Plott (1987, 214) describes experiments in which a proposed method of auctioning airport landing rights was shown to have severe problems in communicating instructions and in computational complexity. In a related case, Plott (1987, 200-205) reports demonstrating the advantages of auction methods over committee allocations of aircraft rights to an audience of policy makers. In this case, the experimental results were not in doubt; rather the experimental sessions were used to demonstrated the nature of the proposed auction process in a particularly vivid way.

Applications of experimental economics to price controls

Our earlier study, in common with some other econometric studies of the effects of rent controls, was unable to detect a significant influence of Canadian rent controls on vacancy rates and rent levels. In fact Muller (1991) found some suggestion that rent controls were associated with higher rather than lower rent increases, although the effect was not statistically significant. One conjecture is that rent controls were simply not binding during most of the rent control episodes captured in the data. Another is that the guideline price increases characteristic of most

rent control regimes actually encouraged higher prices by providing a focal point which helped landlords coordinate rent increases that were higher than would otherwise occur.

Neither of these conjectures is based on proven theory that takes into account the institutional environment of rental market transactions. For example, the prediction that price controls above competitive equilibrium prices will not affect prices and trading volumes arises out of a simple model of competitive supply and demand in which both buyers and sellers are price takers. This theory is deficient in that it does not specify the process through which market prices are generated. For example, it does not consider whether prices are determined through bilateral bargaining, formalized auctions or sellers' posting of prices. Moreover, the theory is couched in terms of price levels whereas second generation price controls in Canada by and large control the rate of price increase rather than the price level.

The experimental literature has established beyond doubt that the market institution governing exchange can affect market outcomes. In particular, posted price markets have been shown to lead to slightly higher prices and somewhat lower economic efficiencies than are obtained in double auction markets. The actual rental market is better characterized as a mildly concentrated, posted price market with product differentiation than as a perfectly competitive homogeneous good market. Therefore we may reasonably expect that the predictions of competitive theory will need modification when applied to the field rental market. Unfortunately, very little work has examined how price controls interact with posted price markets and we have essentially no information on how controls on the rate of price increase actually affect posted price markets.

Previous Experiments

Early experiments by Isaac and Plott (1981) and by Smith and Williams (1981) indicate that non-binding price controls may influence the convergence of prices to competitive equilibrium in a double auction environment. In particular a non-binding price ceiling tends to depress the distribution of bids and offers in a double auction institution. These results have been replicated (Isaac, Ramey, and Williams, 1984). However, the double auction institution studied in these experiments was quite different from the field institutions in place in Canada over the past 20 years. In the Isaac-Plott and Smith-Williams papers the parameters were chosen to induce a competitive equilibrium price that was constant throughout the experimental session. The ceiling price was variously placed below, at and above the equilibrium price, and both offers and bids above the ceiling were rejected by the experimenter. In the field, rental prices are generally set by the seller, the competitive equilibrium price of rental housing has been rising continously due to inflation, and rent controls have been implemented as a ceiling on the rate of increase in posted prices. To our knowledge, no other experiments with price controls have been carried out.

What Could Be Learned from New Experiments?

A set of new experiments could be designed to test whether non-binding price controls influence prices upwards in a posted price environment. Experiments could also attempt to assess the influence of guideline rent increases on initial offers and rates of price increase in an inflationary environment. An appropriate strategy would proceed incrementally. First, the Smith-Williams or Isaac-Plott design would be replicated and its results compared with outcomes using the same cost parameters but a posted price market institution. Then the design would be

modified to allow for a continuously increasing competitive equilibrium price. During the first part of the experiment, subjects would trade in an uncontrolled environment; in a second phase, controls on the rate of price increase would be announced. In all cases, prices and rates of price increase would be compared across treatments to identify the effect of rent controls. Related experiments could investigate the hypothesis that actual or anticipated rent controls lead sellers to post higher initial prices.

Fundamentally, these experiments would be testing whether the negative effects predicted for rent controls in an abstract, perfectly competitive environment can be detected in a laboratory setting that more closely resembles the environment and trading institutions to be found in the field. It is particularly important to learn whether these negative effects predicted for rent controls emerge in a laboratory environment, for if they do not it is fruitless to expect to detect them in the field data.

Facilities at McMaster

McMaster University probably houses Canada's leading department in experimental economics, with at least five faculty members conducting active research programs in the area. Laboratory research in Economics at McMaster has now been greatly strengthened with the recent establishment of the McMaster Experimental Economics Laboratory.

The McMaster Experimental Economics Laboratory became operational in December 1993, and will officially open in March, 1994. Although there are similar facilities at universities in the United States and Europe, this is the first laboratory in Canada to be dedicated to computer-mediated interactive experiments in Economics and related disciplines. It consists of a network of 18 personal computers, each contained in an isolated carrel, and a Novell file server. The

laboratory is capable of running a wide range of experimental software and has aquired programming libraries to support its own development work.

The laboratory is located on the McMaster Campus. The initial costs of renovations and the aquisition of equipment have been financed by a grant from the Federal Government's Tri-Council Eco-Research Program under the Green Plan and by a donation from DuPont Canada. The Tri-Council Eco-Research funding was obtained by David Feeny, Stuart Mestelman and Andrew Muller to investigate economic instruments for achieving environmental objectives and to study alternative methods of managing common pool resources such as Hamilton Harbour. The Laboratory will also be available to graduate students who are studying market institutions and collective decision-making and who wish to use laboratory methods in their doctoral research.

The expertise of the McMaster Experimental Economics Group and the facilities of the McMaster Experimental Economic Laboratory are available to support a laboratory research program into the effects of price controls on rental markets, should CMHC wish to pursue this possibility.

## 5. CONCLUSION

We were asked to consider practical methods to improve, extend or complement our earlier study for CMHC. That study attempted to use available data to test systematically a variety of hypotheses about the effects of rent controls. Our conclusions were largely negative: the available evidence did not indicate that there were significant effects. The study was limited by the hypotheses specified by CMHC for testing, by time constraints, and by data limitations.

Given time, the approach taken in our study could certainly be extended, most notably by estimating a structural model of the rental housing market. We believe, however, that data limitations are by far the most severe obstacle to hypothesis testing in this area. If aggregate field data are to be used to test hypotheses about rent control, an improved database is the first prerequisite. Alternatively, micro-data from the Statistics Canada Family Expenditure Survey might prove a useful supplement to aggregate data in estimating the effect of rent control on tenure choice. Even then, the historical data may not provide enough independent variation in rent control regimes to assess effectively the quantitative impact of rent control on housing market variables. Accordingly, we suggest that CMHC might consider a program of laboratory research designed to improve our understanding of how second generation rent controls interact with the rental market institutions generally found in the field. Confirmation of the negative effects of rent control in a laboratory setting would strengthen the policy case against rent control even if the aggregate data are not precise enough for us to detect these negative effects in the field. If the hypothesized negative effects of rent control cannot be observed in a laboratory setting, then it seems fruitless to spend large sums to detect them in the field.

## 6. QUALIFICATIONS AND EXPERIENCE OF RESEARCHERS

This section provides a statement of the qualifications and experience of the authors, as requested by CMHC. Denton, Muller, Robb, and Spencer are faculty members of the McMaster Department of Economics, where they teach graduate and undergraduate courses in econometrics, statistics, economic theory, industrial organization, and other areas. All have had considerable experience in empirical studies relating to various aspects of housing in Canada.

Reports prepared in recent years for CMHC include "The Economics of Shelter Allowances," "Housing as Consumption and Investment," "ETHOS: Employment Through Home Ownership Stimulation," and "Study on Migration and the Urban Housing Market." Brief resumés follow; complete resumés are attached.

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