12-2005

An Evaluation of the Renaissance Zone Programs of Michigan

Yuanlei Zhu
Western Michigan University

AN EVALUATION OF THE RENAISSANCE ZONE PROGRAMS OF MICHIGAN

by

Yuanlei Zhu

A Dissertation
Submitted to the
Faculty of The Graduate College
in partial fulfillment of the
requirements for the
Degree of Doctor of Philosophy
Department of Economics
Dr. Wei-Chiao Huang, Adviser

Western Michigan University
Kalamazoo, Michigan
December 2005

AN EVALUATION OF THE RENAISSANCE ZONE PROGRAMS OF MICHIGAN

Yuanlei Zhu, Ph.D.

Western Michigan University, 2005

This dissertation analyzes whether the Renaissance Zone (RZ) programs in the state of Michigan are effective in helping distressed urban areas. The unique ES202 data permits us to use both establishment level data and aggregated zip level data to examine the impact of RZ programs on the establishment number and the firms' employment, real wage, and life duration. Based upon the presumption that different firms are sensitive to the tax incentives in different ways, this study examines the impact on all firms, on new firms, dead firms, and existing firms, on manufacturing and service firms, and on large and small firms.

Selection biases on observed and unobserved variables usually arise from compiling data to conduct program evaluation when comparing the business outcomes of zone areas due to RZ programs to those of non-zone areas or comparison areas without RZ programs. To correct for the observed selection bias and to also test if the findings are robust and consistent with different specification of the control groups, this study chooses two comparison groups, the 2nd round RZ and the propensity score picked group. To remove the possible unobserved selection bias, I apply three model specifications to the

estimation of employment and real wage effects on unbalanced panel data: (1) fixed effect model, (2) random growth rate model, and (3) lagged dependent variable model, and Difference-in-Difference tests to the estimation of duration effect.

I find that RZ programs cause fewer firms to start up, and fewer firms to close down in zone areas than those firms in non-zone areas at the same time. I also find that the Renaissance Zone programs raise employment by around 9% for service firms and around 3.8% for small firms. For manufacturing firms and large firms, the employment effect of RZ programs is not significant. Contrary to its mostly positive employment effect, the RZ program appears to cause the real wage to drop by 10.8% for manufacturing firms, by 11.7% for service firms, and 6.5% for small firms. The real wage effect is not significant for all firms taken together and for large firms. It is also found that employment and real wage effects change over time for manufacturing, service, large and small firms. Finally, based upon Cox proportional hazard function estimations and Difference-in-Difference tests, it appears that RZ programs do not help firms to last longer.

ACKNOWLEDGMENTS

This dissertation would never have been completed without help, support, and encouragement from so many great people from Western Michigan University, the W.E. Upjohn Institute and my family.

First of all, I would like to thank my committee: Dr. Timothy Bartik, Mr. George Erickeck, Dr. Matthew Higgins, and Dr. Wei-Chiao Huang, for their hard work and valuable comments. My advisor chair, Dr. Wei-Chiao Huang deserves special recognition for his guidance and patience throughout the project. I am also very grateful to the faculty and students of Economics Department at Western Michigan University. I benefit greatly from discussion with them and from a good academic atmosphere.

I have been fortunate to have the opportunity to work with all the wonderful colleagues at W.E. Upjohn Institute. Their help is invaluable to me and to my research. I am grateful to the W.E. Upjohn Institute for allowing me to use the data. I would like to give a very special thanks to Mr. George Erickcek. Working under him as a research assistant is the most rewarding professional experience I have ever had.

Finally, I would like to thank my wife, Huiying Yin, for her love, understanding, support and help. My last thanks will go to my lovely five-month old daughter, Alysa.

She has been a good sleeper so that I can be awake and refreshed at work every day (almost).

Yuanlei Zhu

TABLE OF CONTENTS

CHAPTER

CHAPTER

CHAPTER

CHAPTER

LIST OF TABLES

LIST OF FIGURES

"The role of government is to create conditions in which jobs are created, in which people can find work."---- George W. Bush

CHAPTER I

INTRODUCTION

1. Background

1.1 Economic Development Policy and Enterprise Zones

While metropolitan areas are expanding and growing, many inner metro areas are suffering from an array of problems including declining economic growth, job loss, disinvestment, income decline, high rates of unemployment and poverty, blighted industrial and commercial areas, and shrinking industrial and tax bases. To deal with this issue, many local governments took a variety of actions, generally called economic development programs, to stimulate local economic growth in those distressed areas. Economic development programs include the following elements: Direct grants or subsidies, financing tools, loans, tax credits or abatements, technical assistance, and training etc.

Of all the economic development programs, tax incentive is the most popular tool used by local, state and federal governments. Many of the tax incentive programs are called "Enterprise Zones." The concept of "Enterprise Zone," originated in England, was first introduced to the United States in the early 1980s as an economic development tool. Since then it has been adopted in more than 40 states. An enterprise zone is an

economically distressed area of a city designated by the local or federal government to offer a package of state and local incentives for a certain period. The incentives are designed to encourage business start-up, recruitment and expansion through state and local tax relief, local regulatory flexibility and infrastructure development. Some other elements such as grants, subsidies, or loans are used as complementary tools. The programs vary from state to state, zone to zone. In 1996, the state government of Michigan established the Renaissance Zone program, another name for Enterprise Zone programs.

1.2 Debate on Enterprise Zone Programs

Over the past 20 years, a substantial body of literature has emerged from studies on economic development policies and enterprise zones. Whether these programs work or not is one of the most controversial topics in public economics and existing studies still cannot reach consensus on this issue.

On the one hand, some analysts argue that tax incentives in enterprise zone programs are needed to create new jobs. Job growth may come from two kinds of firms: new firms and expanding firms. Theoretically, tax abatement can affect firms' behavior by reducing their business cost and increasing profits or returns to capital if all things are equal. According to the location theory, there are two reasons that a firm would be induced to locate in zone areas. One is that firms can pay less tax in a zone than outside of a zone. The other reason is that firms in a zone can pay lower wages than firms not in a

zone because workers in a zone area don't have to pay personal income tax and they have lower expected wages from firms. The Renaissance Zone program fits in here since residents in a renaissance zone don't pay state personal income tax and local income tax. For existing firms, low tax costs and high profits increase the competitive power, which lead firms to expand their business and hire more workers. Bartik (1991) reviewed these programs and suggests that state and local economic development policies have significant effects on local growth.

On the other hand, some analysts argue that this job growth might not be the case. Papke (1993) and Netzer (1997) point out that tax abatements that decrease the price of capital and labor can cause firms to substitute their demand for labor with capital. If the substitution effect is strong, tax abatement programs may even reduce employment. The incentive measure may have a significant effect on firms' behavior, but their effect on job creation is not apparent.

Thus, a comprehensive assessment of enterprise zone programs requires further examining; its efficacy in other dimensions such as other business activities, investment, and firms' life duration, and not just employment creation.

2. Michigan Renaissance Zone

Even though the debate on the efficacy of enterprise zone programs is still not settled, many state governments keep establishing new enterprise zones or expanding old enterprise zones for economic and political reasons. In particular, the Renaissance Zone

in the State of Michigan is viewed as the boldest economic development initiative among the various tax incentive measures.

2.1 What's a Renaissance Zone?

In 1996, the Michigan Legislative Council approved Public Act 376, cited as the "Michigan Renaissance Zone Act" *"to foster economic opportunities in this state; to facilitate economic development; to stimulate industrial, commercial, and residential improvements; to prevent physical and infrastructure deterioration of geographic areas in this state; to authorize expenditures; to provide exemptions and credits from certain taxes; to create certain obligations of this state and local government units; to require disclosure of certain transactions and gifts; to provide for appropriations; and to prescribe the powers and duties of certain state and local departments, agencies, and officials."*[1]

As a local economic development incentive, a Renaissance zone program is intended to improve the economic performance of distressed urban and rural areas by waiving most local and state taxes.

2.2 How are Renaissance Zones selected?

Basically, any local governmental unit like a city, village, or township satisfying the following key criteria can apply for Renaissance Zone designation: (a) Evidence of adverse economic and socio-economic conditions, such as high poverty rate, high

---

[1] See Michigan Renaissance Zone Act (1996).

unemployment rate and low income; (b) A creative, viable development plan; (c) Public and private commitment to the zones.

Members of the Renaissance Zone Review Board review all applications and based on the key criteria listed above, make recommendations to the board for approval of applications.

Six urban zones, three rural zones and two ex-military facilities out of 20 applications were selected as the first round Renaissance Zones by the Zone Review Board and became effective on January 1, 1997. Subsequently, on January 1, 2000, four urban zones, four rural zones and one ex-military facility were activated as the second round Renaissance Zones. The map distribution for both Renaissance Zone rounds is shown in Figure 1.1. Table 1.1 gives a brief summary of both 1st and 2nd round urban RZs.

2.3 What are the Benefits of a Renaissance Zone?

The taxes that businesses and residents are exempted from paying are: single business tax; state personal income tax; state education tax; local real/personal property tax; local income tax; and in Detroit only, utility user tax.

However, a business will continue to pay state sales tax (6%), social security tax, unemployment compensation, workers' compensation, sewer/water fees, and property taxes levied to finance local bonded indebtedness or special assessments.

Both 1st and 2nd round zones typically last 15 years. During the final three years, the taxpayers will pay 25% of their tax liability two years before the final year, 50% for one year before final year, and 75% for the last year.

2.4 What's the Difference between a Renaissance Zone and an Enterprise Zone?

Federal empowerment zones and enterprise zones target individual types of qualified businesses with special tax incentives, although they address the same urban problems as Renaissance Zones. Renaissance zone programs provide more drastic tax reductions in two aspects. One is that tax incentives apply to any businesses in the zone. Another is that residents in a zone can also get a tax rebate.

3. Literature Review on Policy Evaluation and Methodology

The most important question concerning the Enterprise Zone (EZ) or other economic development policies is if the programs work, or if the incentives contribute to local growth and job creation. A comprehensive evaluation of an economic development program can provide useful feedback to program managers and policy makers on how well this program is working and how it can be improved. Two strands of literature are reviewed here.

3.1 Literature on Evaluation of Relevant Economic Development Policy

Numerous studies have evaluated enterprise zones or other economic development programs. The overall findings are mixed; the effect of an enterprise zone

was found significant in some studies, but not significant, even negative in other studies. The evaluations vary in programs, methodologies, data, and variables considered.

Some studies find that enterprise zone programs are successful in job creation and economic growth theoretically and empirically. Ge (1995) showed that, in her two-areas-two-goods neoclassical model, an EZ creates jobs directly and indirectly, decreasing the rate of urban unemployment and increasing the agricultural wage rate. In his influential book, Bartik (1991) reviewed extensively the empirical literature on the effects of taxes and other factors on state and local economic growth. He concluded that a variety of state and local policies can make a difference to a local economy. He suggests that new wave programs should have a potential effect on growth. In chapter 3, Bartik (1991) provided a theoretical analysis of the effects of economic development policies on national and local income distribution. Following the theoretical analysis, he presented in detail empirical estimates of the effects of local growth on unemployment, housing prices, real wages and individual earnings, income distribution, and economic efficiency. Finally, he concluded that the empirical evidence implies that the nation can benefit from state and local economic development policies. Wilder and Rubin (1996) reviewed 21 empirical studies on two major aspects of EZ programs: their effects on jobs and investment, and their costs. Their review revealed that EZ programs were effective in stimulating employment and investment in certain areas, but were cost ineffective. The most recent study by O'Keefe (2004) used propensity score matching to pick the comparison census tract and

estimated the effect of California's enterprise zone. She found that an enterprise zone improved employment growth about 3% each year nearly six years after designation. She also found that firms in zones experienced more employment growth than firms not in zones. Papke (1994) found that businesses in the Indiana EZ permanently increased the value of inventories by 8%, but reduced the value of equipment by 13%. Moreover, the number of unemployment claims declined by 19% in the EZ. Also, after interviewing the business and officers in EZ, HUD (1986) concludes that the EZ exerted a positive and tangible impact on investment and job creations in 9 states and 10 zones. Finally, Sridhar (1996) and Rubin (1990) examined costs and benefits of EZ programs in Illinois and New Jersey, respectively and found that the benefits of the EZ program exceeded costs.

However, other studies found contradictory evidence on the effect of enterprise zones. Peters and Fishers (2002) used the hypothetical firm approach to evaluate the impact of enterprise zone programs on firm income or profit for 75 zones in 13 states. In brief, the hypothetical firm approach replicates a set of financial statements for real or "potentially" real firms by using the data from annual reports, federal tax statistics, and the Census of Manufacturers, and then applies the tax incentives to those firms. They developed TAIM (the Tax and Incentive Model) to measure the size of tax incentives. They found that the tax incentive is too small relative to other factors to influence firms' location decisions. In addition, Lambert and Coomes (2001) found that the Louisville EZ program in Kentucky was not effective for job growth and the residential estate values

declined in the EZ. Bondonio and Engberg (2000) examined EZ programs in California, Kentucky, New York, Pennsylvania, and Virginia. They found that EZ programs did not have a significant impact on local employment. Greenbaum and Engberg (1998) used the same data as in Bondonio and Engberg (2000) and found that EZ programs appeared to have little impact on employment, establishments, shipment, payroll, and capital spending outcomes. Engberg and Greenbaum (1999) also found that, on average, EZ programs in the United States didn't increase housing values. Along the same line, Dowall (1996), Boarnet and Bogart (1996), and Nissen (1991) found no positive effect of EZ programs on local economic development. In his dissertation on enterprise zone evaluations, Elvery (2004) found that the EZ programs of California and Florida have no impact on the employment of zone residents. Moore (2001) also didn't find a significant employment effect of EZ programs in his dissertation.

3.2 Literature on Policy Evaluation Methodology

Evaluation methods include survey, cost-benefit analysis, shift-share analysis, and econometric approach. It is common to use econometric models to test the effect of economic development policies. The standard approach to evaluating economic development policy is to compare the performance of a treatment group with a comparison group using either experimental or non-experimental method. The experimental method creates comparison groups through random assignment serving as a counterfactual, and is presumably free from selection bias. This method assumes that all

observable and unobservable characteristics of both the treatment and comparison groups are identical. If this assumption is not true, the estimation would be biased. The non-experimental method generates comparison groups resembling the treatment group in observed and hopefully unobserved characteristics. However, the usage of a non-experimental comparison group can generate biased estimates due to possible selectivity bias.

Due to the lack of ideal experimental data, only a few studies have been able to use the experimental method to evaluate social policies. By randomly assigning volunteers to treatment and control groups, Ham and LaLonde (1996) used experimental data to evaluate the impact of a government sponsored employment and training program, the National Supported Work Demonstration, and found that this program lengthened trainees' employment duration. LaLonde (1986) and Bratberg, Grasdal and Risa (2002) used both experimental and non-experimental data to evaluate the Norwegian rehabilitation project and concluded that the non-experimental method is unreliable.

Reliability problems aside, non-experimental evaluations of social policy are practical and generally used when experimental data are not available. In non-experimental evaluations, many statistical models such as matching, instrumental variables, and propensity score, are attempted to arrive at an unbiased estimate. In particular, the propensity score (the probability of assignment to treatment) methodology is, as advocated by Rosenbaum and Rubin (1983), Heckman and Smith (1996), and

Dehejia and Wahda (1998), a good solution to the problem of selection bias. This method controls for differences between the treatment and non-experimental comparison groups through estimated propensity score.

3.3 Lack of Evaluation on Renaissance Zone Programs

So far, few studies have examined the economic or social impact of the Renaissance Zones (RZ). The only published work on Renaissance Zone evaluation is by Sands (2003). He used survey data to evaluate the Renaissance Zone program. He summarized the survey results and found that the Renaissance Zone created 3,750 jobs and more than $367 million in private investment after three years of zone designation. Another work on RZ evaluation is the Report of the Citizens Research Council of Michigan (1998). This report evaluated the 1st round RZ program after the first year and a half of activation, and found that the RZ program had attracted 59 businesses, expansions and openings which brought in approximately 4,000 jobs to the zones, with an estimated total investment of $173 million. However, this positive effect of the RZ program is observed for a very short run and the report is just a summary of RZ economic activity.

These two evaluations assume that there are only positive results from the program without comparing the zone performance with non-zone performance. These inquiries are not complete since they only count jobs and money spent in the post-zone designation period. A continuous, ongoing examination of the economic and social changes that occur in the Renaissance Zones relative to what would have occurred

without the RZ designation is needed to acquire a meaningful evaluation of the effect of Renaissance Zones. This project aims to add to the literature by attempting a more comprehensive and rigorous evaluation.

3.4 Difficulties of Evaluations

The difficulties of evaluation come mainly from two sources: the program itself and data availability. Nearly all enterprise zones are not randomly assigned. They are selected according to economic and social economic background. The selection procedures can cause biased and inconsistent results if evaluations fail to control observed and unobserved factors influencing zone designation. On the other hand, many local authorities don't define the zone areas matched with city, zip code or census tract boundaries. This increases the degree of difficulty to select proper comparison groups. It is also very hard to find accurate data for both treatment and comparison groups. Many previous studies use the roughly matched zip code or census tract data for estimation. Those results might be affected by the potential spill-over effects. Only a few studies like O'Keefe (2004) use establishment data that can solve this problem.

A good dataset, which accurately reflects the performance of zones can increase the credit of evaluation. However, many performance measures like investment, value, housing, and output are not available for evaluation.

4. Evaluation Strategies of this Project

The best way of evaluating the Renaissance Zone is to find an ideal control group perfectly matched with zones in economic and social background and compare the performance of zone and comparison areas. This task can be easily achieved by experimental strategy if the zones are randomly selected. But in reality, nearly all economic development policies are designed for particular communities based on the economic and social economic performance. The zone areas are not randomly selected. Thus, the key part of program evaluation is to choose the comparison groups that match with zone areas as closely as possible.

This study undertakes two steps to examine the effect of renaissance zone programs. The first step determines the appropriate comparison groups. The second step estimates the effect of renaissance zone programs on employment and firms' duration.

In this research, two comparison groups are used. One comparison group is the second round Renaissance Zone since both the 1st round and 2nd round zones are based on the same selection criteria. The other comparison group is selected through propensity score, which is obtained from estimating a probit model of RZ selection with independent variables including poverty rate, average housing value, average income, and unemployment rate for zip codes.

The reason for choosing two comparison groups is for conducting sensitivity analysis. This allows me to test if the results are consistent for both comparison groups and how important the comparison group selections are in program evaluation.

The second step of this dissertation is to examine the RZ effect on four variables: employment, establishment number, real wage per worker, and duration of firms. These four variables will directly and indirectly reflect the impact of the Renaissance Zone programs. Employment growth is the major objective of local economic development policies. It is also a common outcome measure used in evaluating local economic development incentives.

I will follow Papke's (1994) fixed effect, random growth, and lagged dependent variable model specifications to estimate the employment and real wage effects of renaissance zone programs by using unbalanced panel data.

Analysis of firms' duration is interesting in two aspects. The first is that firms' life duration affects the employment stability and security of the regional labor market. The other is that firms' duration affects the future tax revenue buoyancy and recovery rate of local areas especially if the community has invested substantial funds in attracting these businesses.

The advantage of this research is that I can use firm level data instead of zone, zip code, or census tract data used in most previous studies. Firm level data can better reflect the behavior and decisions of firms since firms benefit directly from tax abatement of the RZ programs.

5. Data

Two datasets are used in this research. They are the ES202 (Michigan Unemployment Insurance Report) and the 2000 U.S. Census data.

5.1 ES202 Database

Each quarter the Michigan Employment Security Commission (MESC) Bureau of Research and Statistics assembles the data from quarterly tax reports filed by employers subject to the Michigan Unemployment Insurance (UI) laws and by Federal agencies subject to the Unemployment Compensation for Federal Employees (UCFE) program. MESC edits and processes the data and sends the information to the Bureau of Labor Statistics (BLS) as part of the Quarterly Census of Employment and Wages (ES202) program.

The ES202 database includes the following information for each active employer during the reported quarter: Employer identifier number (ID), Federal ID, 4-digit SIC code or 6-digit NAIC code, monthly employment during the quarter, total quarterly wages, establishment's name, physical/tax address, Zip code, city name, county codes. Predecessor and Successor numbers are often provided when an establishment's identifier number is changed. The predecessor number is the establishment's former ID number (used in the previous quarter), while the successor number is the establishment's new ID number that the establishment will use to be listed in the coming quarter. Both are used to link the establishments from one quarter to the next. Initial liability data,

inactive date, terminated date, reinstated date are used for recording the establishments' UI changes.

Since these data contain comprehensive employment and payroll information for nearly all employers in the state of Michigan, it is very useful for users to measure the job flows, evaluate the impact of development tools, and conduct special studies such as firms' survival rate and wages by industries. However, some records are not always accurate and consistent throughout time periods. To create a consistent historical series for each firm, I first corrected the errors on zip code since this research consistently uses zip code. The 2^{nd} step is to link the firms together through quarters.

5.2 Cleaning Database

In other applications of ES202, we have found the same establishment at the same address may have different zip codes in different quarters. The percentage of wrong zip codes is around 0.2% to 0.5%. To pick up those wrong zip codes and identify the right zip codes, we matched three consecutive quarters. If addresses match but zip codes do not, the zip code errors are detected. To correct those zip errors, if two zip codes are the same in three quarters, we take them as the right one and replace the wrong one. If any two zips are not the same, we use the addresses to locate the right zip code manually. After the correction, nearly all zip codes match with their address. In the three-quarter-matching zip case, we also corrected some typographical errors in addresses and IDs.

5.3 Linking Database

To measure job and establishment flows such as job creation, job destruction, and the life cycle of establishments, quarter-to-quarter matches are made using SAS. Only single establishment IDs are used and Corporate IDs are discarded since they exist at the same time. The matching procedure contains four continuous steps. The average matching percentage is presented in the following table.

	ID-ID Matching	Predecessor/Successor Matching	Shared Character Matching
Average % of records captured by matching	85.8	0.2	0.4

1. The first procedure matches the records by employer ID number (UI identifier). These are establishments that did not change their ownership, or reorganize their structures from one quarter to the next.

2. If there is no ID match, the next step is to match the records by their predecessor/successor numbers.

3. If a match is still not found, further matching is attempted by finding common characteristics in the establishment's address that are shared by establishments between two quarters. The first six letters of the address combining the ID with the last two digits omitted are used to link the establishments.

4. After the above three steps, we could identify the staying, opening, and closing establishments.

As a result of the above four matching steps, an average of 9,174 out of 229,742 records go to the "Death" category in each quarter, which is about 4.1% of average total observations, and 9,596 records go into the "Birth" category, which is about 4.3% of average total observations.

5.4 Applications of Database

We can use the constructed longitudinal ES202 database to conduct many economic analyses such as employment and establishment dynamics, firms' survival analysis, and the Renaissance Zone evaluation in the state of Michigan. For Renaissance Zone evaluation, I selected the firms in zone and comparison groups by firms' zip codes and addresses. The quarterly ES202 database used in this research covers the period from Q1:1990 to Q1:2004. Using ES202 establishment level data permits me to examine the firms' behavior in a more precise, time-consistent manner than using aggregate data like zone, zip, or census tract data.

5.5 2000 Census Data

2000 census data are used to pick matched pair comparison zip codes. For each five-digit zip code, six variables, namely, per capita income, poverty rate, unemployment rate, median housing value, house vacancy rate, and median rent, are used to explain the zone designation outcome according to zone selection criteria.

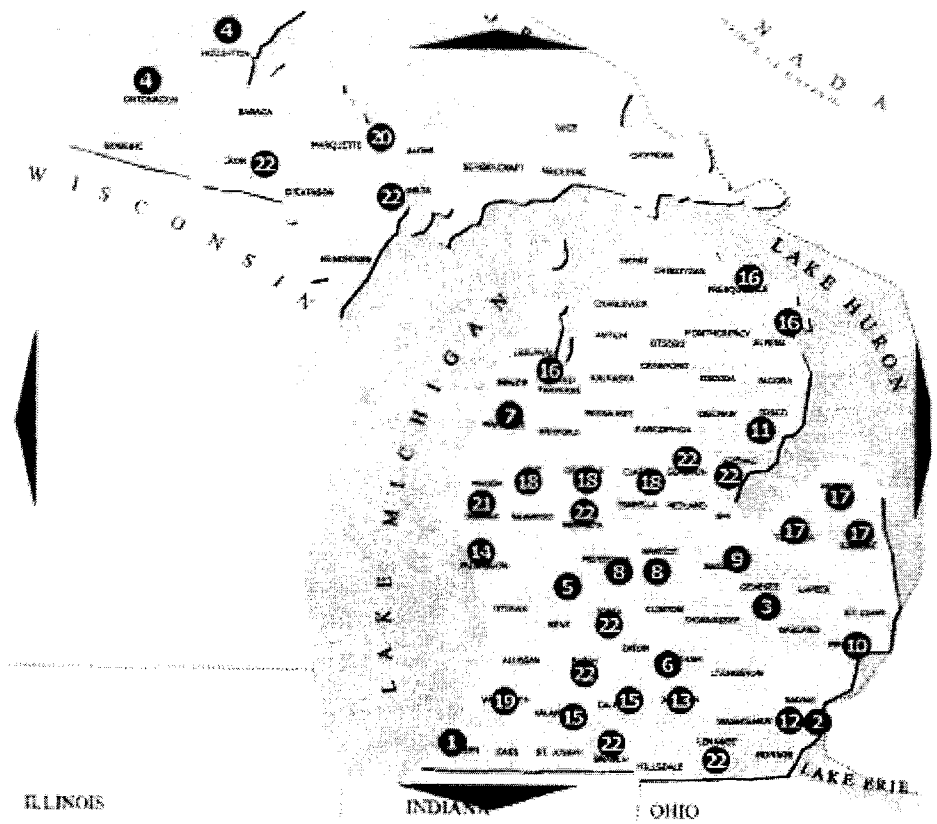
6. Overview and Organization of Dissertation

This dissertation investigates the impact of the Michigan Renaissance Zone program on employment and firms' longevity. A unique Michigan ES202 database allows this study to apply unbalanced panel data estimation techniques to examine firms' employment and duration change before and after zone designation. The next Chapter further discusses comparison group selection and presents some brief statistical analysis for all groups. This dissertation uses the same selection criteria and propensity score to match the observed characteristics between the comparison group and treatment group. Chapter III examines the employment, establishment and real wage effects of Renaissance Zone programs. To control for unobserved factors, Papke's (1994) fixed effect, random growth, and lagged dependent growth models are estimated. Chapter IV examines the impact of Renaissance Zone programs on firms' life duration. The difference-in-difference method is used to eliminate the effect of unobserved factors. Chapter V provides conclusions and policy implications.

Table1.1

1st and 2nd Round Urban Renaissance Zones

Zone	Round	Area (acres)	Start Date	Expire Date	No. of Subzone	No. of Census Tract
Benton Harbor/ St. Joseph	1	1771.97	1997	2011	10	5
Detroit	1	1433.45	1997	2008, 2011, 2012	9	7
Flint	1	1262.48	1997	2011	8	8
Grand Rapids	1	599.59	1997	2011	6	13
Jackson	2	297.24	2001	2014	5	5
Kalamazoo/ Battle Creek	2	254.4	2001	2015	10	10
Lansing	1	110	1997	2008	2	2
Muskegon/ Muskegon Heights	2	55.59	1997	2014, 2016	7	6
Saginaw	1	858.51	1997	2014, 2016	8	8
Wayne County	2	57.02	2001	2015	4	4
Total		6700.3			69	68



1 Benton Harbor /St. Joseph /Benton Twp.
2 Detroit
3 Flint
4 Gogebic /Ontonagon / Houghton Counties
5 Grand Rapids
6 Lansing
7 Manistee County
8 Montcalm/Gratiot Counties
9 Saginaw
10 Warren Tank Arsenal (former)
11 Wurtsmith Air Force Base (former)
12 Wayne County
13 City of Jackson and Jackson County
14 City of Muskegon and City of Muskegon Heights
15 Kalamazoo County, the City of Kalamazoo and the City of Battle Creek
16 Alpena County, the City of Alpena, Grand Traverse County and Presque Isle County
17 Huron, Sanilac and Tuscola Counties
18 Lake, Osceola and Clare Counties
19 Van Buren County
20 K.I. Sawyer Air Force Base
21 Agricultural Processing
22 Border to Border

Figure 1.1 Map of Michigan Renaissance Zones.

Source: Michigan Economic Development Corporation webpage.

CHAPTER II

THE SELECTION OF COMPARISON GROUPS AND BASIC STATISTICAL ANALYSIS

One key element for a non-experimental evaluation is the selection of comparison groups which are comparable to treatment groups in every other way except for not having received tax incentives. If the comparison groups differ in many ways from the treatment groups, the estimates of program impacts would be biased, generally referred to as "selection bias." To account for the difference in observed and unobserved characteristics between comparison and treatment groups, many techniques of matching and selecting comparison groups have been developed in the last 20 years. This chapter first discusses the selection bias problems and their solutions in general. The detailed procedure on how to select the comparison groups for Renaissance Zones is then reported. Lastly, a brief statistical analysis for three groups is provided.

1. The Problem of Selection Bias

Let Y_{it} be the employment of firm i in period t before zone designation, and let $d_i = 1$ if a firm is located in a zone, and 0 otherwise. Selection bias exits when

$$E\,(Y_{it} \mid d_i = 1) \neq E\,(Y_{it} \mid d_i = 0)$$

Suppose we estimate the following simple linear function:

$$Y_{it} = d_i\,\alpha + U_{it} \quad (t = 0, 1, \ldots.., T) \qquad (1)$$

α is used to ascertain the impact of zone programs. When $E\,(U_{it} \mid d_i) \neq 0$, selection bias is present. OLS estimates of α is inconsistent in this case.

We also assume that zone status Z_i is determined by X_i and V_i. X_i is a set of observed characteristics and V_i is a set of unobserved characteristics. If the relationship is in linear fashion, we can write:

$$Z_i = X_i \rho + V_i \qquad (2)$$

$d_i = 1$ if $Z_i > a$; otherwise $d_i = 0$; a is the threshold for zone designation.

If the dependence of U_{it} on d_i is due to the dependence of d_i on X_i, we call it a case of selection bias from observable variables. If the dependence of U_{it} and d_i is due to the dependence of d_i on V_i, we call it a case of selection bias from unobservable variables.

1.1 Solution to Selection Bias from Observable Variables

There are two ways to solve the selection problem on observable characteristics. One way is to add a set of observable variables (X_i) as the additional independent variables into equation (1) to remove the dependence between U_{it} and d_i. This method, termed the control function method, was proposed by Heckman and Robb (1985) and applied by Heckman and Hotz (1989) in their training program evaluations. The advantage of this method is the ease of interpretation. The limitation is that X_i should include all observable variables, which reduces the efficiency of estimation. Another constraint of this method is that the linear function form is imposed.

Another way to solve the selection problem on observable variables that was used in many previous studies is to select comparison groups by way of matching. A direct method of matching is to compare zone and non-zone areas with the same or similar

economic and social characteristics. This study applies this method and picks the 2nd round Renaissance Zone as the direct-matching comparison group because 1st round and 2nd round Renaissance Zone programs adopt the same selection criteria.

The propensity score method is another popular way to indirectly select comparison groups. The propensity score approach was first proposed by Rosenbaum and Rubin (1983), subsequently advocated by Dehejia and Wahda (1998), and applied by O'Keefe (2004) and Bondonio and Engberg (2000). This method assumes that observed variables affect the probability of zone designation. With all important observed variables considered, the predicted probability of zone participation for comparison groups should match with the predicted probability of zone participation for selected zones. The predicted probability is obtained from logit/probit modeling. In contrast to the linear relationship assumed in Heckman and Hotz's control function method, the propensity score approach assumes non-linear relationship.

1.2 Solution to Selection Bias from Unobservable Variables

Generally, econometric models are used to deal with selection problems arising from unobservable variables. Two econometric models proposed by Heckman and Hotz (1989), fixed-effect and random growth models, are widely used by evaluation analysts. These models are applied to panel data. Papke (1994), Boarnet and Bogart (1996), and Bondonio and Engberg (2000) have applied these two models in their enterprise zone

evaluations. This dissertation uses the above two models to obtain estimates for employment impact. The estimation details are presented in Chapter III.

Another established method to remove the selection on unobservable variables is the "Difference-in-Difference" method. This method examines the difference in outcomes before and after the zone programs come into effect across the treatment and control (comparison) groups. This method is used in Chapter IV for firms' life duration analysis.

Comparing the two methods mentioned above, Heckman and Hotz's two-panel data models are more complex than "Difference-in-Difference" estimating, but can engage detailed dynamic investigation into the time effects and unit-specific effects.

2. Comparison Groups Selected for Renaissance Zones

Previous analysts have used the following entities as comparison groups: (a) local units that applied for zone designation but were not accepted; (b) local units that qualified the selection criteria but didn't apply; or (c) sample units similar to those designated zones in terms of economic and social characteristics. Two different comparison groups are used here to estimate the impact of Renaissance Zones. They are the 2nd round Renaissance Zones as a direct-match comparison group and an alternative comparison group selected through propensity scores.

2.1 The 2nd Round Renaissance Zone as a Comparison Group

For this study, a natural comparison group for the 1st round Renaissance Zones is the 2nd round Renaissance Zones. Both rounds are based on the same selection criteria and selection procedures, which make them directly comparable in terms of economic and social background. In other words, we can say that the 2nd round Renaissance Zones are the counterfactual areas of the 1st round Renaissance Zones, but only received the tax abatement in later period.

Table 2.1 summarizes the economic and social economic conditions for both rounds of Renaissance Zones in 2000 census data.

The variables in the above table include most of the variables used as selection criteria for zone designation. As shown in this table, both 1st and 2nd round Renaissance Zones have higher unemployment and poverty rates, lower house value and income than the state average. This suggests that both areas are indeed economically distressed areas. However, the 2nd round Renaissance Zone group is performing slightly better than the 1st Renaissance Zones, even without tax benefits from Renaissance Zone programs.

According to the boundaries of 1st and 2nd round Renaissance Zones defined by Michigan Economic Development Corporation, there are a total of 1,815 firms in 1st round Renaissance Zones and 131 firms in 2nd round Renaissance Zones.

2.2 Comparison Groups Selected through Propensity Score

There are two steps involved in matching and choosing comparison groups using the propensity score method.

The first step is to estimate a probit model:

$$P = Pr(RZ{=}1 \mid X_i) = \text{probit}(X_i\,\beta) \qquad (3)$$

where X_i is a set of observed variables to determine the likelihood of zone designation. These variables include unemployment rate, median house value, median contract rent, vacancy rate, per capita income, median family/household income, and poverty rate.

This step uses 2000 census data for all the zip codes in the State of Michigan. Table 2.2 displays the estimation results from Equation (3). Standard errors are given in parentheses. Table 2.2 shows that unemployment rate, vacancy rate, poverty rate, and median household value as the observable characteristics of zip codes, can significantly determine the zone assignment.

Based on the probit estimation, 1st round Renaissance Zone zip codes are matched one-to-one with the remaining zip codes that have the same or closest predicted probabilities. Twenty-eight zip codes are selected as comparison groups. Corrected zip codes in ES202 database guarantee the accuracy of this selection. Table 2.3 shows the economic conditions of the 1st round Renaissance Zone and the matched pair zip codes comparison groups.

Compared to the 2nd round Renaissance Zones, the comparison groups selected by the propensity score method appear to be more similar to the 1st round Renaissance Zones in terms of social and economic conditions.

The second step in comparison selection is to pick up the firms in the same industry as firms in the 1st round Renaissance Zones under pair-matched zip codes. The comparison group so formed consists of 7,671 firms. There are 20 firms that are in both the 2nd round Renaissance Zone and the propensity score picked comparison group.

3. Brief Statistical Analysis of Three Groups

Before conducting econometric analysis, I performed some basic statistical analyses for three groups to give a casual picture of the RZ program effect. I examined the growth patterns of employment and number of establishments for three groups over the sample periods. This growth pattern analysis is for all firms taken together, new firms, dead firms and existing firms separately.

3.1 Growth Pattern Analysis for All Firms

The firms' average employment level in each quarter for both rounds of Renaissance Zones and the propensity score picked comparison group (hereafter referred as the comparison group in this chapter) is plotted in Figure 2.1. The two vertical lines represent the activation time points for 1st and 2nd round Renaissance Zones on Q1:1997 and Q1:2000 respectively. These two vertical lines appear in all the following figures. A cautious note is in order: Simply comparing the dynamic changes in average employment across the groups is not enough to tell if the programs really have an effect on the employment change.

Thus, I create an alternative index to measure the changes in employment or establishment number of three groups over time with the following formula:

Employment (Establishment) Growth Index =

$$100^* \frac{\text{Current year total employment (establishment number) in each group}}{\text{Base year total employment (establishment number) in same group}}$$

where the base year is 1990.

Figure 2.2 shows the total employment index for three groups over time. The Comparison line and Zone1 line start to diverge from the beginning. But, after Q1:1997, these two lines seem to be parallel for several years and start to converge after Q1:2001. On the other hand, Zone1 line and Zone2 line mix together from the beginning to Q3:1994 and then Zone2 line suddenly goes up and moves parallel above Zone1 line. At Q1:2001, these two lines move together and then diverge immediately. The Zone2 line has larger variations than the other two lines. Overall, there is no evidence that RZ programs change the employment growth patterns of the three groups.

I use the same method to plot the establishment growth index for three groups in Figure 2.3. Comparison line and Zone1 line diverge in the first three years and then move parallel together. At Q1:2000, these two lines start to converge. This indicates that the net growth rate of firm number in comparison groups declines more quickly than that in zone groups. The implication is that RZ programs help to either reduce the number of dead

firms or increase the number of new firms. However, comparing Zone1 with Zone2 lines don't show any discernable program effects.

3.2 Growth Pattern Analysis for New Firms, Dead Firms and Existing Firms

It is important to know where jobs are and if RZ programs have ever contributed to the creation of new jobs. New jobs may be gained either from new firms or from expanding firms. Lost jobs may be either from dead firms or from contracting firms. Examining the employment changes of new, dead and existing firms is significant for RZ program evaluations.

To know if there are changes in employment growth patterns for the three groups, another index is created with following formulas:

$$\text{Employment index for new firms} = 100 * \frac{\text{Employment of new firms in each group}}{\text{Base year employment in same group}}$$

$$\text{Employment index for dead firms} = 100 * \frac{\text{Employment of dead firms in each group}}{\text{Base year employment in same group}}$$

Employment index for existing firms=

$$100 * \frac{\text{Net employment growth of existing firms in each group}}{\text{Base year employment in same group}}$$

Employment growth indices of new firms for the three groups plotted in Figure 2.4 show that the employment growth patterns of new firms for the three groups don't act differently before and after the 1^{st} round and 2^{nd} round zone designation. This is also the

case for dead firms and existing firms shown in Figure 2.5 and Figure 2.6 respectively. Thus, we can't tell by simply examining their indices if the RZ programs cause the employment of the three groups to change in speed for new firms, dead firms and existing firms.

In addition to employment growth analysis, it is also important to examine if the number of total new firms is larger than the number of total dead firms and if the growth of new establishment number or the growth of dead establishment number is different for the three groups. The relevant index is calculated as follows:

$$\text{Establishment index for new firms}=100^{*}\ \frac{\text{Number of new firms in each group}}{\text{Base year total number of firms in same group}}$$

$$\text{Establishment index for dead firms}=100^{*}\ \frac{\text{Number of dead firms in each group}}{\text{Base year total number of firms in same group}}$$

Both Figure 2.7 and Figure 2.8 show that the growth of establishment number for either new firms or dead firms in three groups does not present any RZ program effect.

In summary, it is not obvious from mere simple visual inspection of index changes that the growth of employment and establishment number for new firms, dead firms and existing firms in the three groups is acting differently due to RZ designation. Thus, a more rigorous econometric analysis on new firms, dead firms and existing firms in the following chapter is necessary.

Table 2.1

Economic Conditions of 1st and 2nd Round Renaissance Zones, by Zip Code

Variables	1st Round	2nd Round	Difference	State Average
Unemployment Rate	0.11	0.08	0.04	0.06
Median House Value ($)	66464.00	76925.00	-10461.00	104692.52
Median Contract Rent ($)	404.50	423.00	-18.50	409.8
Residential Vacancy Rate	14.61	15.58	-0.97	29.19
Per Capita Income ($)	16294.82	17647.33	-1352.51	20397.76
Median Family Income ($)	37543.43	42300.00	-4756.57	49602.08
Median Household Income ($)	31787.39	34943.58	-3156.19	42868.96
Poverty Rate	0.23	0.16	0.07	0.09
No. of Zip Codes	28	12		

Table 2.2

Probit Regression Results

Variable	Coefficient
Intercept	-12.4146
	(18.3410)
Unemployment Rate	1.8091*
	(0.7475)
Residential Vacancy Rate	2.1475*
	(0.6166)
Poverty Rate	-2.8916*
	(0.8986)
Median House Value (log)	3.6871*
	(1.2038)
Median Contract Rent (log)	-1.7278
	(1.9234)
Per Capita Income (log)	-0.9699
	(1.9077)
Median Houshold Income (log)	-1.2755
	(1.9248)
Number of Obs	924
Log Likelihood	-73.78

* P-value 0.05

Table 2.3

Economic Conditions of 1st Round Renaissance Zone
and Comparison Zip Codes

Variables	1st Round	Comparison	Difference	State Average
Unemployment Rate	0.11	0.1	0.01	0.06
Median House Value ($)	66464	70350	-3886	104692.52
Median Contract Rent ($)	404.5	408.89	-4.39	409.8
Vacant Rate	14.61	13.68	0.93	29.19
Per Capita Income ($)	16294.82	16080.39	214.43	20397.76
Median Family Income ($)	37543.43	37793.89	-250.46	49602.08
Median Household Income ($)	31787.39	32050.46	-263.07	42868.96
Poverty Rate	0.23	0.22	0.01	0.09
No. of Zip Codes	28	28		

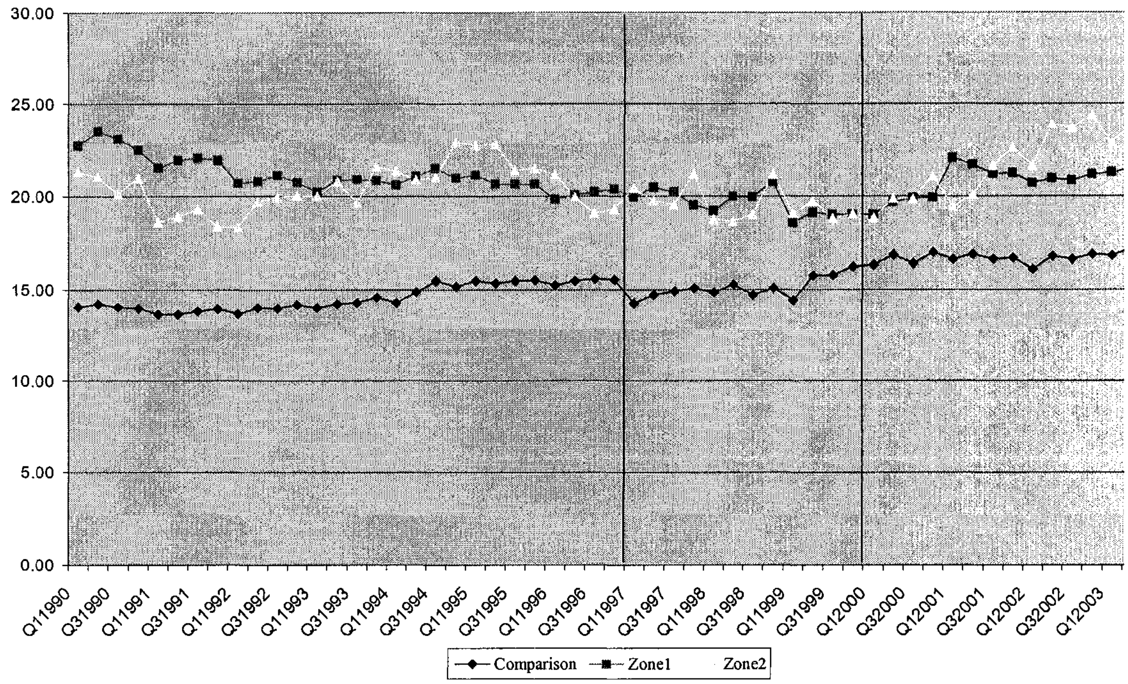


Figure 2.1 Average Employment over Sample Period for Three Groups.

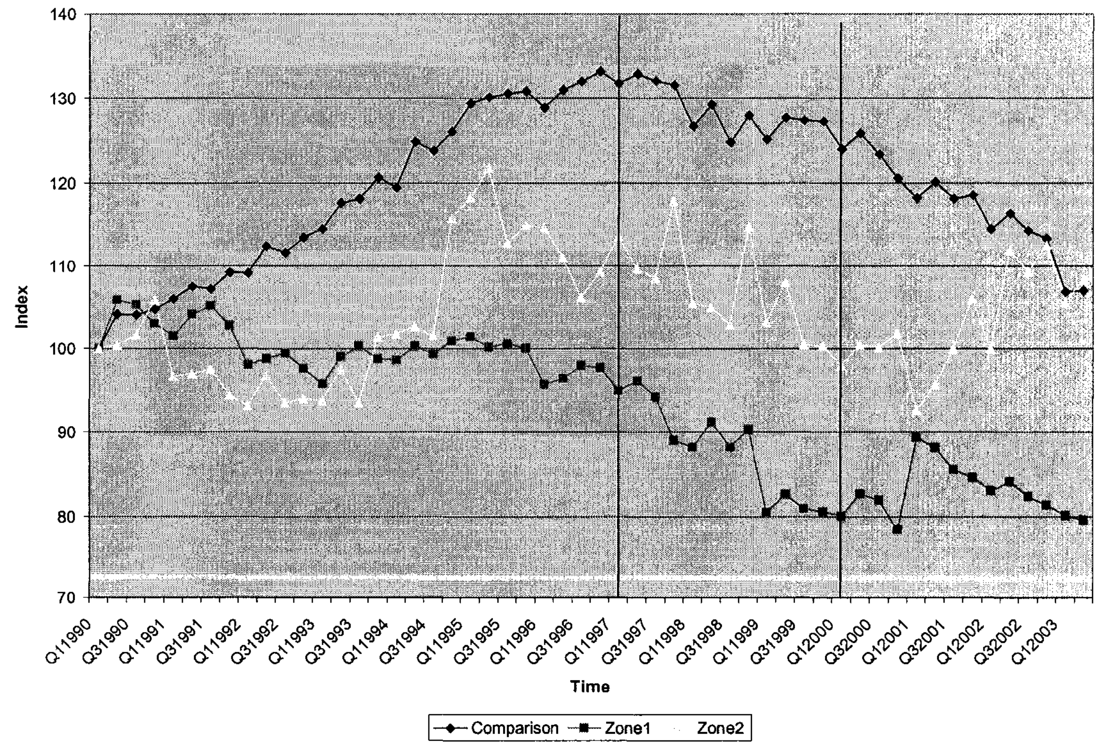


Figure 2.2 Total Employment Index over Sample Period for Three Groups.

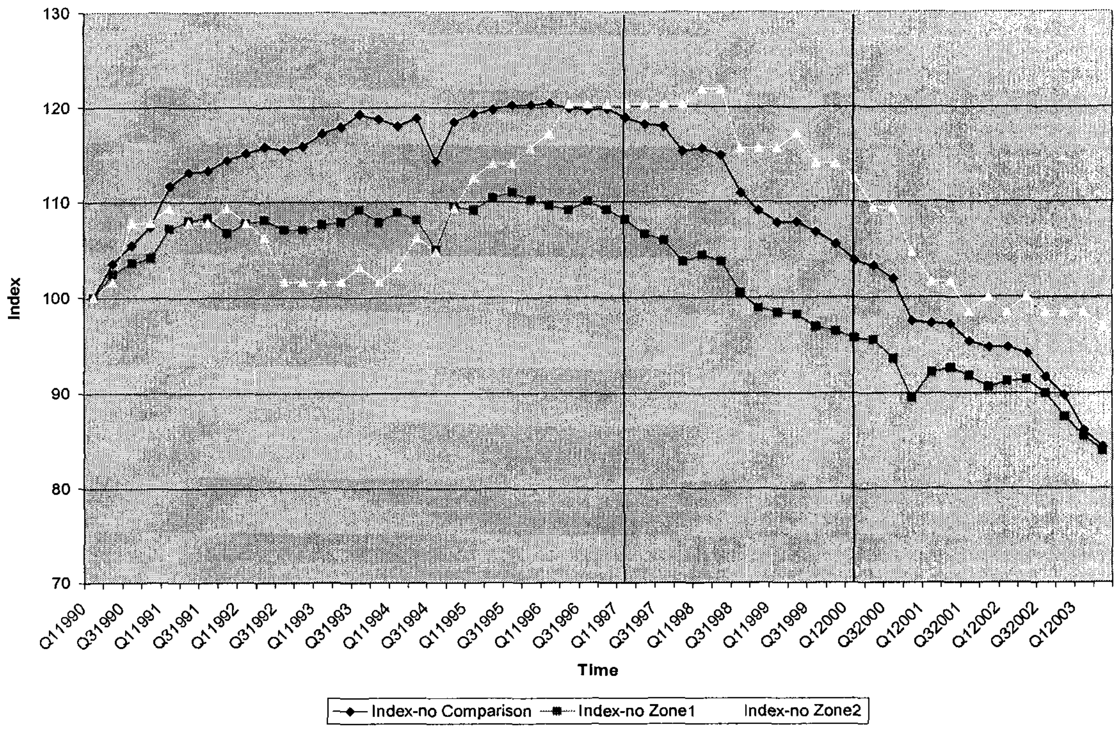


Figure 2.3 Total Establishment Index over Sample Period for Three Groups.

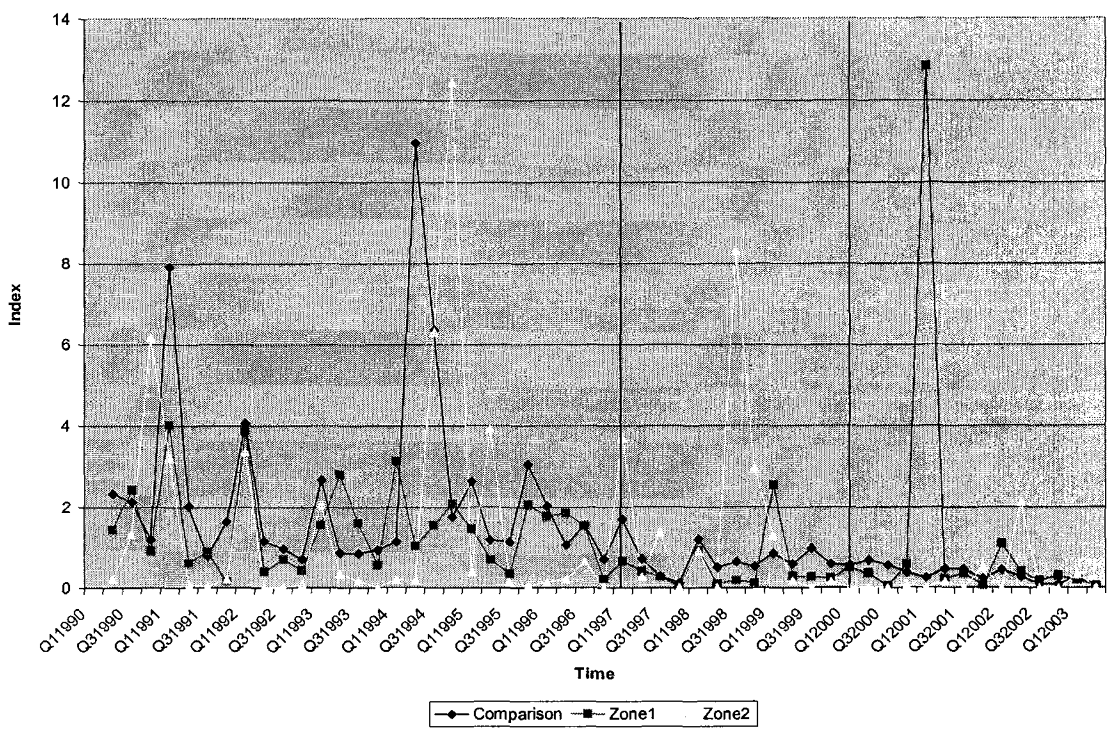


Figure 2.4 Employment Growth Index of New Firms over Sample Period for Three Groups.

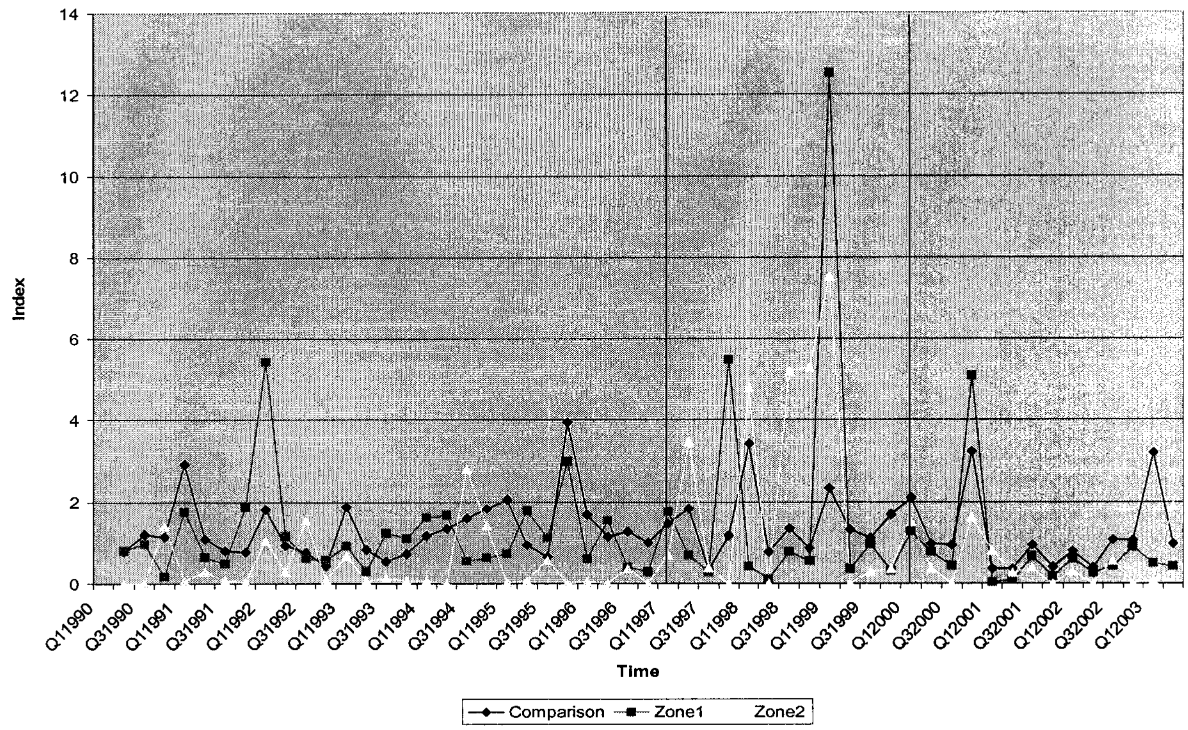


Figure 2.5 Employment Growth Index of Dead Firms over Sample Period for Three Groups.

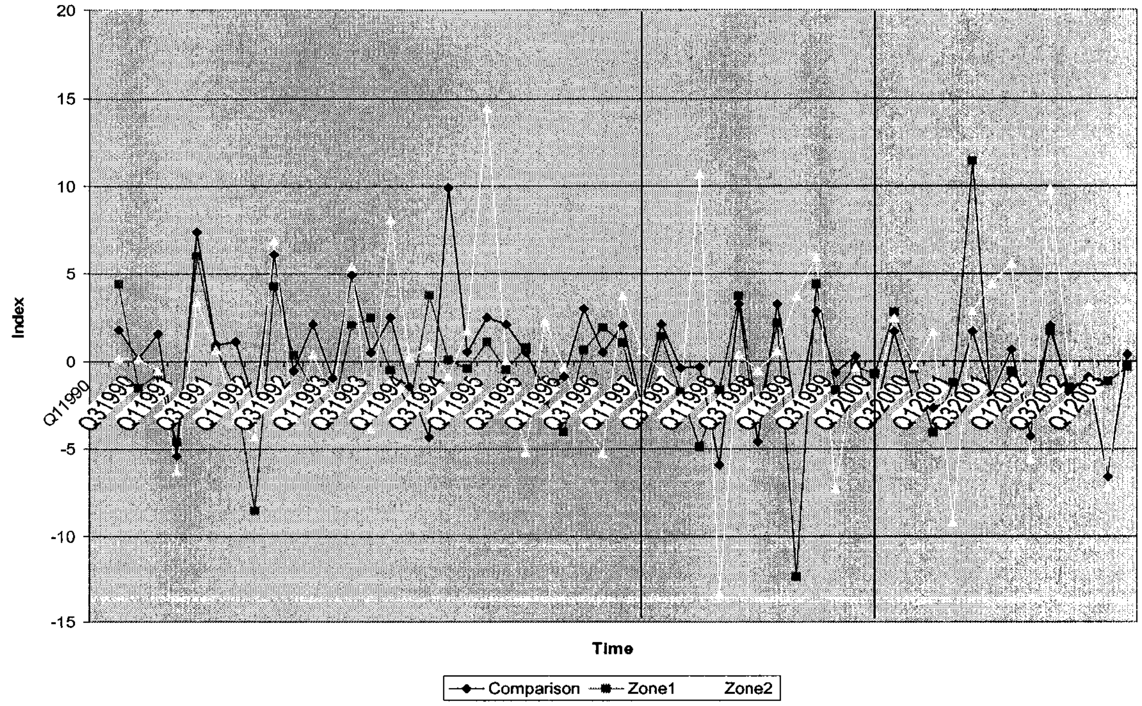


Figure 2.6 Employment Growth Index of Existing Firms over Sample Period for Three Groups.

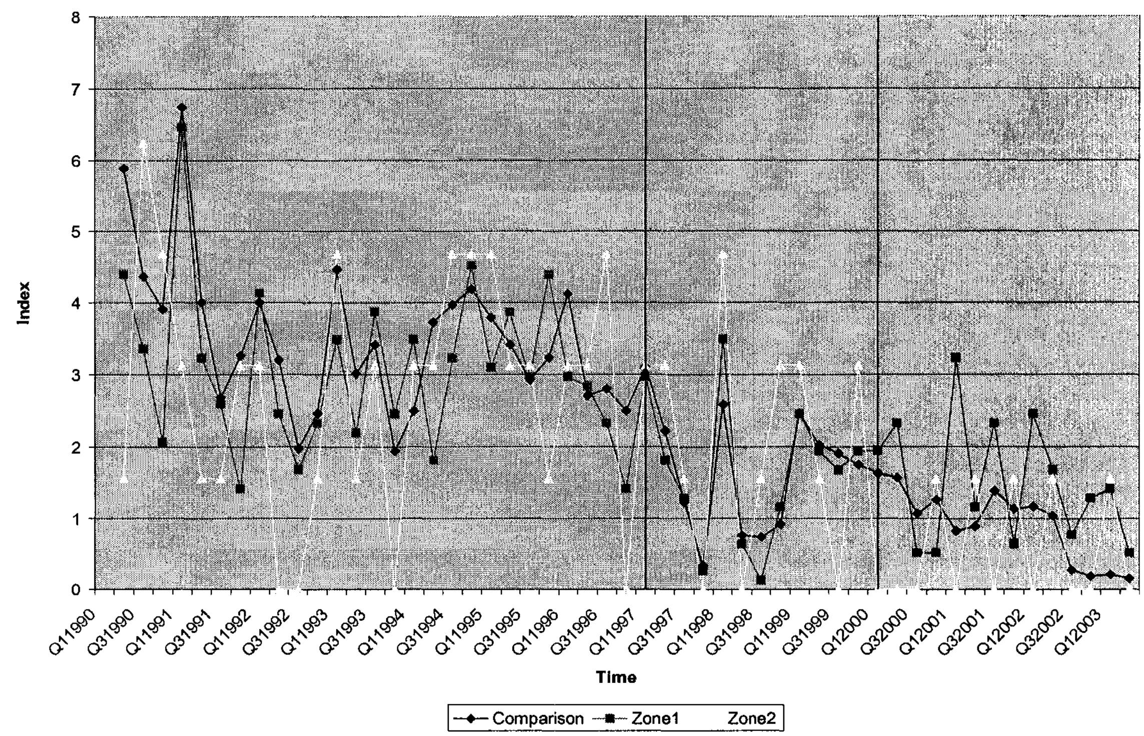


Figure 2.7 Establishment Number Growth Index of New Firms over Sample Period for Three Groups.

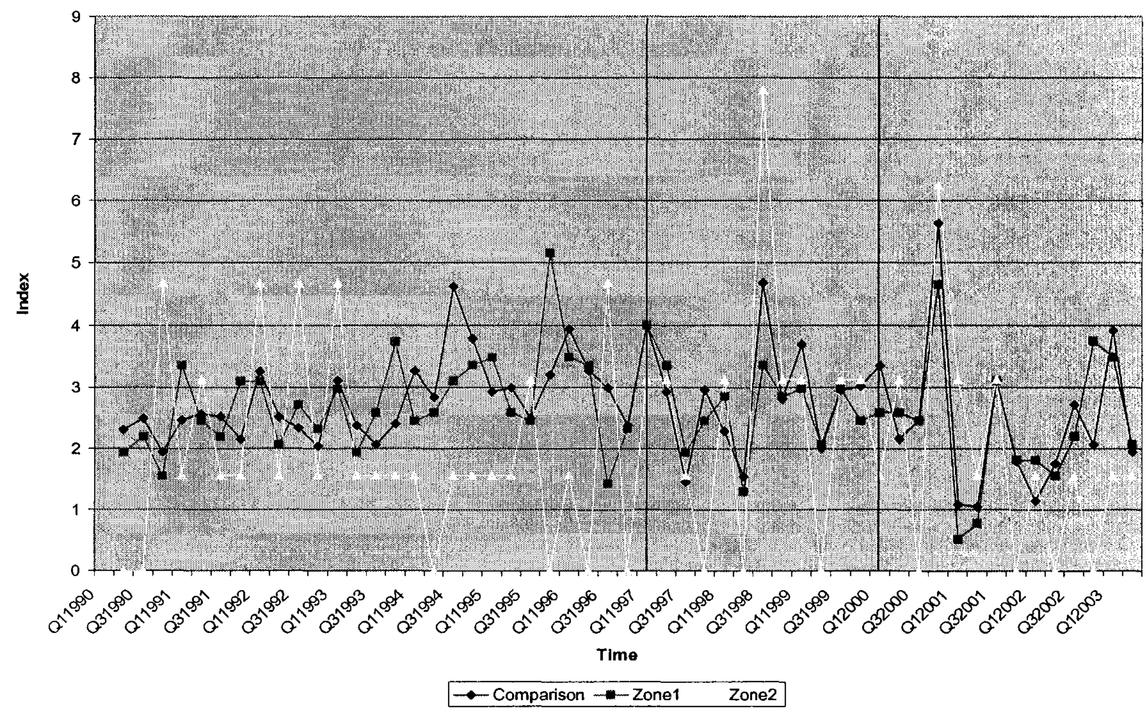


Figure 2.8 Establishment Number Growth Index of Dead Firms over Sample Period for Three Groups.

CHAPTER III

THE EMPLOYMENT, ESTABLISHMENT, AND REAL WAGE EFFECTS OF THE RENAISSANCE ZONE

1. Introduction

The ideal policy outcomes of Renaissance Zone programs are increases in investment, employment, and housing values, improvements in business and residential environments, and lowered poverty rates. As a result of implementing Renaissance Zone programs, the distressed zone areas are expected to develop and advance to the same as or higher level than other similar areas. Eventually, local governments could collect more taxes to compensate for earlier tax concessions and reap political benefits and support.

To the public, the direct and apparent outcome of the zone program is improvement of the employment situation. Job creation is the central goal of a Renaissance Zone. Thus, employment is widely used in previous evaluations of enterprise zones as the most important outcome measurement of local economic development policies.

1.1 How Does the Renaissance Zone Create Jobs?

Employment growth is not the direct and immediate outcome of zone programs. What comes first is the reduction in business costs owing to tax incentives of the zone programs. Lower business cost not only attracts firms to relocate into the zone or start new branches in the zone, but also helps existing firms to expand. In the case of Michigan's Renaissance Zone program, firms that relocate within the state from a non-

renaissance zone community to a renaissance zone for tax incentive purposes, and that leave without the blessing of the abandoned local community, are prevented from receiving tax benefit. So "new" firms in zones either come from outside of Michigan or are new branches of existing firms in Michigan.

As a result of lowered cost, new and existing firms tend to produce more output, and output expansion usually means that firms need to employ more labor, and probably require more capital than before. This job creation effect of zone program is called the output effect.

Zone programs also entail another effect, the substitution effect, which may lead to job gains or losses depending on the incentive structure of the program. If the program's tax incentive structure is biased toward capital, firms would use more capital to substitute for labor since capital becomes cheaper. This provides a theoretical explanation for those studies that have found the employment effect of economic development policies to be non-significant.

On the other hand, if the program's tax incentive structure is biased toward labor, firms would use more labor than capital since labor becomes cheaper. In this case, the substitution effect reinforces the output effect and zone programs create more jobs unambiguously.

With respect to the substitution effect in Michigan's Renaissance Zone program, on the one hand, single business tax, state education tax, and local real estate/personal

property tax are business-based taxes. However, there is no way to distinguish tax on labor from tax on capital for businesses. On the other hand, State personal income tax, local real property tax, and local income tax are resident-based taxes. These tax abatements seem like labor subsidies. Thus, the Renaissance Zone programs are likely to subsidize both labor and capital more or less equally, making the substitution effect insignificant.

One other reason that the employment effect of the zone program may be small is, as pointed out by Papke (1993), because most empirical evidence suggests that the supply of labor is inelastic.

1.2 Why is Wage Effect Also Important to Consider?

Although Papke (1993) also points out that tax incentives on labor or capital should have a larger impact on wages than on employment, the wage effect of the zone program has received little attention in program evaluation studies.

In general, the implementation of a zone program will increase the price of immobile factors of production. Bartik (1990) mentioned that labor is widely accepted as an immobile factor. Thus, the prices of labor, capital and land should increase after the zone incentives are introduced. Indeed, Papke (1993) found that in resident-targeted programs, the estimated increase in zone wage ranges from 2.5 to 5.2 percent.

Thus, both the employment and real wage effects of Renaissance Zone programs will be examined here to better understand the impact of the zone programs on the price and usage of labor.

1.3 Employment Effect for New Firms, Dead Firms and Existing Firms

As mentioned above, RZ programs may create jobs through new firms starting up and existing firms expanding. On the other hand, it is also true that zone areas lose jobs from dead firms exiting and existing firms contracting. Thus, evaluation of the net employment effect should consider new firms, dead firms, and existing firms, as well as all firms taken together. This is because firms in different developing stages have different cost structures and may respond differently to the tax incentives.

This study uses aggregated zip level data to examine the employment effect of RZ programs for new firms, dead firms and existing firms separately to see if RZ programs gain jobs through creating jobs from new firms, or through saving jobs from dead firms or existing firms. Using a similar method, this study also examines the number of establishment effects of RZ programs for new firms and dead firms. This is as important and as interesting as employment effects since the number of establishments is an important indicator of the success of local economic development policy.

1.4 Employment and Wage Effects for Manufacturing and Service Industries

Renaissance Zone programs may have different employment and wage effects on different industries. For one thing, manufacturing firms are more capital intensive than

service firms. Conceivably, the output and substitution effects of the tax incentives can then be different between manufacturing and service firms.

In addition to examining the overall effect of Renaissance Zone programs, this study also examines the effect of Renaissance Zone programs on manufacturing and service industries separately. The same method is used to examine the program effects in separate industries as is in overall industries.

1.5 Employment and Wage Effects for Large and Small Firms

The Renaissance Zone program may also have different employment and wage effects on large and small firms. The tax incentive of the RZ program is commonly perceived to have a larger impact on small firms because the marginal benefit of tax abatement is larger for small firms than for large firms. In other words, the output effect could be larger for small firms. Actually, many small business assistance/subsidies are often combined with the tax incentives in the implementation of enterprise zone programs.

Therefore, this study also attempts to test the hypothesis that the RZ programs have a larger impact on small firms than large firms by using the same method as is used in the evaluation of manufacturing and service firms.

It is hoped that examining the RZ program effects in different industries and different firm sizes can help us gain a deeper understanding of the zone program's business outcomes.

2. Model and Data

Three model specifications are used in this chapter to control for zone selection problems from unobservable variables by using a panel of zones and non-zones both before and after zone designation. Fixed-effect and random growth models are first proposed by Heckman and Hotz (1989) and applied by Papke (1994), Boarnet and Bogart (1996), and Bondonio and Engberg (2000) in their enterprise zone evaluations. I also apply Papke's (1994) lagged dependent variable specification in the estimations.

2.1 Fixed Effect Model

Following Heckman and Hotz (1989), we assume that: $U_{it} = \phi_{Ii} + \mu_{it}$, where U_{it} is defined in Chapter II, and ϕ_{Ii} is the firm-specific fixed effect to account for the dependence of U_{it} on V_i. The fixed effect model can be written in the following linear regression form:

$$\log y_{it} = \alpha_i + \beta t + \delta RZ_{it} + u_{it}$$

where RZ_{it} is 1 for firm i in Renaissance Zone at time t, and 0 otherwise; y_{it} is either the employment or real wage; α_i is the fixed effect for each individual firm i. The consistent estimator can be obtained from OLS estimation of the first difference equation below, whether u_{it} are white noise errors correlated with zone designation or not.

$$\Delta \log y_{it} = \beta + \delta \Delta RZ_{it} + \Delta u_{it}$$

The coefficient of the RZ dummy variable measures the percentage change in employment or real wage due to the zone designation.

2.2 Random Growth Rates Model

In a more general case, we assume $U_{it} = \phi_{1i} + t\phi_{2i} + \mu_{it}$, where ϕ_{2i} is a firm-specific growth rate. Following Chapter II, the dependence of U_{it} on d_i is due to the dependence of d_i on (ϕ_{1i}, ϕ_{2i}). The random growth model also can be expressed in the following linear regression form:

$$\log y_{it} = \alpha_i + \beta_{1i}\, t + \beta_{2t} + \delta RZ_{it} + u_{it}$$

This model allows zone selection to be based not only on the fixed effect α_i, but also on the growth rates β_{1i} for each firm i as well. β_{2t} represents the aggregate time effects. After first differencing the regression equation becomes:

$$\Delta\log y_{it} = \beta_{1i} + \Delta\beta_{2t} + \delta\Delta RZ_{it} + \Delta u_{it}$$

This equation is estimated by standard fixed effect. The resulting estimator of δ is consistent under standard conditions.

From the assumptions on U_{it}, it is easy to tell that above two models are nested. Fixed effect model is a special case of the random growth rate model.

2.3 Lagged Dependent Variable Model

In Papke (1994), the lagged value of the dependent variable replaces the random growth rate term to account for the possibility that zone designation is based on the lagged dependent variable.

$$\log y_{it} = \alpha_i + \beta_{2t} + \rho \log y_{i,t-1} + \delta RZ_{it} + u_{it}$$

After the first difference the estimation equation becomes,

$$\Delta\log y_{it} = \Delta\beta_{2t} + \rho\, \Delta \log y_{i,t-1} + \delta\Delta RZ_{it} + \Delta u_{it}$$

This equation is estimated by instrumental variables, using $\Delta\log y_{i,t-2}$ as an instrument for $\Delta\log y_{i,t-1}$ since $\Delta\log y_{i,t-1}$ and Δu_{it} are correlated.

In the above three specifications, the use of one single dummy variable, *RZ*, assumes that the Renaissance Zone programs cause a permanent shift of employment or wage in zone areas. This could be a restrictive specification if the influence of the zone program changes over time. A more general specification can be written as:

$$\log y_{it} = \alpha_i + \beta t + \delta_1 RZY1_{it} + \delta_2 RZY2_{it} + \dots + \delta_7 RZY7_{it} + u_{it}$$

where $RZY1_{it}$ =1 if firm i has been in a zone for one year, and 0 otherwise; $RZY2_{it}$ =1 if firm i has been in a zone for two years, and 0 otherwise; similarly for $RZY3_{it}$, $RZY4_{it}$, $RZY5_{it}$, $RZY6_{it}$, and $RZY7_{it}$.

2.4 Intra Group Correlation Problems

In the above three models, dependent variables vary across firms, but independent (dummy) variables only vary across zip code. In other words, it is conceivable that firms in the same zip code are not truly independent and the standard errors of individual firms may have a group structure within a zip code. This problem violates the assumption that all error terms are independent.

STATA has one feature to relax the assumption of dependence of intra group observations and produce robust standard errors. Option "cluster" specifies that the observations are independent across groups (clusters), but not necessarily within groups.

2.5 Unbalanced Panel Data

Researchers in policy evaluation increasingly use panel data. Panel data enables analysts to study dynamic changes over space and time due to the economic policy being enforced. Panel data has a cross-sectional unit of observation, each observed at several points in time. The typical panel data, called "balanced panel" or "complete panel" data, include time dimensions that are the same for each cross-sectional unit. However, in reality, it is common for some cross-sectional units to have missing values in several periods. This kind of data is called "unbalanced panel" or "incomplete panel" data. The data used in this study is unbalanced panel data since, by nature, employment and real wage information are nonexistent before firms' opening for business and after firms' closing during the period from Q1 of 1990 to Q2 of 2003.

Given that some observations are missing in time dimension, the error components of the unbalanced panel data are different from those of the balanced panel data and need to be estimated differently. The estimation of unbalanced panel data has received extensive attention by researchers. Among them, Baltagi (2001), Baltagi and Chang(1994), Hsiao (2003) have dealt effectively with the unbalanced panel data estimations. Most of the popular econometric software packages such as E-Views, SAS, and STATA are now available for the estimation of unbalanced panel data. This study uses STATA to estimate the above model specifications since it is widely accepted that the estimation on unbalanced panel data from STATA is robust.

In this study, the cross sectional unit is individual firm. For each individual firm, we observe 54 quarters covering from Q1:1990 to Q2:2003. For employment effect, the unit of observation is quarterly job number for each firm. For real wage effect, the unit of observation is average quarterly real wage per worker for each firm. The number of firms in different types and in each group is listed in the following table.

	Treatment Group (1st Round RZ)	Comparison Group1 (2nd Round RZ)	Comparison Group2 (propensity score)
All Firms	1815	131	7671
Manufacture Firms	231	43	451
Service Firms	780	50	3827
Large Firms (Employment>50)	86	13	253
Small Firms (Employment<=50)	1729	118	7418
Large Firms (Employment>20)	217	24	779
Small Firms (Employment<=20)	1598	107	6892

Note that for the dynamic effect dummy variables $RZY1_{it}$,...$RZY7_{it}$, only three year dummies $RZY1_{it}$, $RZY2_{it}$, and $RZY3_{it}$ are actually used in comparison group1 (2nd round Renaissance Zone) estimations even though we have the data for seven years. This is because the 2nd round Renaissance Zones started to receive tax incentives in January 2000. Since both the treatment and comparison groups were under the same tax incentive programs beginning the first quarter of 2000, putting the data of both treatment and comparison groups together and continuing to regard the 2nd round Renaissance Zone as a

control group after year 2000 does not allow us to single out and identify the true treatment effect.

3. Estimation Results of Employment and Establishment Effect with Zip Level Data

3.1 Aggregated Zip Level Data

To give a big "picture" of RZ program effects, I aggregate firms in the same zip code together and examine the employment and establishment effect at the zip code level. The advantage of using aggregated zip level data is that it can distinguish employment from new firms, from deceased firms and from existing firms. It also can tell how many firms in one zip code are new firms, deceased firms and existing firms. Another advantage of aggregated zip level data is to avoid the aforementioned intra group correlations among observations in the same zip code.

Since the total observations of zip level data are much less than those of firm level data, I only estimate fixed effect and random growth rate models here. A single time dummy and multiple time dummies are applied in both models for employment effects. Only one time dummy variable is used for establishment effect.

3.2 Employment Effect

Tables 3.1 and 3.2 list the estimation results for all firms. In the case of comparion1, coefficients of both single dummy and multiple dummies are not significant in two models. In the comparion2 case, the coefficient of the single dummy is not significant either, but the coefficients of *RZY7* in both models are negative and significant.

This is an interesting finding since it is hard to believe that the employment effect lags up to seven years.

For new firms, the estimation results in Tables 3.3 and 3.4 show that the coefficients of the single dummy are not significant in all cases. In the comparison1 case, coefficients of *RZY1* in both models are positive and significant and the coefficient of *RZY2* is significant only in fixed effect model. This result indicates that new firms in the zone increase their employment significantly more than new firms not in the zone in the first year of zone designation. In the comparison2 case, the coefficients of *RZY2* in both models are positive and significant and the coefficient of *RZY7* is negative and significant, the same results as for all firms. This result also implies that RZ programs work in the first two years. But the negative effect in the last year is still a challenging finding.

Turning to closed firms, Tables 3.5 and 3.6 show that there is no significant effect in the case of comparison1. In the case of comparison2, only coefficients of *RZY4* in both models are positive and significant at 10% level. This means that dead firms in the zone lost more jobs in the 4^{th} year of zone designation than dead firms not in the zone.

Tables 3.7 and 3.8 provide the estimation results for existing firms. It is easy to see that in all cases, no significant employment effect can be found.

In sum, there exists positive employment effects of RZ programs in the first two years of zone designation for new firms and negative employment effect in the 4^{th} year of zone designation for dead firms. It seems that RZ programs don't have any impact on

existing firms. However, a significant and negative effect in the 7^{th} year of zone designation for all firms and new firms in the case of comparison2 needs further examination.

3.3 Establishment Effect

Using the same methods as above, I examine if the RZ programs have any effect on the number of establishments for all firms, new firms, and dead firms.

Table 3.9 gives the results for all firms taken together. Only the result from the random growth rate model in the case of comparison1 shows that RZ programs cause the total number of firms to decline.

The estimation results for new firms are listed in Table 3.10. In both comparison1 and comparison2 cases, the coefficients of *RZ* dummy from random growth rate model are significant. The negative sign indicates that fewer firms open up in zones than not in zones.

Table 3.11 provides the estimation results for closed down firms. The coefficients of *RZ* in all cases are significant and negative. This result means that there are less establishments closed in zones than not in zones due to the zone designation, or RZ programs help to prevent around 26% to 30% firms in zone to close.

Overall, RZ programs cause fewer firms to open up, and fewer firms to close down in zone areas than not in zone areas at the same time. But, the total number of establishments in zones does not change due to zone designation.

4. Estimation Results of Employment Effect with Firm Level Data

Compared to aggregate zip level data, firm level data have more observations to allow for more complicated model specifications and to better analyze the firms' behaviors due to RZ programs.

4.1 Employment Effect for All Firms Taken Together

Tables 3.12, 3.13 and 3.14 present the estimation results of employment effects for all firms. When the 2nd round Renaissance Zones are chosen as the comparison group (comparison1) and a single *RZ* dummy variable is used, it can be seen that the RZ programs have insignificant impact on employment in all three model estimations. The same conclusions of no employment effect are reached when examining the dynamic effect of the zone programs with three-year *RZ* dummy variables.

When we use the propensity score picked comparison group (comparison2), the results are not more revealing than those from comparison1. In cases of both the single *RZ* dummy variable and dynamic multiple dummy variables, fixed effect and random growth rate model specifications show that the employment effects of RZ programs are all statistically insignificant, and imply that the RZ programs didn't boost employment. The results from the lagged dependent variable model show that employment increased by 2.6% due to zone designation, when examining the single time effect of the zone program. In the dynamic multiple years of *RZ* dummy variable case, the RZ program effects are significant and negative for the 4th year, but significant and positive for the 7th

year in the lagged dependent variable model. The significant positive coefficient of the 7th year dummy $RZY7_{it}$ is in contrast to the results from aggregated zip data with opposing signs.

For all firms taken together, comparison1 and comparison2 estimations do not generate mutually consistent conclusions regarding the employment effect of RZ programs. One possible explanation might be that comparison2 estimation has a longer period of data than comparison1 estimation.

4.2 Employment Effect for Manufacturing Firms

Tables 3.15, 3.16 and 3.17 provide the estimation results of employment effects for manufacturing firms. Tables 3.18, 3.19 and 3.20 provide the estimation results of employment effects for service firms (discussed in the next subsection). These results are derived from estimating the respective subset of samples that were used in all firms' case. Two-digit-NAIC or SIC code is used to select the manufacturing and service firms.

For manufacturing firms, in comparison1 estimations, the employment effect of both a single *RZ* dummy variable and multiple dynamic *RZ* dummies is not significant.

The results from comparison2 estimation are quite similar to those from the comparison2 estimation for all firms taken together. The coefficient of the single *RZ* dummy is significant only in lagged dependent variable model estimation. The dynamic time effect appears to be similar between comparison2 estimations for all firms and for manufacturing firms. The coefficients of dummy variables *RZY2* and *RZY7* imply that

zone programs have a negative effect on employment during the second year and have a positive effect on employment during the 7^{th} year. But, in the fixed effect and random growth rate models, the results do not show any dynamic time effect pattern.

Considering all estimation results for manufacturing firms, we may conclude that the employment effect is weak or insignificant for both single dummy and multiple dynamic dummy variables.

4.3 Employment Effect for Service Firms

Turning to service firms, the results of the fixed effect and random growth rate models in comparison1 estimations show that the employment effect of RZ programs is not significant, while in the lagged dependent variable model RZ programs raise employment by around 3.4%. In addition, no dynamic time effect was found in all three specifications except that *RZY3* in the lagged dependent variable model is positive and significant.

In comparison2 estimations, the results from estimating three models are very close and consistent with one another. In the case of a single time-effect dummy *RZ* specification, the coefficients are statistically significant and positive in all models, indicating that RZ programs increase employment by 9.8%, 8.8% and 9.6% in the fixed effect model, random growth rate model, and lagged dependent variable model respectively. With respect to the case of multiple dynamic time-effect dummies, estimation results of all three models show that RZ programs have significant negative

impacts on employment at the 4^{th}, 5^{th}, and 6^{th} year of zone designation, and significant positive impact on employment at the 7^{th} year of zone designation. The specific timing and reversal of effects found here are interesting and need further study.

4.4 Employment Effect for Large Firms

The only official standard available to define the size of firms is given by the Small Business Administration (SBA). In terms of the number of workers employed, SBA considers those having less than 500 employees for most manufacturing and mining industries as small firms. This number is too big for most firms in our study though. Thus, we take 20 employees as the cutoff point for large firms. According to this standard, around 10% of the firms in the sample are large firms, and around 90% are small firms.

Examining the employment and wage effects of RZ programs for small firms can also be viewed as a sensitivity test if we take large firms as the outliers. The results for all firms will be robust if they are similar to the results for small firms after trimming large firms out of the whole sample.

Tables 3.21, 3.22 and 3.23 provide the estimation results of employment effect for large firms. In the case of comparison1, estimated coefficients of the single *RZ* dummy in all three models are not significant. Whereas, only in the lagged dependent variable model, the coefficients of multiple time dummies are positive and significant in the first year of zone designation, and negative and significant in the third year of zone

designation. The magnitude of decrease in the third year is larger than the increase in the first year.

In the case of comparison2, the coefficient of a single dummy *RZ* is negative and significant only in the lagged dependent variable model. The results indicate that employment declines by 5.1% due to zone designation. The RZ's multiple dynamic time effects in all three models are pretty similar too. They show that employment increases significantly in the 5^{th} or 6^{th} year of zone designation, but decreases dramatically in the 7^{th} year of zone designation.

4.5 Employment Effect for Small Firms

The employment effects of the RZ program for small firms are shown in Tables 3.24, 3.25, and 3.26. In the case of comparison1, only the lagged dependent variable estimation shows that the coefficient of a single time *RZ* dummy and the coefficient of multiple time 3^{rd} year dummy *RZY3* are positive and significant. The other two model estimations don't show any significant employment effect in both single time dummy and multiple dynamic dummies. These results are qualitatively and quantitatively similar to the results obtained earlier for service firms.

In the case of comparison2, the coefficient of a single time dummy *RZ* is significant in fix effect and lagged dependent variable models. It indicates that zone designation causes employment to increase by 3.8% and 3.6% respectively in the fixed effect model and lagged dependent variable model. For multiple time dummies, the

results are very close between the fix effect model and lagged dependent variable model. They all show that employment declines in the 4th year and 6th year of the RZ program and rises more in the 7th year of the RZ program. In the random growth rate model, no multiple time effect exists.

4.6 Summary

The employment effect is summarized in the following table:

Summary Table for Employment Effect Estimations

	Single Time Effect		Dynamic Time Effect	
	Comparison1	Comparison2	Comparison1	Comparison2
All Firms Taken Together	No	Yes & Weak	No	Yes & Weak
Manufacturing Firms	No	Yes & Weak	No	Yes & Weak
Service Firms	Yes & Weak	Yes & Strong	Yes & Strong	Yes & Strong
Large Firms	No	Yes & Weak	Yes & Weak	Yes & Strong
Small Firms	Yes & Weak	Yes & Strong	Yes & Weak	Yes & Strong

Overall, in comparison1 estimations, the employment effects of Renaissance Zone programs are not significant for all firms and for manufacturing firms in both the single time-effect dummy and multiple dynamic time-effect dummies cases. The results for all firms taken together are consistent with those from zip level data. But, the significant effects of RZ programs for service firms, small firms and large firms only exist in the lagged dependent variable model. In comparison2 estimations, employment effects are much stronger than those in comparison1 estimations. In the single time-effect case, as a

result of the RZ designation, employment increases 2.6% for all firms taken together and 6.4% for manufacturing firms in the lagged dependent variable model, increases 9.4% on average for service firms, and increases 3.7% on average for small firms. However, a significant negative employment effect is found for large firms in the lagged dependent variable model. The employment of large firms declines about 5.1% due to zone designation. In the multiple time-effect case, most comparison2 cases show that dynamic time effect exists. For service firms and small firms in particular, employment decreases significantly in the first six years and then increases in the seventh year more than the decrease in the previous years. For large firms, the multiple time effect is opposite to the above pattern. The dynamic time-effect for manufacturing firms in the comparison1 estimation is similar to that for all firms.

It appears that the estimation results for service firms are very similar to the results for small firms. The employment effect of the RZ program in service firms, however, seems to be stronger than that in small firms. The employment effects for all firms, manufacturing firms, and large firms are found to be weak since the coefficient only appears significant in one out of three model estimations.

5. Estimation Results of Real Wage Effect with Firm Level Data

This subsection uses the same method and procedure as the last subsection to examine the real wage effect of the Renaissance Zone programs. The quarterly real wage per worker is calculated in the following formula.

$$\text{Real Wage} = \text{Nominal Wage} / (1 + \text{Inflation})$$

where the nominal wage is the average quarterly payroll per worker, inflation is measured by the U.S. consumer price index (1982-84=100). The estimation procedure used here also takes care of intra group correlation problems.

5.1 Wage Effect for All Firms Taken Together

Tables 3.27, 3.28, and 3.29 display the estimation results for all firms based on the three model specifications. In comparison1 estimations, no significant single-time wage effect and multiple-time wage effect were found in all three model specifications.

In comparison2 estimations, the coefficients of *RZ* in all three models are not statistically significant, indicating that zone designation didn't cause real wage to change. For the multiple dynamic time effect case, the real wage significantly increased in the 2nd year of zone designation only in the random growth model, and significantly decreased in the 4th year of zone designation in the lagged dependent variable model.

5.2 Wage Effect for Manufacturing Firms

Tables 3.30, 3.31, and 3.32 list the estimation results for manufacturing firms based on the three model specifications. In comparison1 estimations, for manufacturing firms, the coefficients of a single *RZ* dummy variable show that RZ designation exerts no significant effects on manufacturing wages for all three models. But for the dynamic multiple time effect case, the manufacturing real wage significantly increased 8.2% and

5.2% in the 2nd year in the random growth rate model and lagged dependent variable model respectively.

In comparison2 estimations, the real wage permanently decreased (*RZ* coefficients) 9.4%, 10.8% and 6.6% in the fixed effect model, random growth rate model and lagged dependent variable model respectively. All decreases are statistically significant. Turning to the dynamic time effect, random growth rate and lagged dependent variable model estimations show that the real wage significantly rose in the 2nd year or the 3rd year, and then significantly declined in the 7th year. The magnitude of wage decline is always greater than the magnitude of wage increase, which causes the real wage to decline eventually. However, no significant dynamic time effect was found in the fix effect model estimation.

5.3 Wage Effect for Service Firms

Tables 3.33, 3.34, and 3.35 list the estimation results for service firms based on the three model specifications. In both comaprison1 and comparison2 estimations, the real wage effects of RZ programs are shown negative and significant, except that the real wage effect is insignificant in the comparison1 fix effect model estimation. The coefficients of *RZ* in comparison2 estimations are greater than those in the comparison1 estimation. For example, the real wage decreased 6.8% in comparison1 estimation, and 11.7% in comparison2 estimation in the random growth rate model. In the lagged

dependent variable model, the real wage decreased 3.7% in comparison1 estimation, and 6.5% in comparison2 estimation.

For the dynamic time effect, results from comparison1 estimations of the three models show that real wage increased in the 2nd year and decreased more in the 3rd year. Results from comparison2 estimations in all three models also show that real wage increased in the 2nd, 4th, 5th, and 6th year, but decreased more in the 7th year.

5.4 Wage Effect for Large Firms

The estimation results of the real wage effect for large firms are provided in Tables 3.36, 3.37, and 3.38. In the case of comparison1 estimations, there is no evidence that the RZ program has impact on the real wage of large firms since the coefficients of a single *RZ* dummy and multiple time dummies in all three models are not significant. In the case of comparison2 estimations, the coefficient of a single *RZ* dummy is not significant in all three models. But, the dynamic time effect from the fixed effect model estimation is contrary to that from the lagged dependent variable model. In the random growth rate models, no dynamic time effect exists.

5.5 Wage Effect for Small Firms

Tables 3.39, 3.40, and 3.41 provide the estimation results for small firms. In the case of comparison1 estimations, both the single-time RZ effect and multiple dynamic time effects on real wage are not significant in all three models.

In the case of compariosn2 estimations, the single time RZ effect is significant and consistent for the fixed effect model and random effect model. The real wage declines by 4.8% and 6.5% in the fixed effect model and the random growth rate model respectively. The multiple dynamic time effect in the fixed effect model is in contrast to that in the random growth rate model. The real wage decreases in first six years and increases in the seventh year in the fixed effect model, but increases in the first six years and decreases in the seventh year in the random growth rate model. The opposite results from the preceding two models are surprising and need further study. In the lagged dependent variable model, only coefficient of *RZY7* is found significant and negative.

5.6 Summary

The real wage effect for all cases is summarized in the following table. Overall, Renaissance Zone programs have no impact on real wage if we put all firms together. They have a negative real wage impact for manufacturing firms, for service firms and for small firms. This negative impact is larger for manufacturing firms and service firms than for small firms. In both single time effect and multiple dynamic time effect cases, the real wage effect from comparison2 estimations is more evident than that from comparison1 estimations since comparion2 has a longer time period than comparion1. Taking the random growth rate model as a representative case, zone programs cause the real wage to decline by about 10.8% for manufacturing firms, 10.9% for service firms, and 6.5% for small firms. In the case of comparison2 estimations, the RZ wage effect for

manufacturing firms is similar to the effect for service firms and the magnitude of impact is also very close to that for service firms. The impact magnitude for small firms is smaller than for manufacturing firms and service firms.

Summary Table for Wage Effect Estimations

	Single Time Effect		Dynamic Time Effect	
	Comparison1	Comparison2	Comparison1	Comparison2
All Firms Taken Together	No	No	No	Yes & Weak
Manufacturing Firms	No	Yes & Strong	Yes & Weak	Yes & Strong
Service Firms	Yes & Strong	Yes & Strong	Yes & Strong	Yes & Strong
Large Firms	No	No	No	Yes & Weak
Small Firms	No	Yes & Strong	No	Yes & Strong

Finally, the estimation results from both comparison groups can infer a clear time effect zone program pattern for manufacturing firms and service firms. Typically, the real wage starts to increase in the first six years of zone designation, but decrease more in the 3^{rd} year in the comparison1 case and in the 7^{th} year in the comparison2 case. However, the dynamic time effect is not obvious for all firms, large firms and small firms.

6. Conclusions

In this chapter, we first use the aggregated zip level data to examine the employment and establishment effects of Renaissance Zone programs for all firms, and for new firms, dead firms and existing firms by applying fixed effect and random growth rate models. Then we examine the employment and real wage effects of the RZ programs

for all firms taken together, and manufacturing firms, service firms, large firms and small firms by applying fixed effect, random growth rate, and lagged dependent variable models on establishment level data. Two alternative comparison groups, the 2nd round Renaissance Zone and Propensity Score picked group, are used in the analysis. A single time effect dummy and a set of dynamic multiple time dummies are applied on employment and real wage effect estimations. Several key findings are:

1. From the estimations on aggregated zip level data, there is no evidence of significant permanent employment change due to zone designation. But, when examining the dynamic multiple time effect, RZ programs raise employment in the first two years of zone designation for new firms and have negative employment effect in the 4th year of zone designation for deceased firms. Another important finding is that RZ programs cause fewer firms to open up, and fewer firms to close down in zone areas than in non-zone areas at the same time.

2. In general, employment effects from aggregated zip level data and firm level data are consistently insignificant, when we pool all firms together. However, by applying firm level data, the Renaissance Zone programs raise employment by around 9% for service firms and around 3.8% for small firms. For manufacturing firms and large firms, the employment effect of the RZ program, just like that for all firms taken together, is not significant either.

3. Overall, RZ programs tend to lower real wage per worker. For manufacturing firms, the real wage significantly drops around 10.8%. For service firms, RZ programs cause real wage to decrease about 11.7%. For small firms, real wage drops around 6.5%. There is no real wage effect for all firms taken together and for large firms.

4. The employment effects are stronger for service firms than for manufacturing firms, stronger for small firms than for large firms. The real wage effects are stronger for service firms, manufacturing firms and small firms. This seems to suggest that zone programs are more effective in influencing service firms or small firms. For service firms and small firms, the positive employment effects imply that the output effect is stronger than the substitute effect. It is also true that most service firms and small firms are more labor intensive. The decreases in real wage for service firms and small firms cause them to hire more labor. Thus, in terms of employment provision, the RZ program seems to favor labor over capital.

5. In most cases, the results for manufacturing firms and large firms are similar, and the results for service firms and small firms are similar as well. This is because most manufacturing firms are large in size and service firms tend to be small in size. The consistent results for both pairs of firms also show that the overall conclusions are robust. In all cases, the employment and real wage effects from comparison2 estimations are stronger than those from compariosn1 estimations. This might be because comparison2

estimations have longer data series than comparison1 to examine the effects of the RZ program, thereby allowing the lagged effect of RZ programs to show up.

6. Comparing the extent of employment effect and real wage effect, wage impact of the zone programs appears to be a little larger than that of the employment impact. This result is consistent with Papke's (1993) finding. This conclusion is more evident in dynamic time effect cases than in single time (permanent) effect cases.

7. In most cases, the results from the three model specifications are consistent with one another. This is a reason that we don't feel the need to conduct model specification tests.

8. Some interesting findings need further study. For example, why are there sudden significant employment and real wage changes in the 7th year of zone designation?

Table 3.1

Aggregate Employment Effect Estimation for All Firms:
Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.017	_	-0.086	_
	(0.056)	_	(0.056)	_
RZY1	_	0.047	_	0.076
	_	(0.0.069)	_	(0.072)
RZY2	_	0.035	_	0.065
	_	(0.068)	_	(0.071)
RZY3	_	-0.059	_	0.068
	_	(0.070)	_	(0.72)
RZY4	_	_	_	-0.006
	_	_	_	(0.071)
RZY5	_	_	_	0.028
	_	_	_	(0.072)
RZY6	_	_	_	-0.061
	_	_	_	(0.073)
RZY7	_	_	_	-0.176
	_	_	_	(0.075)
R2	0.0001	0.0008	0.0008	0.0028
Observations	1519	1519	2815	2815

Table 3.2

Aggregate Employment Effect Estimation for All Firms:
Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.015	_	-0.081	_
	(0.056)	_	(0.055)	_
RZY1	_	0.047	_	0.075
	_	(0.068)	_	(0.071)
RZY2	_	0.035	_	0.063
	_	(0.069)	_	(0.071)
RZY3	_	-0.057	_	0.066
	_	(0.070)	_	(0.071)
RZY4	_	_	_	-0.007
	_	_	_	(0.072)
RZY5	_	_	_	0.027
	_	_	_	(0.071)
RZY6	_	_	_	-0.057
	_	_	_	(0.072)
RZY7	_	_	_	-0.169**
	_	_	_	(0.075)
R2	0.0001	0.0008	0.0008	0.0027
Observations	1519	1519	15575	15575

Table 3.3

Aggregate Employment Effect Estimation for New Firms:
Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	0.341	_	0.030	_
	(0.282)	_	(0.259)	_
RZY1	_	1.093***	_	0.477
	_	(0.366)	_	(0.395)
RZY2	_	0.618*	_	0.716*
	_	(0.367)	_	(0.386)
RZY3	_	-0.539	_	0.451
	_	(0.351)	_	(0.391)
RZY4	_	_	_	-0.137
	_	_	_	(0.422)
RZY5	_	_	_	0.040
	_	_	_	(0.402)
RZY6	_	_	_	0.246
	_	_	_	(0.487)
RZY7	_	_	_	-0.937**
	_	_	_	(0.467)
R2	0.0023	0.0295	0.000	0.0118
Observations	646	646	1399	1399

Table 3.4

Aggregate Employment Effect Estimation for New Firms:
Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	0.428	_	0.025	_
	(0.293)	_	(0.275)	_
RZY1	_	1.063***	_	0.468
	_	(0.378)	_	(0.403)
RZY2	_	0.589	_	0.704*
	_	(0.379)	_	(0.394)
RZY3	_	-0.446	_	0.471
	_	(0.370)	_	(0.400)
RZY4	_	_	_	-0.140
	_	_	_	(0.432)
RZY5	_	_	_	0.031
	_	_	_	(0.414)
RZY6	_	_	_	0.366
	_	_	_	(0.513)
RZY7	_	_	_	-1.036**
	_	_	_	(0.501)
R2	0.0035	0.0283	0.000	0.0117
Observations	646	646	1399	1399

Table 3.5

Aggregate Employment Effect Estimation for Closed Firms:
Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.165	_	-0.273	_
	(0.296)	_	(0.282)	_
RZY1	_	-0.290	_	-0.371
	_	(0.372)	_	(0.387)
RZY2	_	0.018	_	0.303
	_	(0.412)	_	(0.415)
RZY3	_	-0.010	_	-0.498
	_	(0.418)	_	(0.418)
RZY4	_	_	_	0.779*
	_	_	_	(0.426)
RZY5	_	_	_	-0.466
	_	_	_	(0.412)
RZY6	_	_	_	0.732
	_	_	_	(0.541)
RZY7	_	_	_	-0.771
	_	_	_	(0.541)
R2	0.0005	0.0014	0.0006	0.0008
Observations	682	682	1553	1553

Table 3.6

Aggregate Employment Effect Estimation for Closed Firms:
Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.189	–	-0.319	–
	(0.306)	–	(0.294)	–
RZY1	–	-0.291	–	-0.329
	–	(0.385)	–	(0.395)
RZY2	–	0.037	–	0.356
	–	(0.427)	–	(0.424)
RZY3	–	-0.047	–	-0.514
	–	(0.436)	–	(0.428)
RZY4	–	–	–	0.721*
	–	–	–	(0.437)
RZY5	–	–	–	-0.531
	–	–	–	(0.423)
RZY6	–	–	–	0.844
	–	–	–	(0.570)
RZY7	–	–	–	-0.874
	–	–	–	(0.574)
R2	0.0006	0.0015	0.0008	0.0057
Observations	682	682	1553	1553

Table 3.7

Aggregate Employment Effect Estimation for Existing Firms:
Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.076	_	0.047	_
	(0.060)	_	(0.058)	_
RZY1	_	-0.016	_	-0.003
	_	(0.073)	_	(0.076)
RZY2	_	0.017	_	0.012
	_	(0.073)	_	(0.076)
RZY3	_	-0.077	_	-0.013
	_	(0.074)	_	(0.077)
RZY4	_	_	_	-0.023
	_	_	_	(0.076)
RZY5	_	_	_	-0.037
	_	_	_	(0.076)
RZY6	_	_	_	-0.068
	_	_	_	(0.078)
RZY7	_	_	_	0.114
	_	_	_	(0.078)
R2	0.0011	0.0011	0.0002	0.001
Observations	1521	1521	2817	2817

Table 3.8

Aggregate Employment Effect Estimation for Existing Firms:
Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison 2 (Propensity Score)	
RZ	-0.075	–	0.052	–
	(0.061)	–	(0.058)	–
RZY1	–	-0.016	–	-0.005
	–	(0.074)	–	(0.076)
RZY2	–	0.017	–	0.010
	–	(0.074)	–	(0.076)
RZY3	–	-0.076	–	-0.015
	–	(0.075)	–	(0.076)
RZY4	–	–	–	-0.025
	–	–	–	(0.076)
RZY5	–	–	–	-0.039
	–	–	–	(0.076)
RZY6	–	–	–	-0.065
	–	–	–	(0.077)
RZY7	–	–	–	0.123
	–	–	–	(0.078)
R2	0.001	0.0011	0.0003	0.0012
Observations	1521	1521	2817	2817

Table 3.9

Establishment Number Effect for All Firms

	Fixed Effect Model		Random Growth Rate Model	
	Comparison1 (2nd Round RZ)	Comparison 2 (Propensity Score)	Comparison1 (2nd Round RZ)	Comparison 2 (Propensity Score)
RZ	-0.050	-0.049	-0.219***	-0.214
	(0.039)	(0.039)	(0.038)	(0.038)
R2	0.0011	0.001	0.0115	0.0112
Observations	1519	1519	2815	2815

Table 3.10

Establishment Number Effect for New Firms

	Fixed Effect Model		Random Growth rate Model	
	Comparison1 (2nd Round RZ)	Comparison 2 (Propensity Score)	Comparison1 (2nd Round RZ)	Comparison 2 Propensity Score)
RZ	-0.026	-0.023	-0.189*	-0.192*
	(0.102)	(0.107)	(0.107)	(0.114)
R2	0.0001	0.0001	0.0022	0.0021
Observations	646	646	1399	1399

Table 3.11

Establishment Number Effect for Closed Firms

	Fixed Effect Model		Random Growth rate Model	
	Comparison1 (2nd Round RZ)	Comparison 2 (Propensity Score)	Comparison1 (2nd Round RZ)	Comparison 2 (Propensity Score)
RZ	-0.258***	-0.259***	-0.299***	-0.294***
	(0.115)	(0.118)	(0.125)	(0.130)
R2	0.0074	0.0074	0.0037	0.0034
Observations	682	682	1553	1553

Table 3.12

Employment Effect Estimation for All Industries: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.009	_	0.027	_
	(0.030)	_	(0.029)	_
RZY1	_	0.013	_	0.010
	_	(0.020)	_	(0.015)
RZY2	_	-0.003	_	-0.005
	_	(0.020)	_	(0.016)
RZY3	_	0.002	_	-0.013
	_	(0.056)	_	(0.015)
RZY4	_	_	_	-0.022
	_	_	_	(0.016)
RZY5	_	_	_	-0.011
	_	_	_	(0.016)
RZY6	_	_	_	-0.015
	_	_	_	(0.018)
RZY7	_	_	_	0.071
	_	_	_	(0.087)
R2	0.000	0.0001	0.000	0.0001
Observations	34351	34351	227165	227165

Table 3.13

Employment Effect Estimation for All Industries: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.006	–	0.024	–
	(0.029)	–	(0.029)	–
RZY1	–	0.025	–	0.009
	–	(0.020)	–	(0.015)
RZY2	–	0.012	–	-0.005
	–	(0.020)	–	(0.016)
RZY3	–	-0.033	–	-0.009
	–	(0.057)	–	(0.016)
RZY4	–	–	–	-0.017
	–	–	–	(0.017)
RZY5	–	–	–	-0.002
	–	–	–	(0.016)
RZY6	–	–	–	-0.002
	–	–	–	(0.018)
RZY7	–	–	–	0.043
	–	–	–	(0.092)
R2	0.000	0.0002	0.000	0.0001
Observations	34001	34001	227165	227165

Table 3.14

Employment Effect Estimation for All Industries: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.009	_	0.026***	_
	(0.011)	_	(0.01)	_
RZY1	_	0.012	_	0.008
	_	(0.012)	_	(0.011)
RZY2	_	-0.006	_	-0.009
	_	(0.011)	_	(0.011)
RZY3	_	0.004	_	-0.009
	_	(0.015)	_	(0.011)
RZY4	_	_	_	-0.021*
	_	_	_	(0.011)
RZY5	_	_	_	-0.010
	_	_	_	(0.012)
RZY6	_	_	_	-0.011
	_	_	_	(0.012)
RZY7	_	_	_	0.068***
	_	_	_	(0.018)
R2	0.023	0.023	0.037	0.037
Observations	31548	31548	212608	212608

Table 3.15

Employment Effect Estimation for Manufacturing Industry: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.046	_	0.067	_
	(0.091)	_	(0.082)	_
RZY1	_	0.025	_	0.025
	_	(0.060)	_	(0.04)
RZY2	_	-0.058	_	-0.058
	_	(0.069)	_	(0.044)
RZY3	_	0.068	_	-0.044
	_	(0.17)	_	(0.032)
RZY4	_	_	_	-0.031
	_	_	_	(0.042)
RZY5	_	_	_	-0.029
	_	_	_	(0.049)
RZY6	_	_	_	0.022
	_	_	_	(0.047)
RZY7	_	_	_	0.149
	_	_	_	(0.221)
R2	0.0005	0.001	0.0004	0.001
Observations	5752	5752	19918	19918

Table 3.16

Employment Effect Estimation for Manufacturing Industry:
Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.019	_	-0.001	_
	(0.068)	_	(0.057)	_
RZY1	_	0.065	_	0.047
	_	(0.048)	_	(0.034)
RZY2	_	-0.008	_	-0.029
	_	(0.057)	_	(0.040)
RZY3	_	-0.059	_	-0.016
	_	(0.133)	_	(0.027)
RZY4	_	_	_	-0.006
	_	_	_	(0.036)
RZY5	_	_	_	0.006
	_	_	_	(0.040)
RZY6	_	_	_	0.060
	_	_	_	(0.037)
RZY7	_	_	_	-0.046
	_	_	_	(0.161)
R2	0.0001	0.0009	0.000	0.0004
Observations	5752	5752	19918	19918

Table 3.17

Employment Effect Estimation for Manufacturing Industry:
Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.038	_	0.064***	_
	0.028	_	(0.024)	_
RZY1	_	0.031	_	0.023
	_	(0.031)	_	(0.028)
RZY2	_	-0.043	_	-0.055*
	_	(0.031)	_	(0.028)
RZY3	_	0.035	_	-0.042
	_	(0.038)	_	(0.028)
RZY4	_	_	_	-0.031
	_	_	_	(0.029)
RZY5	_	_	_	-0.018
	_	_	_	(0.028)
RZY6	_	_	_	0.032
	_	_	_	(0.030)
RZY7	_	_	_	0.128***
	_	_	_	(0.040)
R2	0.011	0.012	0.008	0.007
Observations	5402	5402	18700	18700

Table 3.18

Employment Effect Estimation for Service Industry: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.034	_	0.098***	_
	(0.029)	_	(0.023)	_
RZY1	_	0.009	_	-0.004
	_	(0.022)	_	(0.016)
RZY2	_	-0.017	_	-0.003*
	_	(0.025)	_	(0.016)
RZY3	_	0.040	_	-0.020
	_	(0.060)	_	(0.019)
RZY4	_	_	_	-0.049**
	_	_	_	(0.021)
RZY5	_	_	_	-0.050***
	_	_	_	(0.014)
RZY6	_	_	_	-0.077***
	_	_	_	(0.015)
RZY7	_	_	_	0.275***
	_	_	_	(0.073)
R2	0.0003	0.0003	0.0004	0.0011
Observations	15375	15375	119521	119521

Table 3.19

Employment Effect Estimation for Service Industry: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.018	_	0.088***	_
	(0.030)	_	(0.024)	_
RZY1	_	0.021	_	-0.003
	_	(0.024)	_	(0.017)
RZY2	_	-0.002	_	-0.028*
	_	(0.026)	_	(0.016)
RZY3	_	0.003	_	-0.015
	_	(0.063)	_	(0.020)
RZY4	_	_	_	-0.042*
	_	_	_	(0.023)
RZY5	_	_	_	-0.041**
	_	_	_	(0.016)
RZY6	_	_	_	-0.063***
	_	_	_	(0.015)
RZY7	_	_	_	0.237***
	_	_	_	(0.083)
R2	0.0001	0.0002	0.0003	0.0008
Observations	15375	15375	119521	119521

Table 3.20

Employment Effect Estimation for Service Industry:
Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.034**	_	0.096***	_
	(0.016)	_	(0.014)	_
RZY1	_	0.007	_	-0.004
	_	(0.017)	_	(0.016)
RZY2	_	-0.017	_	-0.028*
	_	(0.018)	_	(0.016)
RZY3	_	0.041*	_	0.016
	_	(0.023)	_	(0.016)
RZY4	_	_	_	-0.048***
	_	_	_	(0.016)
RZY5	_	_	_	-0.055***
	_	_	_	(0.016)
RZY6	_	_	_	-0.076***
	_	_	_	(0.017)
RZY7	_	_	_	0.268***
	_	_	_	(0.024)
R2	0.029	0.029	0.0375	0.038
Observations	14356	14356	110291	110291

Table 3.21

Employment Effect Estimation for Large Firms: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.012	–	-0.045	–
	(0.024)	–	(0.035)	–
RZY1	–	0.041	–	0.040
	–	(0.036)	–	(0.024)
RZY2	–	0.022	–	0.020
	–	(0.034)	–	(0.022)
RZY3	–	-0.053	–	0.033
	–	(0.087)	–	(0.024)
RZY4	–	–	–	0.017
	–	–	–	(0.023)
RZY5	–	–	–	0.017**
	–	–	–	(0.023)
RZY6	–	–	–	0.061**
	–	–	–	(0.026)
RZY7	–	–	–	-0.178***
	–	–	–	(0.085)
R2	0.0001	0.0002	0.0002	0.0010
Observations	5591	5591	32598	32598

Table 3.22

Employment Effect Estimation for Large Firms: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.023	–	-0.042	–
	(0.041)	–	(0.036)	–
RZY1	–	0.053	–	0.040*
	–	(0.032)	–	(0.024)
RZY2	–	0.030	–	0.015
	–	(0.029)	–	(0.019)
RZY3	–	-0.077	–	0.037
	–	(0.077)	–	(0.024)
RZY4	–	–	–	0.018
	–	–	–	(0.024)
RZY5	–	–	–	0.023
	–	–	–	(0.023)
RZY6	–	–	–	0.074***
	–	–	–	(0.027)
RZY7	–	–	–	-0.191**
	–	–	–	(0.089)
R2	0.0002	0.0015	0.0001	0.0012
Observations	5591	5591	32598	32598

Table 3.23

Employment Effect Estimation for Large Firms: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.022	–	-0.051**	–
	(0.023)	–	(0.020)	–
RZY1	–	0.054**	–	0.041*
	–	(0.027)	–	(0.023)
RZY2	–	0.025	–	0.007
	–	(0.028)	–	(0.024)
RZY3	–	-0.072**	–	0.040*
	–	(0.031)	–	(0.024)
RZY4	–	–	–	0.022
	–	–	–	(0.025)
RZY5	–	–	–	0.031
	–	–	–	(0.025)
RZY6	–	–	–	0.078***
	–	–	–	(0.025)
RZY7	–	–	–	-0.205***
	–	–	–	(0.032)
R2	0.015	0.0167	0.0156	0.0169
Observations	5308	5308	30969	30969

Table 3.24

Employment Effect Estimation for Small Firms: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.023	_	0.038*	_
	(0.024)	_	(0.023)	_
RZY1	_	0.001	_	0.005
	_	(0.017)	_	(0.012)
RZY2	_	-0.014	_	-0.010
	_	(0.017)	_	(0.015)
RZY3	_	0.033	_	-0.021
	_	(0.044)	_	(0.013)
RZY4	_	_	_	-0.029**
	_	_	_	(0.014)
RZY5	_	_	_	-0.016
	_	_	_	(0.014)
RZY6	_	_	_	-0.028*
	_	_	_	(0.016)
RZY7	_	_	_	0.113*
	_	_	_	(0.063)
R2	0.0001	0.0002	0.0001	0.0002
Observations	28791	28791	186293	186293

Table 3.25

Employment Effect Estimation for Small Firms: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.003	_	0.029	_
	(0.022)	_	(0.023)	_
RZY1	_	0.016	_	0.005
	_	(0.017)	_	(0.012)
RZY2	_	0.005	_	-0.007
	_	(0.017)	_	(0.015)
RZY3	_	-0.012	_	-0.015
	_	(0.043)	_	(0.014)
RZY4	_	_	_	-0.022
	_	_	_	(0.015)
RZY5	_	_	_	-0.005
	_	_	_	(0.015)
RZY6	_	_	_	-0.014
	_	_	_	(0.016)
RZY7	_	_	_	0.076
	_	_	_	(0.068)
R2	0.000	0.0001	0.000	0.0001
Observations	28791	28791	186293	186293

Table 3.26

Employment Effect Estimation for Small Firms: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.025**	_	0.036***	_
	(0.011)	_	(0.011)	_
RZY1	_	-0.003	_	0.002
	_	(0.012)	_	(0.012)
RZY2	_	-0.017	_	-0.011
	_	(0.012)	_	(0.012)
RZY3	_	0.038**	_	-0.016
	_	(0.015)	_	(0.012)
RZY4	_	_	_	-0.027**
	_	_	_	(0.012)
RZY5	_	_	_	-0.017
	_	_	_	(0.013)
RZY6	_	_	_	-0.026**
	_	_	_	(0.013)
RZY7	_	_	_	0.108***
	_	_	_	(0.018)
R2	0.0296	0.0294	0.044	0.083
Observations	26684	26684	168909	168909

Table 3.27

Wage Effect Estimation for All Industries: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.015	_	-0.029	_
	(0.025)	_	(0.027)	_
RZY1	_	0.013	_	0.010
	_	(0.019)	_	(0.015)
RZY2	_	-0.003	_	-0.005
	_	(0.020)	_	(0.016)
RZY3	_	0.002	_	-0.013
	_	(0.057)	_	(0.015)
RZY4	_	_	_	-0.022
	_	_	_	(0.016)
RZY5	_	_	_	-0.011
	_	_	_	(0.016)
RZY6	_	_	_	-0.015
	_	_	_	(0.018)
RZY7	_	_	_	0.071
	_	_	_	(0.088)
R2	0.000	0.0001	0.000	0.0001
Observations	34334	34334	227006	227006

Table 3.28

Wage Effect Estimation for All Industries: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.030	–	-0.043	–
	(0.024)	–	(0.026)	–
RZY1	–	0.006	–	0.01
	–	(0.019)	–	(0.018)
RZY2	–	0.037	–	0.039**
	–	(0.023)	–	(0.017)
RZY3	–	-0.062	–	0.019
	–	(0.051)	–	(0.019)
RZY4	–	–	–	-0.013
	–	–	–	(0.019)
RZY5	–	–	–	0.021
	–	–	–	(0.018)
RZY6	–	–	–	0.022
	–	–	–	(0.022)
RZY7	–	–	–	-0.121
	–	–	–	(0.093)
R2	0.0002	0.0004	0.000	0.0001
Observations	34334	34334	227006	227006

Table 3.29

Wage Effect Estimation for All Industries: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.003	_	0.004	_
	(0.012)	_	(0.012)	_
RZY1	_	-0.016	_	-0.012
	_	(0.013)	_	(0.013)
RZY2	_	0.013	_	0.017
	_	(0.013)	_	(0.014)
RZY3	_	0.005	_	-0.002
	_	(0.016)	_	(0.014)
RZY4	_	_	_	-0.024*
	_	_	_	(0.014)
RZY5	_	_	_	-0.002
	_	_	_	(0.014)
RZY6	_	_	_	-0.001
	_	_	_	(0.015)
RZY7	_	_	_	0.024
	_	_	_	(0.022)
R2	0.139	0.139	0.147	0.147
Observations	31885	31885	212456	212456

Table 3.30

Wage Effect Estimation for Manufacture Industry: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.032	_	-0.094**	_
	(0.051)	_	(0.044)	_
RZY1	_	0.025	_	0.025
	_	(0.060)	_	(0.040)
RZY2	_	-0.058	_	-0.058
	_	(0.069)	_	(0.044)
RZY3	_	0.068	_	-0.044
	_	(0.170)	_	(0.032)
RZY4	_	_	_	-0.031
	_	_	_	(0.042)
RZY5	_	_	_	-0.029
	_	_	_	(0.049)
RZY6	_	_	_	0.022
	_	_	_	(0.047)
RZY7	_	_	_	0.149
	_	_	_	(0.221)
R2	0.0002	0.001	0.0005	0.143
Observations	5656	5656	19749	19749

Table 3.31

Wage Effect Estimation for Manufacture Industry: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.050	_	-0.108***	_
	(0.048)	_	(0.042)	_
RZY1	_	0.009	_	0.019
	_	(0.041)	_	(0.042)
RZY2	_	0.082**	_	0.092***
	_	(0.033)	_	(0.027)
RZY3	_	-0.113	_	0.065**
	_	(0.078)	_	(0.024)
RZY4	_	_	_	0.044
	_	_	_	(0.030)
RZY5	_	_	_	0.051
	_	_	_	(0.044)
RZY6	_	_	_	0.022
	_	_	_	(0.037)
RZY7	_	_	_	-0.324***
	_	_	_	(0.098)
R2	0.0005	0.0017	0.0006	0.0021
Observations	5656	5656	19749	19749

Table 3.32

Wage Effect Estimation for Manufacture Industry: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.019	_	-0.066**	_
	(0.028)	_	(0.028)	_
RZY1	_	-0.015	_	-0.004
	_	(0.031)	_	(0.032)
RZY2	_	0.052*	_	0.064**
	_	(0.031)	_	(0.032)
RZY3	_	-0.044	_	0.052
	_	(0.037)	_	(0.032)
RZY4	_	_	_	0.027
	_	_	_	(0.033)
RZY5	_	_	_	0.042
	_	_	_	(0.033)
RZY6	_	_	_	-0.005
	_	_	_	(0.034)
RZY7	_	_	_	-0.192***
	_	_	_	(0.046)
R2	0.152	0.153	0.132	0.133
Observations	5274	5274	18483	18483

Table 3.33

Wage Effect Estimation for Service Industry: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.057	_	-0.109***	_
	(0.038)	_	(0.030)	_
RZY1	_	0.009	_	-0.0042
	_	(0.022)	_	(0.016)
RZY2	_	-0.017	_	0.030*
	_	(0.025)	_	(0.016)
RZY3	_	0.039	_	-0.020
	_	(0.059)	_	(0.019)
RZY4	_	_	_	0.049**
	_	_	_	(0.021)
RZY5	_	_	_	0.050***
	_	_	_	(0.014)
RZY6	_	_	_	0.077***
	_	_	_	(0.015)
RZY7	_	_	_	-0.275***
	_	_	_	(0.073)
R2	0.0005	0.0003	0.0003	0.0011
Observations	14982	14982	118768	118768

Table 3.34

Wage Effect Estimation for Service Industry: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.068*	_	-0.117***	_
	(0.037)	_	(0.030)	_
RZY1	_	0.027	_	0.031
	_	(0.026)	_	(0.026)
RZY2	_	0.093**	_	0.096***
	_	(0.037)	_	(0.024)
RZY3	_	-0.157**	_	0.032
	_	(0.078)	_	(0.028)
RZY4	_	_	_	0.013
	_	_	_	(0.024)
RZY5	_	_	_	0.076***
	_	_	_	(0.020)
RZY6	_	_	_	0.092***
	_	_	_	(0.019)
RZY7	_	_	_	-0.382***
	_	_	_	(0.084)
R2	0.0008	0.0023	0.0003	0.001
Observations	14982	14982	118768	118768

Table 3.35

Wage Effect Estimation for Service Industry: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.037**	–	-0.065***	–
	(0.019)	–	(0.018)	–
RZY1	–	0.013	–	0.013
	–	(0.020)	–	(0.020)
RZY2	–	0.063***	–	0.063***
	–	(0.020)	–	(0.021)
RZY3	–	-0.091***	–	0.012
	–	(0.026)	–	(0.021)
RZY4	–	–	–	-0.002
	–	–	–	(0.021)
RZY5	–	–	–	0.049**
	–	–	–	(0.021)
RZY6	–	–	–	0.071***
	–	–	–	(0.022)
RZY7	–	–	–	-0.222***
	–	–	–	(0.032)
R2	0.153	0.154	0.155	0.156
Observations	13782	13782	109265	109265

Table 3.36

Wage Effect Estimation for Large Firms: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.001	_	-0.0002	_
	(0.037)	_	(0.064)	_
RZY1	_	0.041	_	0.040
	_	(0.037)	_	(0.024)
RZY2	_	0.022	_	0.020
	_	(0.034)	_	(0.022)
RZY3	_	-0.053	_	0.033
	_	(0.087)	_	(0.024)
RZY4	_	_	_	0.017
	_	_	_	(0.023)
RZY5	_	_	_	0.017
	_	_	_	(0.023)
RZY6	_	_	_	0.061**
	_	_	_	(0.026)
RZY7	_	_	_	-0.177**
	_	_	_	(0.085)
R2	0.000	0.0008	0.000	0.0010
Observations	5567	5567	32533	32533

Table 3.37

Wage Effect Estimation for Large Firms: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.017	_	-0.008	_
	(0.035)	_	(0.064)	_
RZY1	_	-0.009	_	0.008
	_	(0.037)	_	(0.031)
RZY2	_	0.016	_	0.034
	_	(0.035)	_	(0.022)
RZY3	_	-0.021	_	-0.024
	_	(0.065)	_	(0.026)
RZY4	_	_	_	-0.042
	_	_	_	(0.030)
RZY5	_	_	_	-0.041
	_	_	_	(0.034)
RZY6	_	_	_	-0.017
	_	_	_	(0.042)
RZY7	_	_	_	0.051
	_	_	_	(0.131)
R2	0.0001	0.0002	0.000	0.0002
Observations	5567	5567	32533	32533

Table 3.38

Wage Effect Estimation for Large Firms: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.017	_	0.032	_
	(0.022)	_	(0.021)	_
RZY1	_	-0.0344	_	-0.015
	_	(0.026)	_	(0.025)
RZY2	_	-0.007	_	0.014
	_	(0.026)	_	(0.026)
RZY3	_	0.044	_	-0.049*
	_	(0.030)	_	(0.027)
RZY4	_	_	_	-0.044
	_	_	_	(0.027)
RZY5	_	_	_	-0.033
	_	_	_	(0.027)
RZY6	_	_	_	-0.032
	_	_	_	(0.027)
RZY7	_	_	_	0.143***
	_	_	_	(0.034)
R2	0.163	0.1631	0.154	0.154
Observations	5274	5274	30885	30885

Table 3.39

Wage Effect Estimation for Small Firms: Fixed Effect Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.003	_	-0.048**	_
	(0.025)	_	(0.024)	_
RZY1	_	0.001	_	0.005
	_	(0.017)	_	(0.012)
RZY2	_	-0.014	_	-0.009
	_	(0.017)	_	(0.015)
RZY3	_	0.033	_	-0.021
	_	(0.044)	_	(0.013)
RZY4	_	_	_	-0.029**
	_	_	_	(0.014)
RZY5	_	_	_	-0.016
	_	_	_	(0.014)
RZY6	_	_	_	-0.028*
	_	_	_	(0.016)
RZY7	_	_	_	0.113*
	_	_	_	(0.063)
R2	0.000	0.0002	0.0001	0.0002
Observations	28791	28791	184794	184794

Table 3.40

Wage Effect Estimation for Small Firms: Random Growth Rate Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	-0.020	–	-0.065***	–
	(0.024)	–	(0.023)	–
RZY1	–	0.002	–	0.017
	–	(0.020)	–	(0.020)
RZY2	–	0.032	–	0.048***
	–	(0.023)	–	(0.018)
RZY3	–	-0.045	–	0.034*
	–	(0.051)	–	(0.020)
RZY4	–	–	–	0.000
	–	–	–	(0.020)
RZY5	–	–	–	0.040*
	–	–	–	(0.021)
RZY6	–	–	–	0.038*
	–	–	–	(0.021)
RZY7	–	–	–	-0.205**
	–	–	–	(0.090)
R2	0.0001	0.0002	0.0001	0.0003
Observations	28016	28016	184794	184794

Table 3.41

Wage Effect Estimation for Small Firms: Lagged Dependent Variable Model

	Comparison1 (2nd Round RZ)		Comparison2 (Propensity Score)	
RZ	0.013	_	-0.015	_
	(0.013)	_	(0.014)	_
RZY1	_	-0.021	_	-0.005
	_	(0.014)	_	(0.015)
RZY2	_	0.008	_	0.025
	_	(0.014)	_	(0.015)
RZY3	_	0.022	_	0.013
	_	(0.018)	_	(0.016)
RZY4	_	_	_	-0.014
	_	_	_	(0.016)
RZY5	_	_	_	0.011
	_	_	_	(0.016)
RZY6	_	_	_	0.012
	_	_	_	(0.017)
RZY7	_	_	_	-0.046*
	_	_	_	(0.024)
R2	0.137	0.1374	0.146	0.1461
Observations	25566	25566	166916	166916

CHAPTER IV

THE EFFECT OF THE RENAISSANCE ZONE ON FIRMS' LIFE DURATION

1. Introduction

Since job creation is one of the most important goals of RZ or EZ programs, most previous studies have mainly focused on examining the programs' employment impact. Most of these studies, however, found that EZ programs didn't exert anticipated (positive) impacts on employment. The insignificant employment effect of EZ programs can be caused by many factors. One reason, in addition to the substitution effect, might be due to the redistribution of jobs from affluent to distressed areas, or worse yet, from distressed areas to distressed areas. In other words, as EZ promotes more employment in zones, it also "steals" employment away from surrounding areas. Hence, it is necessary to go beyond the analysis of employment effects and to study some other outcomes of the businesses and residents in order to gain a comprehensive understanding of the effectiveness of RZ or EZ programs.

To complement the common employment impact studies, this chapter analyzes the effect of RZ programs on the duration of new firms. This line of study is important in two aspects. First, firms' life duration affects the employment stability and security of the regional labor market and second, firms' duration affects the future tax revenue buoyancy and recovery rate of local areas especially if the community has invested substantial funds into attracting these businesses.

This study examines and tests the hypothesis that the firms in zones live longer under RZ programs. The hypothesis is based on the fact that businesses obtain various benefits from being located in the RZ. A direct benefit to the firms is the increase in business profit from zone tax incentives. An indirect benefit may be derived from the agglomeration effect since zone incentives will attract more similar industries to zone areas. The clustering of firms will, in turn, reduce input costs, particularly input costs in the areas of technology and information. It is thus hypothesized that these benefits will make the firms more competitive and last longer under zone incentives.

This chapter uses the same comparison groups as the preceding chapter to maintain consistency in study and evaluation. Comparison1 is composed of the firms in 2^{nd} round Renaissance zones. Comparison2 is selected through the propensity score method. These two comparison groups were used to deal with the differences in observed selection characteristics. In addition, the Difference-in-Difference method is used here to address the unobserved selection problems.

Like the study on employment and wage effects in the last chapter, this study focuses on not only the overall effect of RZ programs, but also examines the existence and magnitude of RZ program effects on firms' life duration in different industries and in firms of different sizes. The RZ program may have a different impact on the cost structure of firms in different industries and of different sizes.

2. Duration Models and the Difference-in-Difference Method

2.1 Duration Model

Following Kiefer (1988) and Greene (2000), let T be the duration of new firms with density function $f(t)$, where t is a realization of T. The cumulative probability is

$$F(t) = \int_0^t f(t)ds = \text{prob}\ (T \leq t)$$

The survival function, which is the probability that the length of the firm's life is at least t, is expressed as:

$$S(t) = 1 - F(t) = \text{prob}\ (T \geq t)$$

In practice, the hazard rate is more interesting. It is:

$$\lambda(t) = \lim_{\Delta t \to 0+} \frac{P(t \leq T \leq t + \Delta t \mid T \geq t)}{\Delta t} = \frac{f(t)}{S(t)}$$

where $\lambda(t)$ is the hazard rate, and density function $f(t)$ is usually specified as in an Exponential, Weibull, Lognormal, or Log-logistic distribution. For some distributions, the log likelihood function when the sample includes censored observations is formulated as:

$$\ln L = \sum_{uncensoredobservation} \lambda(t|\theta) + \sum_{allobservation} \ln S(t|\theta)$$

where $\theta = (\lambda, p)$, θ is a vector of a location parameter λ and scale parameter p to be estimated, $\lambda(t) = \lambda$ for exponential distribution, $\lambda(t) = \lambda p(\lambda)^{p-1}$ for Weibull distribution. Suppose that each life spell has an associated set of time invariant covariates X. Take the Weibull distribution as an example, $\lambda_i = e^{-\beta' x}$ and i is individual observation. This is

usually called the accelerated failure-time (AFT) model. Another useful hazard rate specification is the Proportional Hazard Model proposed by Cox (1972):

$$\lambda_i = e^{\beta' x} \lambda_0(t)$$

Where $\lambda_0(t)$ is baseline hazard function for X=0. The Cox model provides estimates of β but provides no direct estimates of $\lambda_0(t)$.

A simple way to choose an appropriate hazard function is to plot the estimated hazard function. The parameterized hazard function for Exponential or Weibull distribution exhibits a monotonic increasing or decreasing pattern, while the Cox proportional hazard function can be any shape. The first step of this study is to choose the right hazard function for estimations.

2.2 The Difference-in-Difference Method

The central function of the Difference-in-Difference method is to determine whether any of the pre-to-post-designation differences between the zones and non-zones are significant. This method nets out all extraneous factors that have impacts on the dependent variables of both the test and control groups. Based on the Difference-in-Difference method and a hazard function specified from the above alternative distributions, X is defined as a set of dummy variables. In AFT and Cox proportional hazard models,

$$\beta' X = \beta_1 \, NZA + \beta_2 \, ZA + \beta_3 \, ZB$$

where *ZB* is a dummy variable equal to 1 if the firm is in the RZ before zone assignment and 0 otherwise; *ZA* is a dummy equal to 1 if the firm is in the RZ after zone assignment and 0 otherwise; and *NZA* is a dummy equal to 1 if the firm is not in the RZ after zone assignment and 0 otherwise. The reference group is the firms that are not in the RZ before zone assignment. These three dummies separate all observations into four groups as shown in the following:

	Before Zone Designation	After Zone Designation
Zone	ZB (1,0)	ZA (1,0)
Non-zone	reference	NZA (1,0)

The coefficient β_1 on *NZA* represents the difference of before and after zone designation for firms not in zones. The difference in the coefficients ($\beta_2 - \beta_3$) of *ZB* and *ZA* is the change accrued to in-zone firms before and after zone designation. The difference between these two differences (β_1, ($\beta_2 - \beta_3$)) is the difference-in-difference estimate of the RZ effect.

To examine the effect of the RZ on firms' life duration in different industries and in different sizes of firms, the same set of dummies is used for the manufacturing firm sub-dataset and service firm sub-dataset, and also for the large firm sub-dataset and small firm sub-dataset. Manufacturing and service firms, large and small firms are defined the same way as in Chapter III.

2.3 Firms' Duration Calculation

ES202 does not give ready information on firms' inception and termination. However, the life duration of each firm can be calculated by tracking its ID, and Predecessor and Successor numbers. A new ID number means a new firm opening up. A firm is deemed closed down when its ID number disappeared and it couldn't be linked to any Successor number. ES202 keeps its record up to 6 quarters if a firm stops to report unemployment insurance reimbursement activities. If the firms' reported employment of the last several periods are all zeros then the life duration of the firm does not count for those time periods.

Finally, the dataset for duration analysis is right censored. All firms with nonzero employment in the last quarter are censored by assuming that they remain operating at that time.

3. Empirical Results

To analyze the basic characteristics of the duration data, I first plot the estimated hazard functions for all firms taken together, manufacturing and service firms, and large and small firms separately, by zone status for comparison1 and comparison2 in Figures 4.1, 4.2, 4.3, 4.4 and 4.5. As in Chapter3, comaprison1 has a shorter period than comparison2 because the 2nd round Renaissance Zone became effective after Q1:2000 and can't be used as a comparison group thereafter. With the exception of the hazard function for manufacturing firms and large firms located within the zone areas when the

comparison group is comparison1, the hazard function for all other firms have a similar shape. That is, the hazard rate first rises and peaks after two and a half years, and then starts to decline gradually, then after 8-10 years operation drops sharply. For the manufacturing firms located inside zone areas when comparison1 is used, Figure 4.2 shows that the shape of the hazard function is an interesting inverse "W". For the large firms in zone areas when comparison 1 is used, the hazard rate rises continuously to the highest point at 30 quarters and then drops. This pattern of initially increasing and then decreasing hazard rate that is exhibited in the vast majority of cases suggests that neither Exponential nor Weibull distribution is the appropriate specification for estimation because their distributions exhibit monotonic increasing or decreasing patterns. The Cox proportional hazard model therefore appears to be a better candidate for estimation. The most important assumption of the Cox proportional hazard model is that the hazard ratio is proportional over time. To evaluate the validity of this assumption, –ln(–ln(survival)) curves for zone and non-zone firms versus ln(analysis time) are plotted. If the plotted lines are parallel between firms in zones and firms not in zones, the proportional hazard assumption is not violated. Figures 4.6, 4.7, 4.8, 4.9, and 4.10 reveal that hazard lines of firms in zone and non-zone areas are reasonably parallel in all cases. This implies that the Cox proportional hazards function is an appropriate model to use for firm life duration analysis.

Therefore, Cox proportional hazards model is estimated for all firms and for each specific industry and firm size. Another reason for using the Cox model is that theoretically, semi-parametric Cox estimates are less restrictive or more robust than the parametric models. Following each estimation, the Difference-in-Difference equations are used to test the significance of the impact of zone designation on firm's life duration. To make valid the statistical inference about the coefficient estimates, this paper uses robust standard errors instead of standard errors in estimations. The robust method uses the efficient score residuals for each of the subjects in the data to calculate the variance-covariance matrix instead of the conventional inverse-matrix of negative-second-derivatives method. The robust calculation accounts for the risk that the same subjects appear repeatedly in simple single-record, single-failure survival data.

Table 4.1 displays the duration estimates of all firms taken together for both comparison1 and comparison2. Overall, for all firms, the negative coefficients of *ZA* indicate that the firms in the RZ tend to survive longer after zone designation on January 1, 1997 than before this date. The estimation results are similar for out-of-zone firms. This is true for both comparison1 and comparison2. Both findings provide evidence of the positive influence of the business cycle, when the whole economy was booming after 1997.

To examine if the RZ designation has a significantly different impact on the firms in zones and out of zones, the first difference and second difference are tested based on the null hypothesis:

$$\beta_1=0; \quad \beta_2 = \beta_3; \quad \beta_1 = \beta_2 - \beta_3;$$

The test results are shown in Table 4.2, based on duration model estimates. For all firms taken together in both zones and non-zones, their life durations are significantly different before and after zone designation. However, all Difference-in-Difference tests represented by *NZA=ZA-ZB* equation in Table 4.2 are not significant. This suggests that RZ program policy did not generate a significant difference in the lifetime of firms in zones and out of zones. The results from comparison1 and comparison2 are consistent with each other. In other words, the results show that RZ programs are not effective in strengthening the life duration of firms in zones by using either comparison group.

The above results are visibly shown in Figure 4.11. In both of the comparison1 and comparison2 cases, for Cox regressions of all firms, both the estimated hazard functions of zone-after and non-zone-after firms overlap and lie above the hazard functions of zone-before and non-zone-before firms, which overlap with each other as well. This means that the declining change in the hazard function of non-zone firms before and after zone designation is equal to that in the hazard function of zone firms.

Turing to manufacturing firms, Table 4.3 gives the duration estimation results in comparison1 and comparison2 groups. Generally, the significant and negative

coefficients of *ZA* and *NZA* in the Cox model imply the same influence of the business cycle as mentioned in the previous case of all firms. Table 4.4 provides the test statistics for manufacturing firms. Difference-in-Difference tests in the *NZA=ZA-ZB* equation also show that RZ programs do not have any beneficial impact on the life of manufacturing firms in zones, whichever comparison group is used. The same conclusion can be made by looking into the graph of the estimated hazard functions of manufacturing firms, as shown in Figure 4.12. In the comparison2 regression of Figure 4.12, it looks like the hazard function of zone manufacturing firms before and after zone designation declines more than the hazard function of non-zone manufacturing firms. But, the difference in magnitude of the hazard rate change is still relatively small and insignificant.

Table 4.5 provides proportional hazards model estimation results for service firms. In both comparison1 and comparison2 cases, the coefficients of *ZA*, *ZB* and *NZA* exhibit similar characteristics to those coefficients from estimations of all firms taken together and of manufacturing firms. Difference-in-Difference tests in Table 4.6 show that in the case of comparison1, β_1 is significantly different from (β_2 - β_3) at the 5% level. This means the RZ program has a significant effect on the duration of the service industry. The subsequent question is whether the effect is positive or negative. In the service firm case, β_1= -4.819 and β_2 - β_3= -3.048. Thus, β_1 – (β_2 - β_3) = -1.771. This negative difference means that the decrease in the hazard rate of non-zone firms before and after zone designation is larger than that of zone firms before and after zone designation. In other

words, RZ programs in net terms reduce the duration of service firms in zones after zone designation, comparing to the service firms in non-zones after zone designation. However, when looking at the graph (Figure 4.13) the dynamic changes of hazard functions for service firms in the case of comparison1, it's hard to tell if there really exists the negative effect because the estimated hazard functions of zone-after and zone-before lie across the hazard functions of non-zone-after and non-zone-before. There is no similar finding for the comparison2 case. Both Difference-in-Difference tests in Table 4.6 and the graphical change of the hazard function in Figure 4.13 for comparison2 do not show any significant effect of the RZ program on service firms.

Table 4.7 shows Cox proportional model estimations for large firms in both comparison1 and comparison2 cases. The coefficients of all dummies exhibit similar characteristics to those obtained from estimations for all firms taken together, and for manufacturing firms and service firms as well. In both cases, Difference-in-Difference tests in Table 4.8 show that the RZ program does not lengthen the firm's life duration. The dynamic changes of hazard functions for zone and non-zone firms in Figure 4.14 support the insignificant duration effect of the RZ program.

Turning to small firms, we get the same conclusions. Tables 4.9 and 4.10 provide Cox model estimations and Difference-in-Difference tests for small firms. In both comparison1 and comparison2 cases, the results are similar to those for all firms taken together, and for manufacturing and small firms as well. There is no life duration effect to

be found for small firms in both cases. Consistently, Figure 4.15 also shows that hazard functions do not significantly change due to the RZ program.

In sum, in both comparison1 and comparion2 cases, this study finds that RZ programs have no significant effect on the life duration of all firms taken together, of manufacturing firms, of large firms, or of small firms located in the zones. For service firms, in the comparison2 case, RZ programs also appear to have no significant effect on the life duration of service firms. However, in the comparison1 case, RZ policies appear to decrease the life duration of service firms in zones. This finding is not very strong because it only exists in one case and the effect is significant at the 5% level. To understand this seemingly puzzling finding, more theoretical exploration and empirical studies are needed. Actually, Greenbaum and Engberg (1998), in their urban enterprise zone evaluation study, find that "capital spending grew faster in the comparison areas than in the zones both in new and ongoing manufacturing establishments." Their finding might give some possible hints to explain the results of this study. The real reason for this anomaly in the study may be unveiled with further analysis.

4. Conclusions

This study combines two comparison groups with the duration models to evaluate the effect of Renaissance Zone policy on firms' life duration in the State of Michigan. The unique ES202 data permit this study to examine the effect of urban RZ at the firm level by the Difference-in-Difference method to eliminate the unobserved selection

problems. The comparison1 group selects the 2^{nd} round Renaissance Zones as the comparison group. The comparison2 forms the control group by selecting the comparable firms through propensity score methods. This study examines not only the overall firms' life duration effect, but also the life durations for manufacturing firms, service firms, large firms, and small firms separately.

Initial analysis of firm life duration is conducted by plotting the estimated hazard functions for all firms, and for manufacturing firms, service firms, large firms and small firms. Then the Cox proportional hazards model is estimated and Difference-in-Difference tests are conducted. Estimation results and Difference-in-Difference tests in most cases show that RZ programs have no significant effects on the lifetime of firms overall, of manufacture firms, of service firms, of large firms and of small firms. However, using comparison1 data, this study finds that RZ policies have a negative effect on the life duration of service firms.

Table 4.1

Cox Proportional Model Estimations for All Firms Together

Independent Variables	Comparison1	Comparison2
ZA	-3.084***	-2.416***
	(0.214)	(0.099)
ZB	-0.024	-0.033
	(0.170)	(0.052)
NZA	-3.216***	-2.452***
	(0.252)	(0.063)
Log Pseudolikelihood	-5256.43	-45284.26

Robust errors are in parentheses.

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 4.2

$\chi 2$ and P-value of Difference-in-Difference Tests
for All Firms Taken Together

Tests	Comparison1	Comparison2
NZA=0	162.41***	1498.89***
	(0.000)	(0.000)
ZA=ZB	706.42***	902.17***
	(0.000)	(0.000)
NZA=ZA-ZB	0.37	0.00
	(0.543)	(0.968)

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.
P-values are in parentheses.

Table 4.3

Cox Proportional Model Estimations for Manufacturing Firms

Independent Variables	Comparison1	Comparison2
ZA	-3.554***	-3.059***
	(0.469)	(0.240)
ZB	-0.030	0.207
	(0.349)	(0.186)
NZA	-3.086***	-3.221***
	(0.556)	(0.186)
Log Pseudolikelihood	-490.49	-2032.33

Robust errors are in parentheses.
*** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 4.4

$\chi 2$ and P-value of Difference-in-Difference Tests
for Manufacturing Firms

Tests	Comparison1	Comparison2
NZA=0	30.84***	301.47***
	(0.000)	(0.000)
ZA=ZB	134.77***	200.02***
	(0.000)	(0.000)
NZA=ZA-ZB	0.60	0.05
	(0.439)	(0.827)

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.
P-values are in parentheses.

Table 4.5

Cox Proportional Model Estimations for Service Firms

Independent Variables	Comparison1	Comparison2
ZA	-2.755***	-2.351***
	(0.270)	(0.102)
ZB	0.293	0.054
	(0.242)	(0.067)
NZA	-4.819***	-2.432***
	(0.758)	(0.088)
Log Pseudolikelihood	-1702.36	-17616.16

Robust errors are in parentheses.
*** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 4.6

$\chi 2$ and P-value of Difference-in-Difference Tests
for Service Firms

Tests	Comparison1	Comparison2
NZA=0	40.47***	759.94***
	(0.000)	(0.000)
ZA=ZB	602.37***	705.07***
	(0.000)	(0.000)
NZA=ZA-ZB	5.39**	0.08
	(0.02)	(0.773)

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.
P-values are in parentheses.

Table 4.7

Cox Proportional Model Estimations for Large Firms

Independent Variables	Comparison1	Comparison2
ZA	-4.231***	-3.141***
	(0.403)	(0.191)
ZB	-0.308	0.192
	(0.262)	(0.117)
NZA	-3.501***	-3.259***
	(0.565)	(0.191)
Log Pseudolikelihood	-423.77	-3194.35

Robust errors are in parentheses.
*** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 4.8

$\chi 2$ and P-value of Difference-in-Difference Tests for Large Firms

Tests	Comparison1	Comparison2
NZA=0	86.30***	289.78***
	(0.000)	(0.000)
ZA=ZB	741.96***	308.01***
	(0.000)	(0.000)
NZA=ZA-ZB	0.42	0.16
	(0.517)	(0.693)

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.
P-values are in parentheses.

Table 4.9

Cox Proportional Model Estimations for Small Firms

Independent Variables	Comparison1	Comparison2
ZA	-2.949***	-2.337***
	(0.225)	(0.103)
ZB	-0.007	-0.022
	(0.182)	(0.051)
NZA	-3.432***	-2.367***
	(0.342)	(0.060)
Log Pseudolikelihood	-4514.03	40266.48

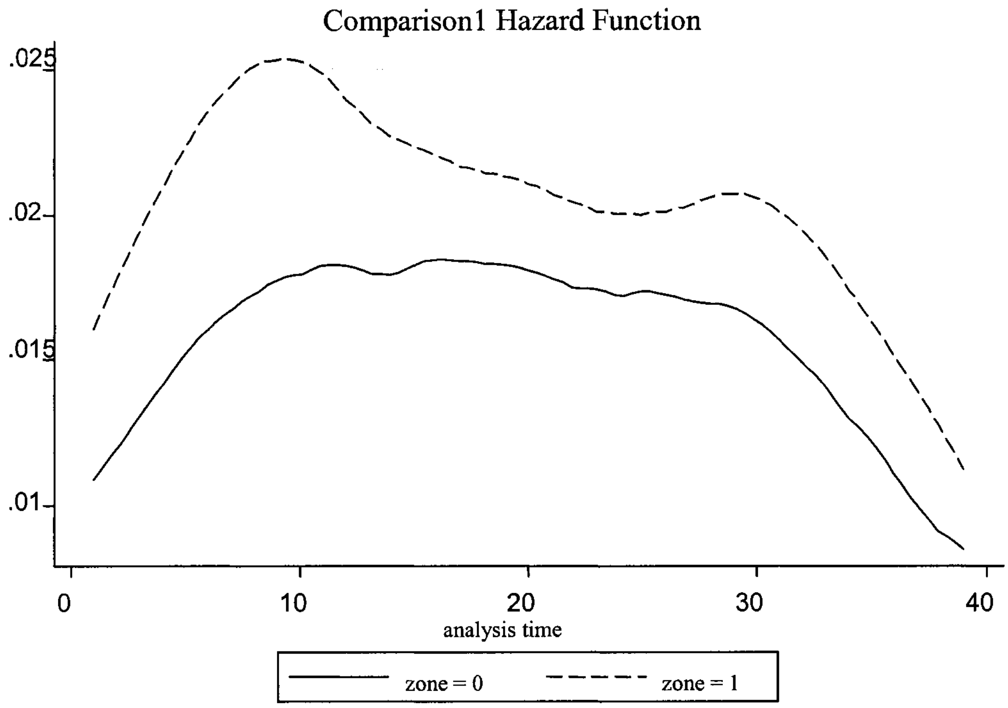
Robust errors are in parentheses.
*** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 4.10

$\chi 2$ and P-value of Difference-in-Difference Tests for Small Firms

Tests	Comparison1	Comparison2
NZA=0	100.99***	1556.54***
	(0.000)	(0.000)
ZA=ZB	598.88***	715.57***
	(0.000)	(0.000)
NZA=ZA-ZB	2.07	0.01
	(0.149)	(0.932)

*** significant at 1% level; ** significant at 5% level; * significant at 10% level.
P-values are in parentheses.



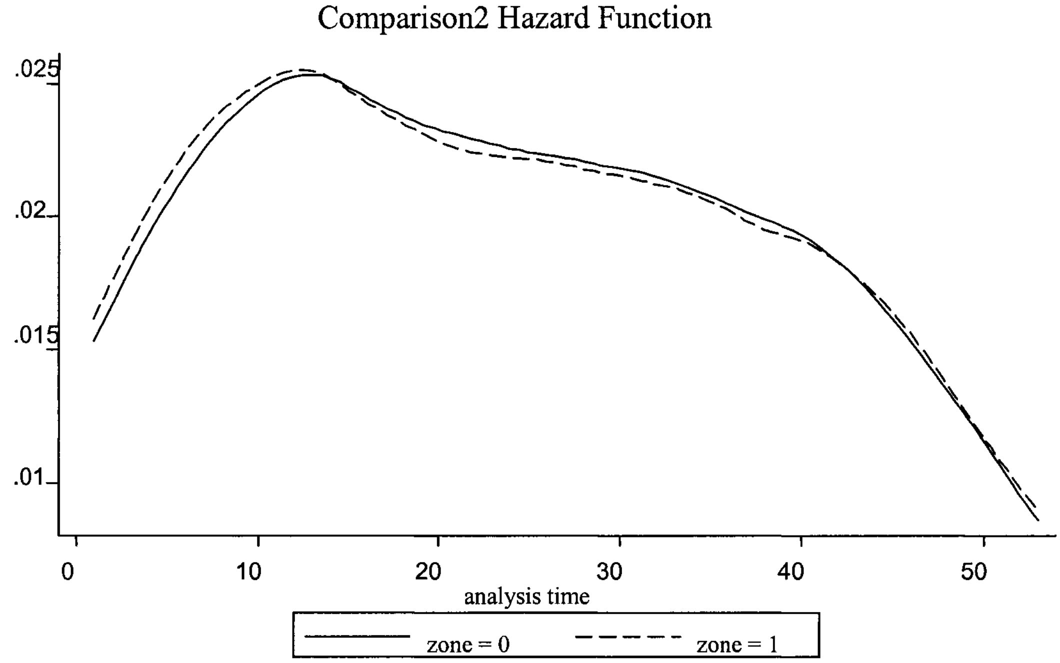


Figure 4.1 Estimated Hazard Functions for All Firms.

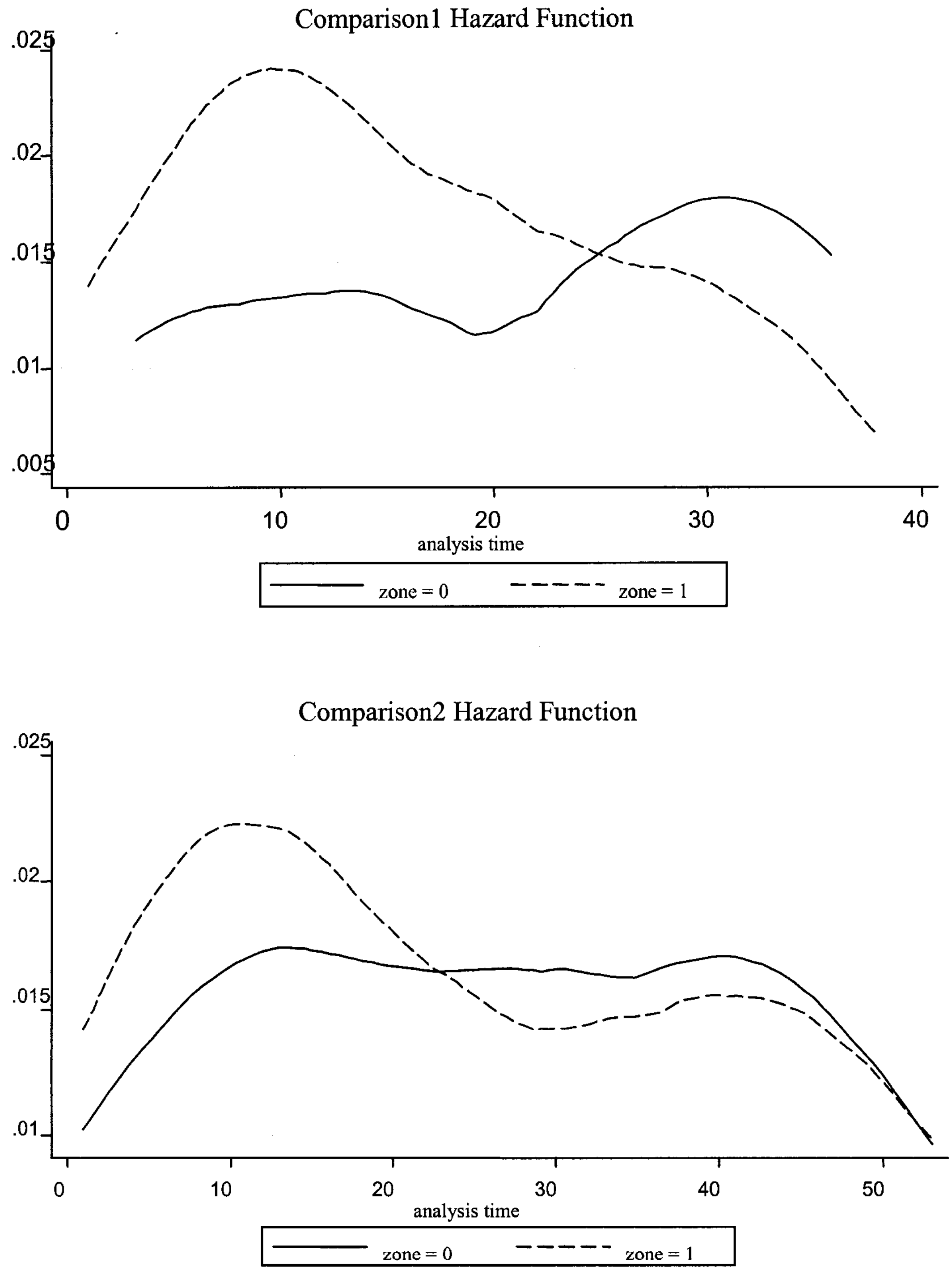
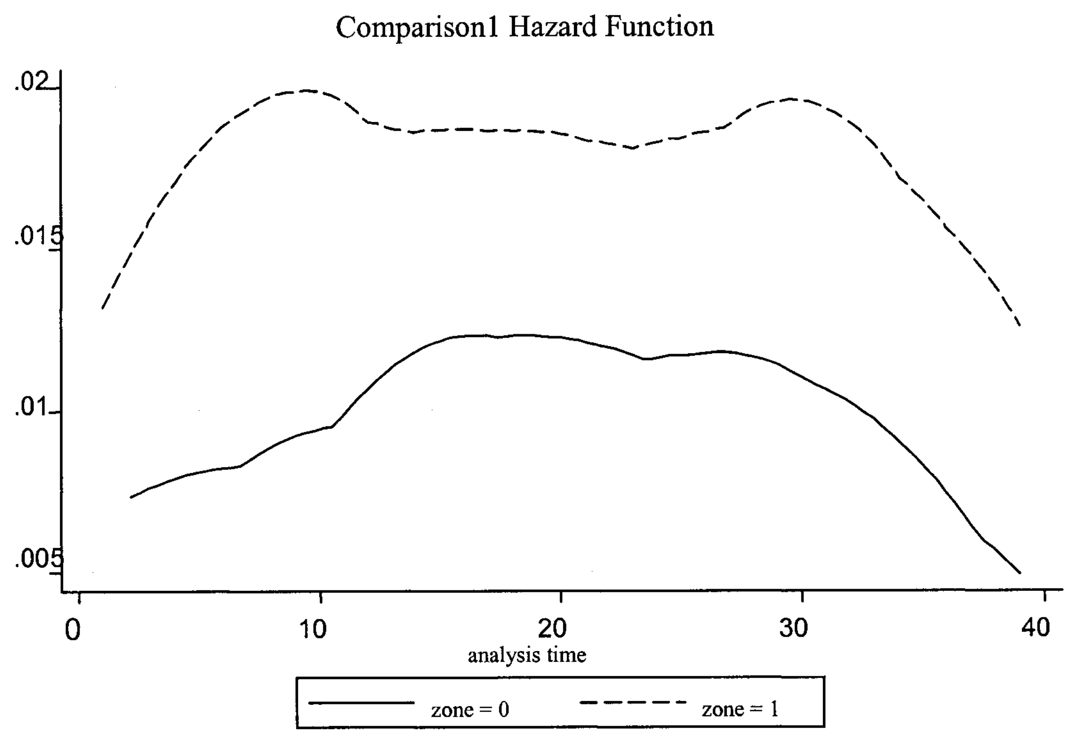


Figure 4.2 Estimated Hazard Functions for Manufacturing Firms.



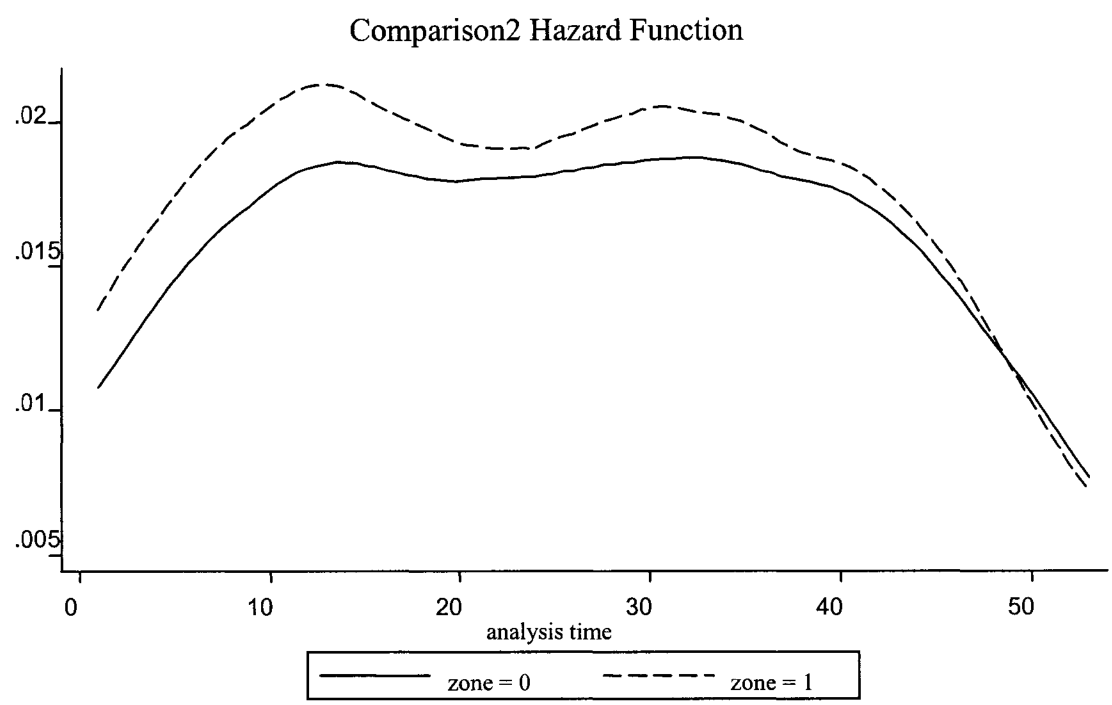
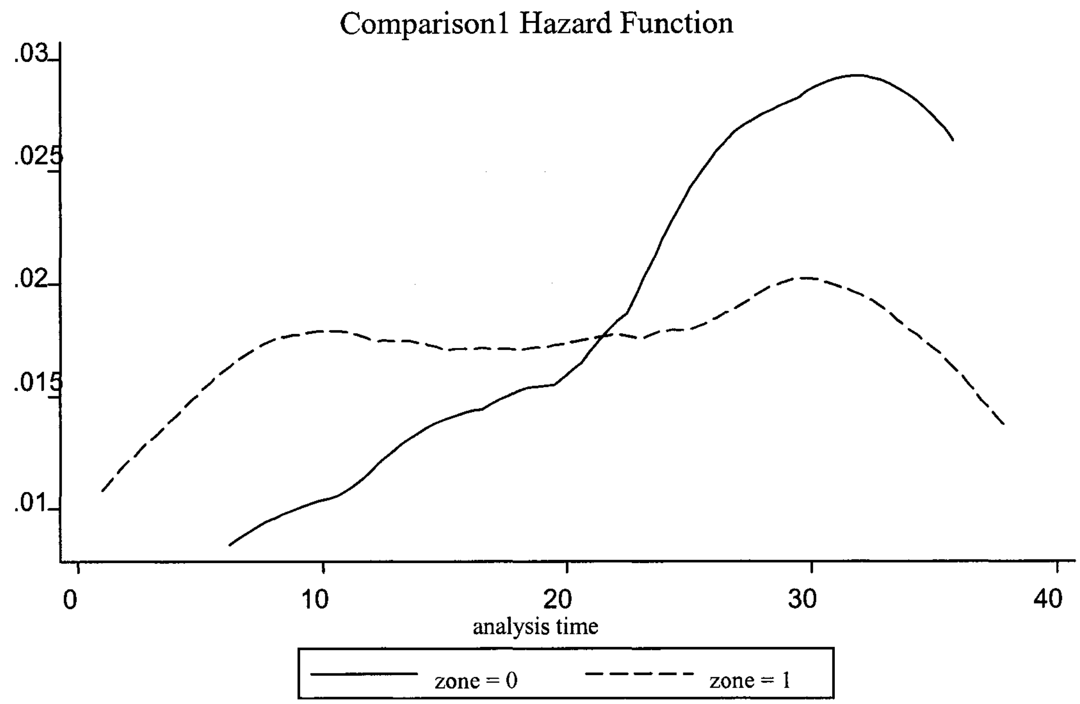


Figure 4.3 Estimated Hazard Functions for Service Firms.



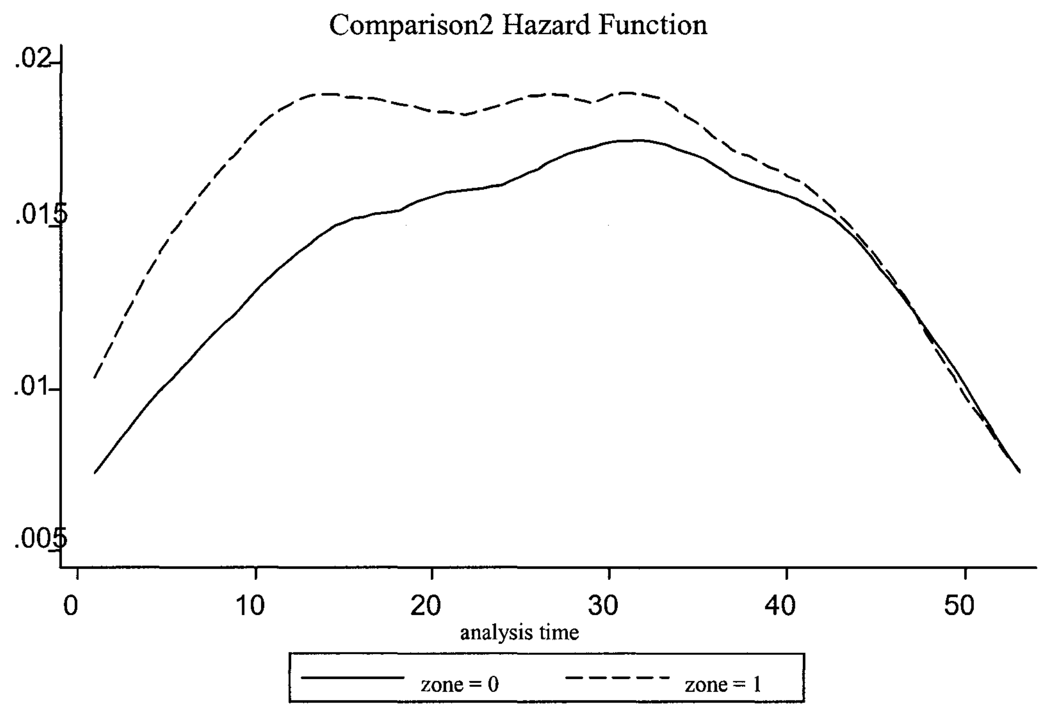
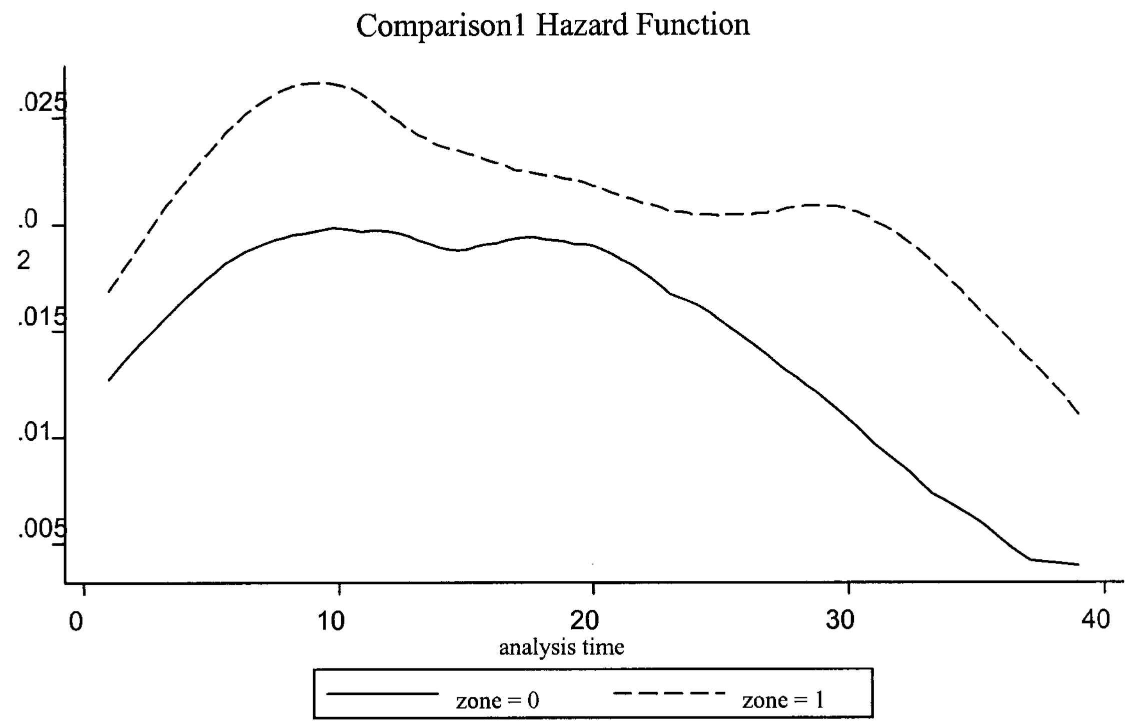


Figure 4.4 Estimated Hazard Functions for Large Firms.



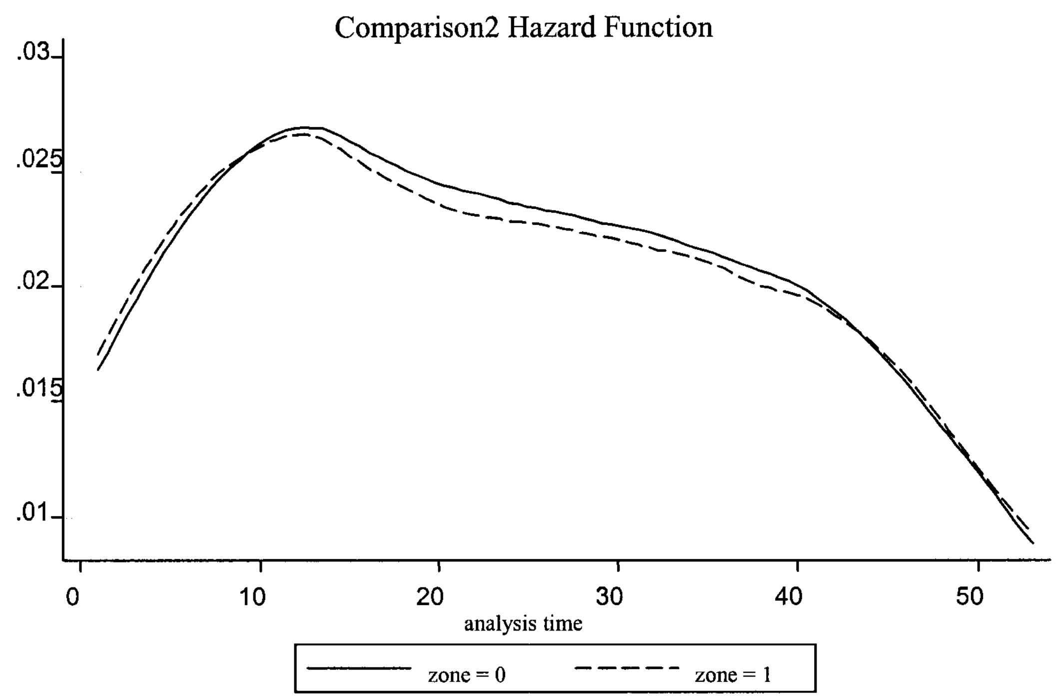
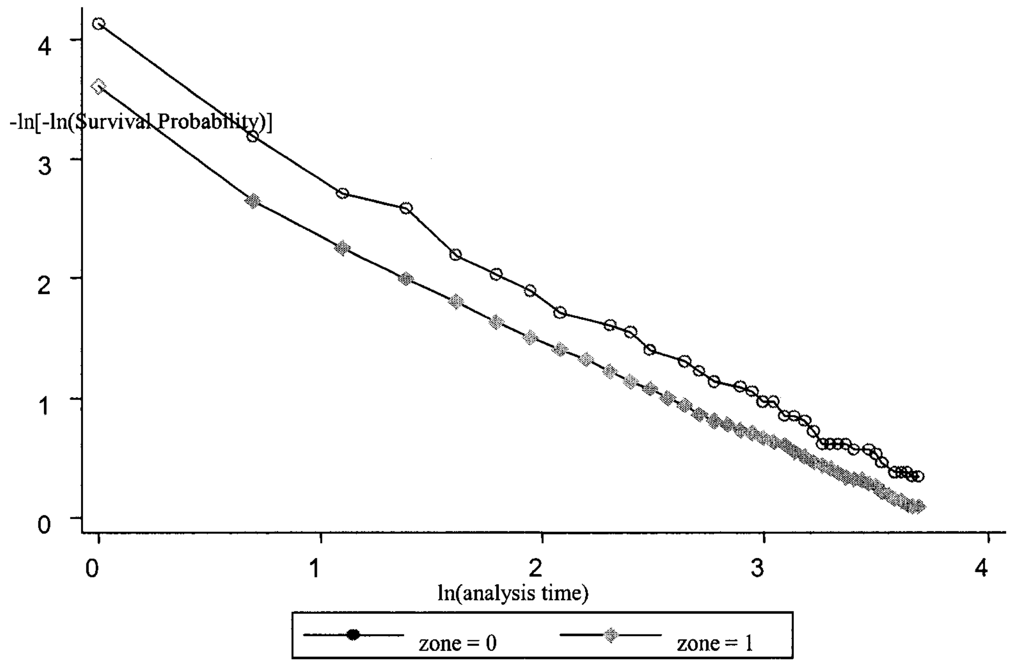


Figure 4.5 Estimated Hazard Functions for Small Firms.

Comparison1



Comparison2

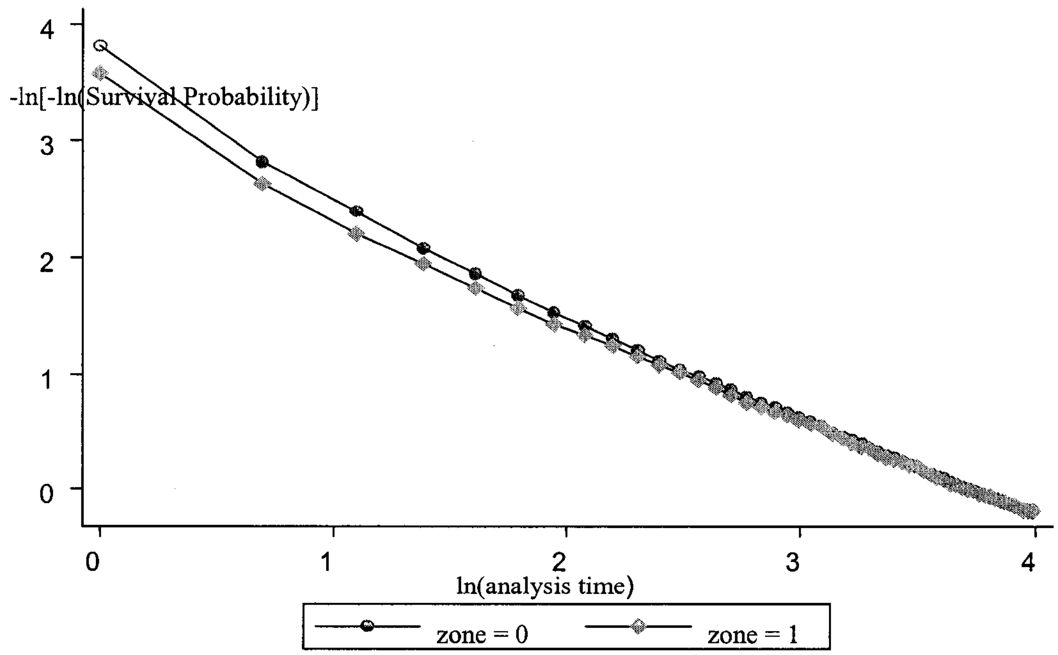
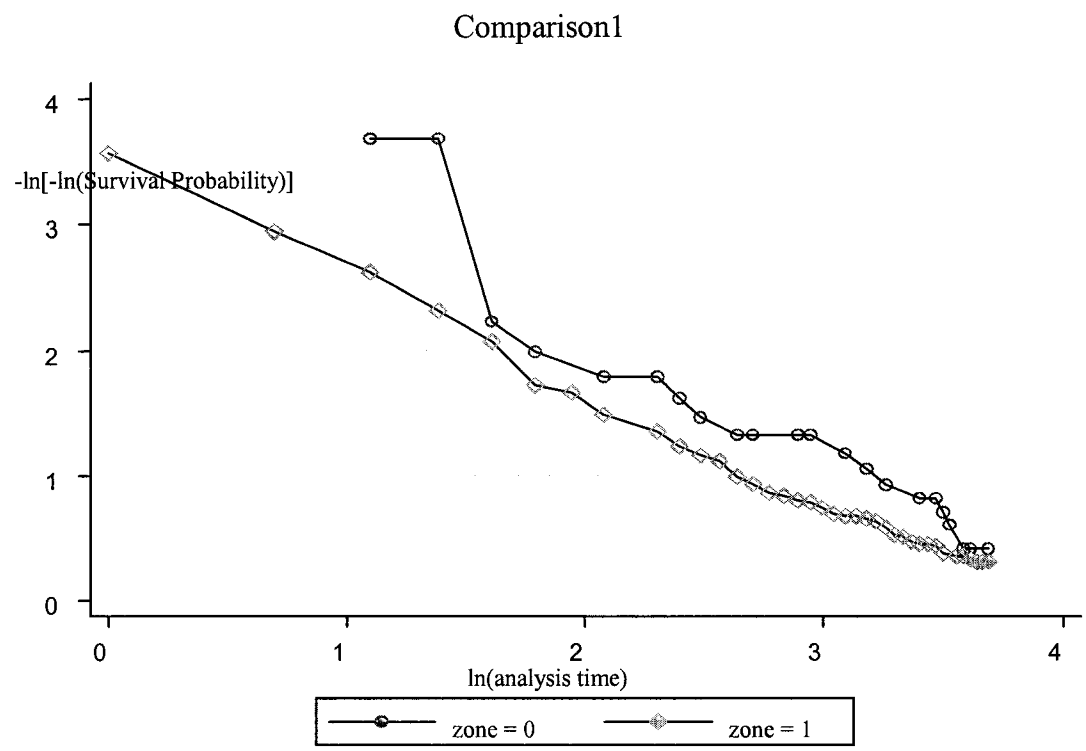


Figure 4.6 Graphical Assessment of the Cox Proportional Hazards Assumption for All Firms.



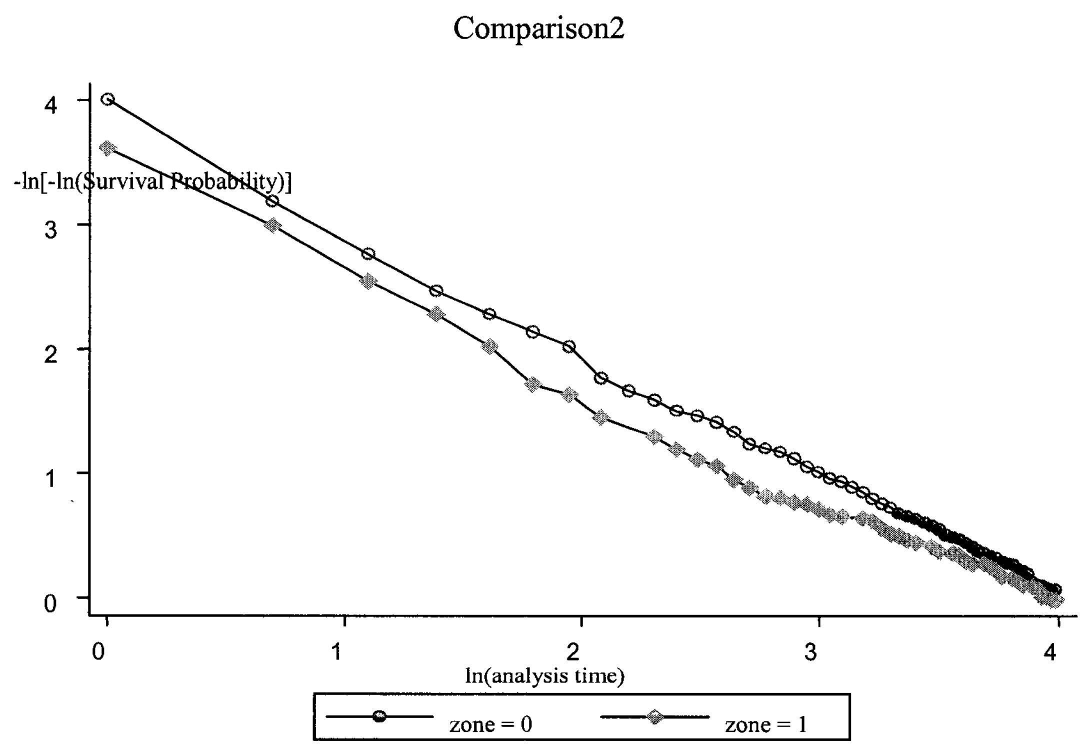
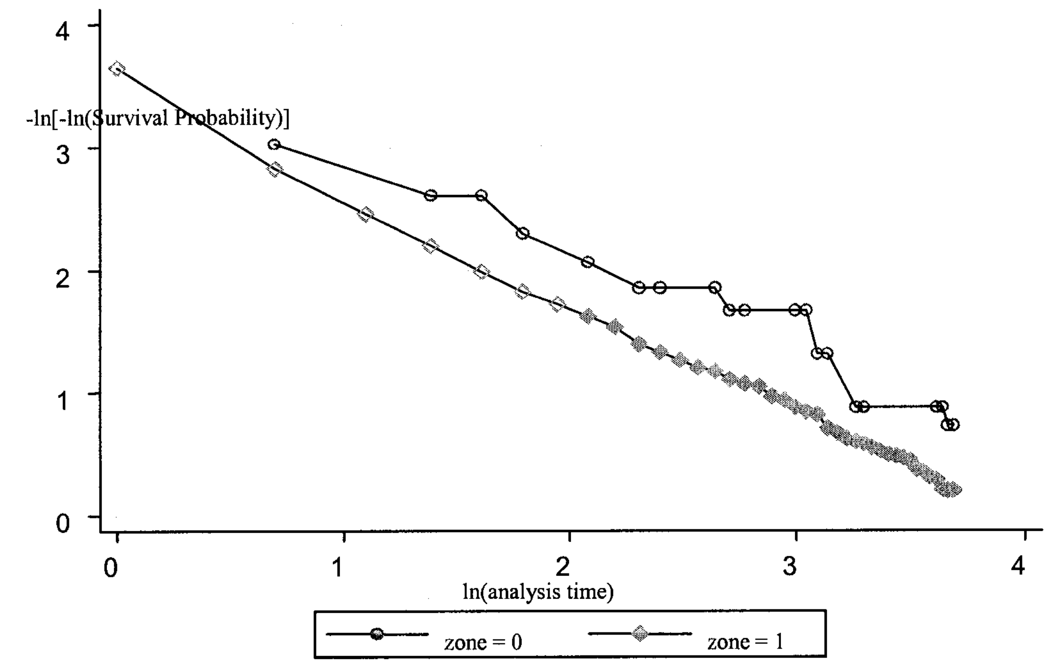


Figure 4.7 Graphical Assessment of the Cox Proportional Hazards Assumption for Manufacturing Firms.

Comparison1



Comparison2

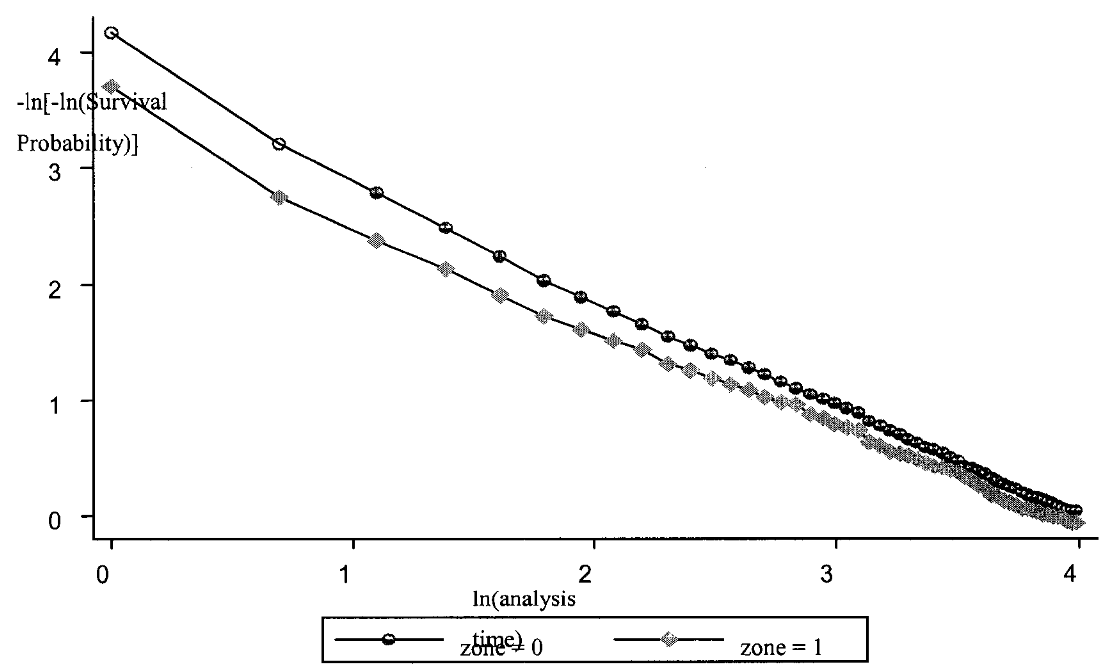
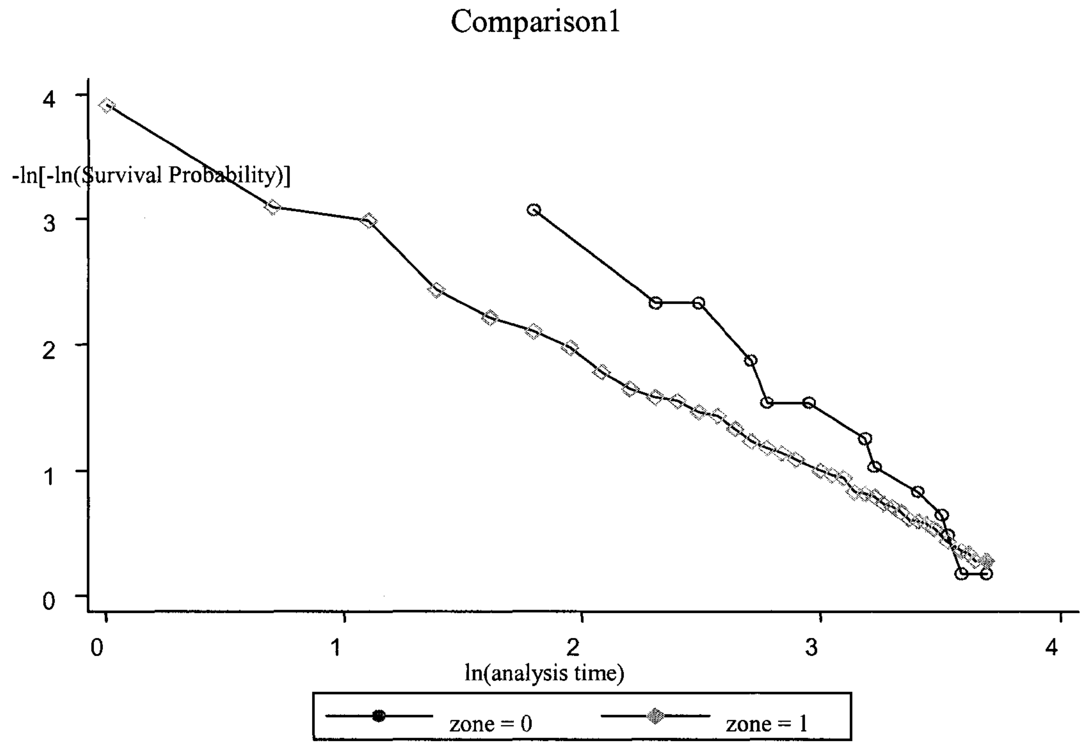


Figure 4.8 Graphical Assessment of the Cox Proportional Hazards Assumption for Service Firms.



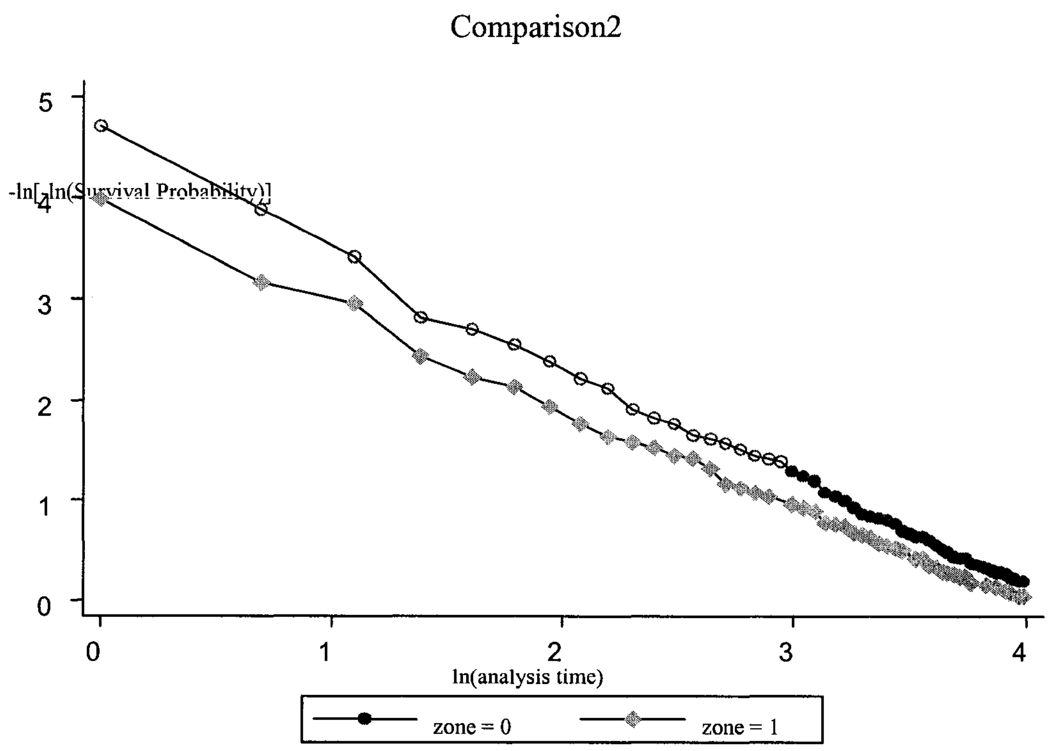


Figure 4.9 Graphical Assessment of the Cox Proportional Hazards Assumption for Large Firms.

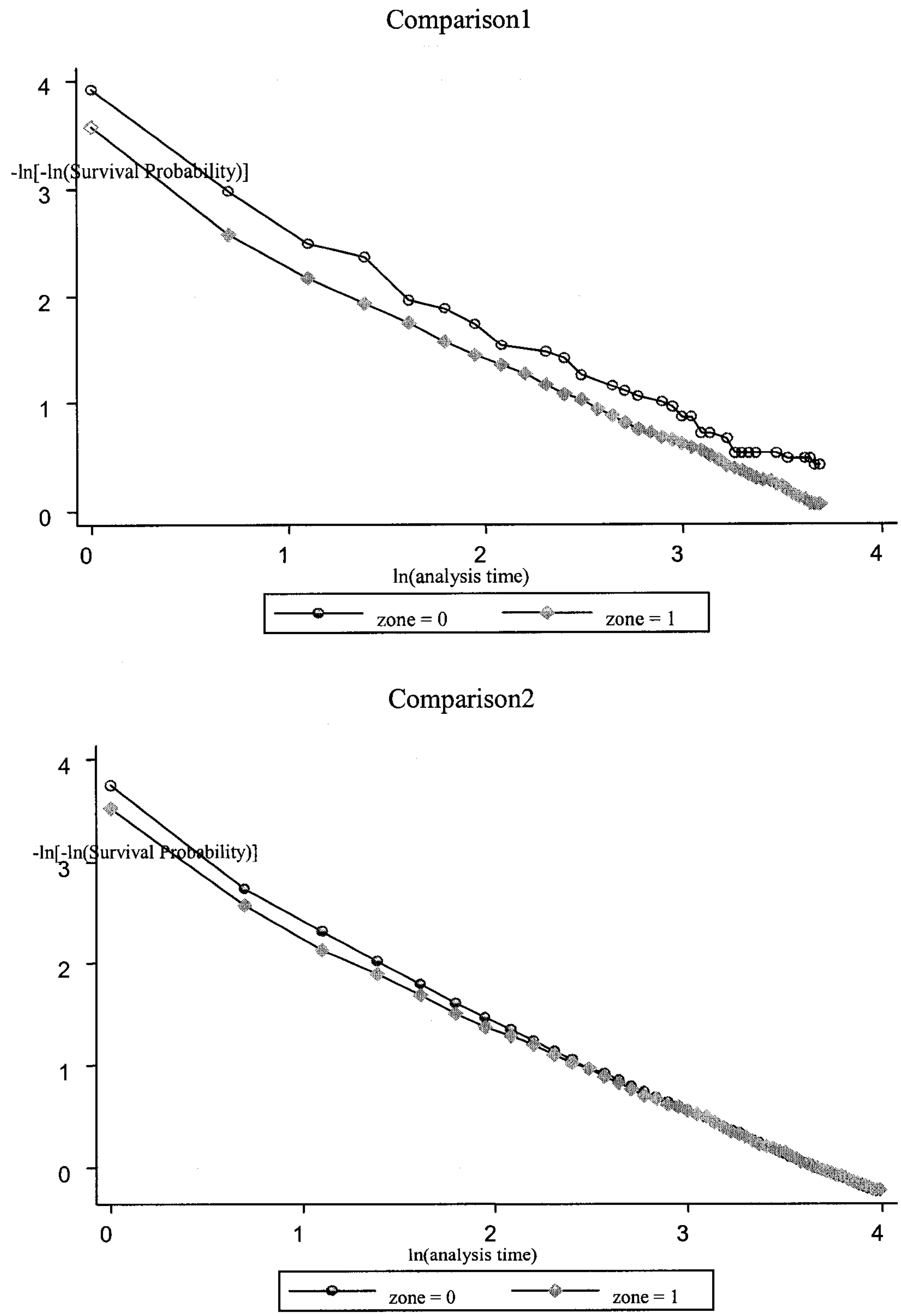


Figure 4.10 Graphical Assessment of the Cox Proportional Hazards Assumption for Small Firms.

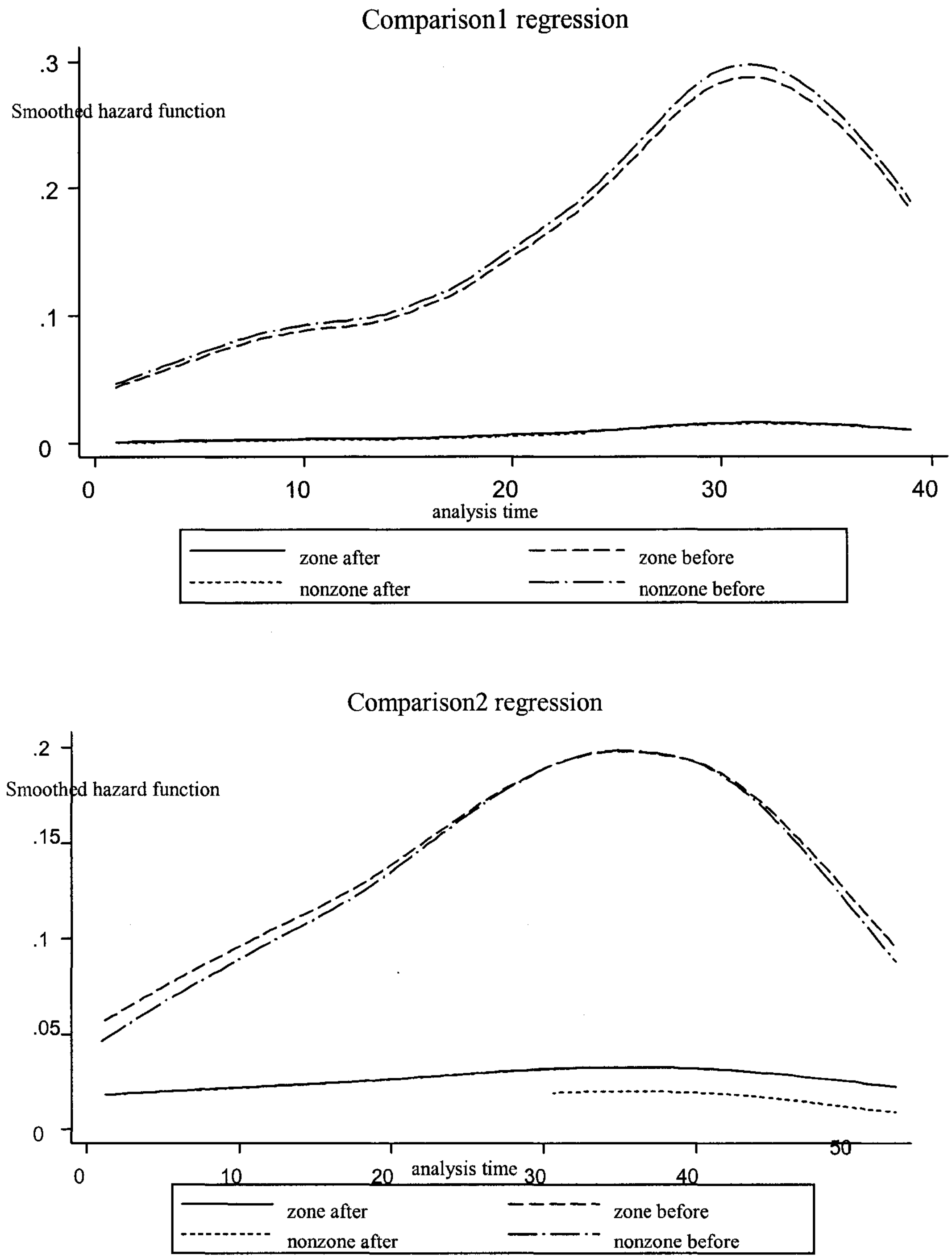


Figure 4.11 Dynamic Changes of Cox Proportional Hazard Functions for All Firms.

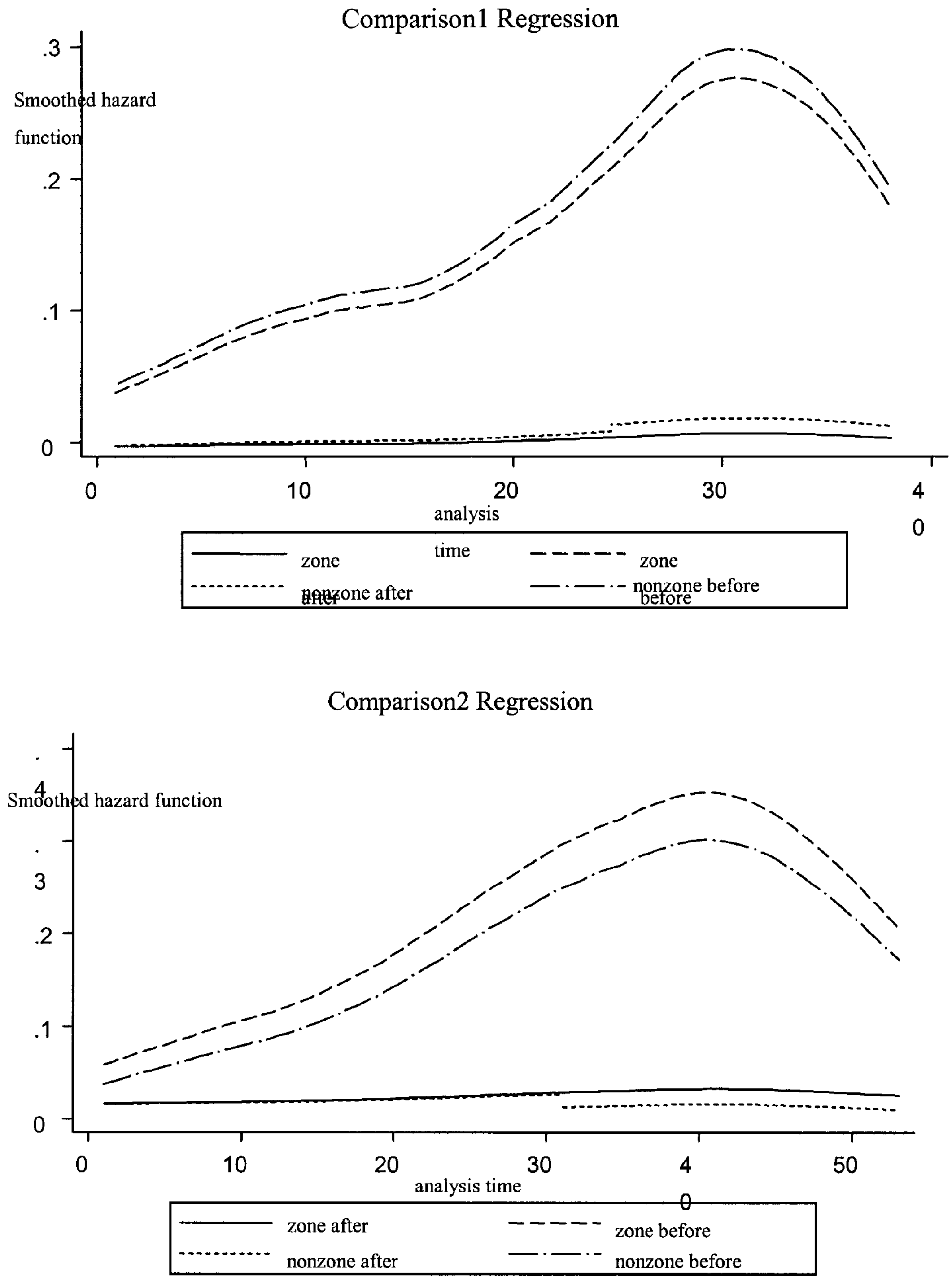
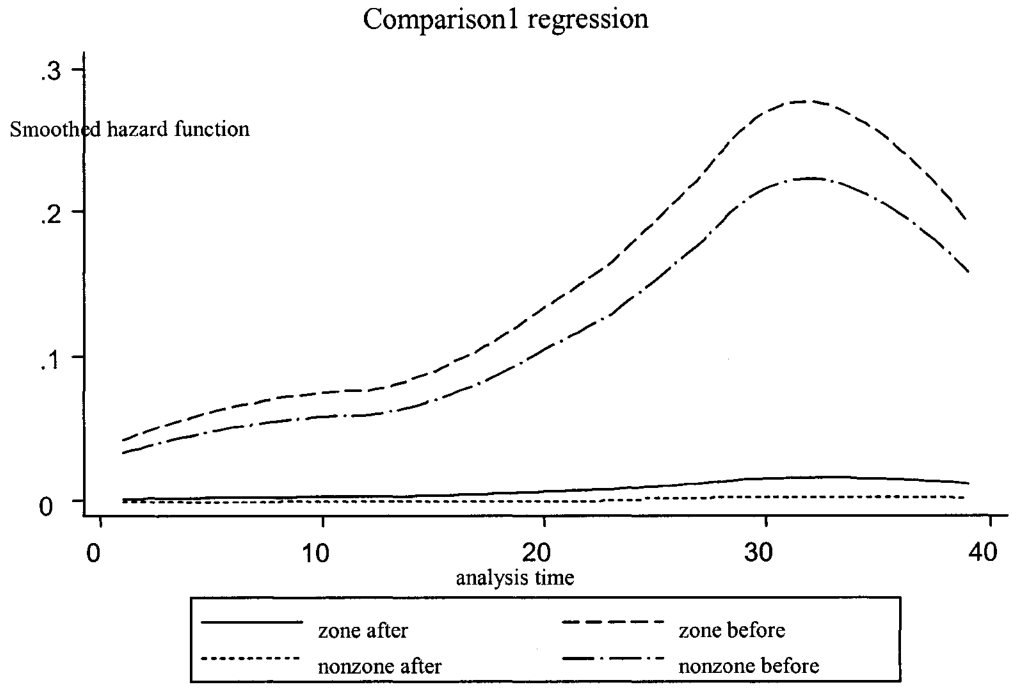


Figure 4.12 Dynamic Changes of Cox Proportional Hazard Functions for Manufacturing Firms.



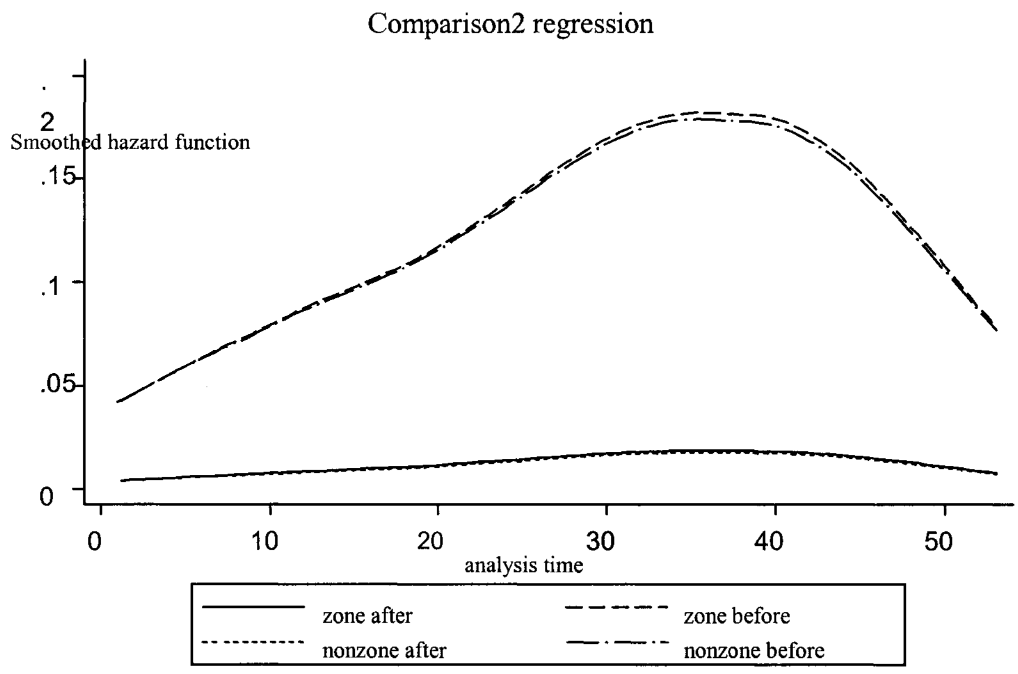
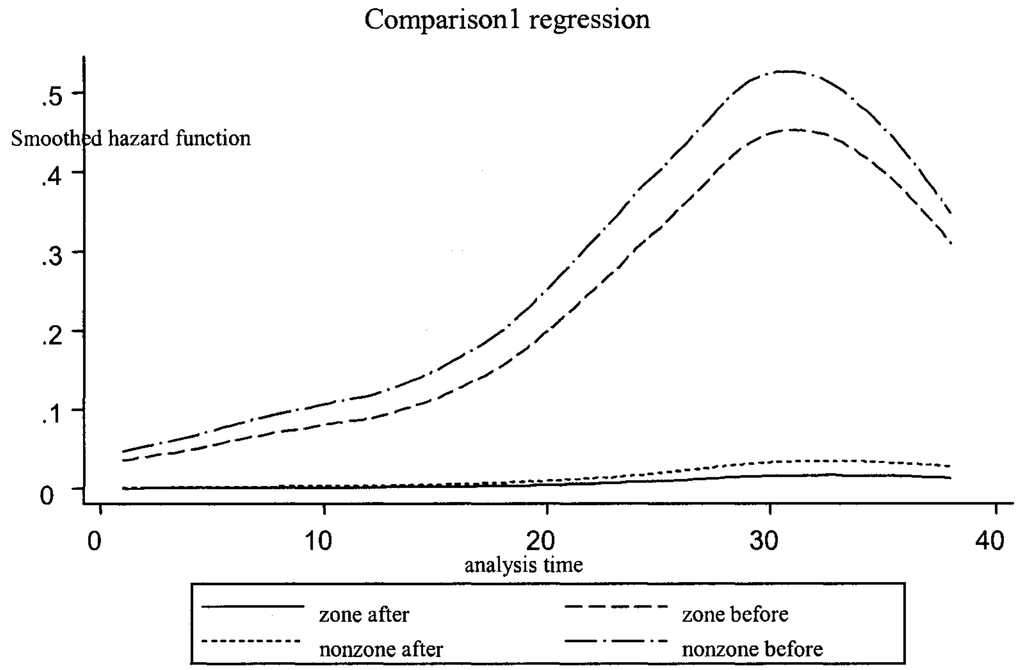


Figure 4.13 Dynamic Changes of Cox Proportional Hazard Functions for Service Firms.



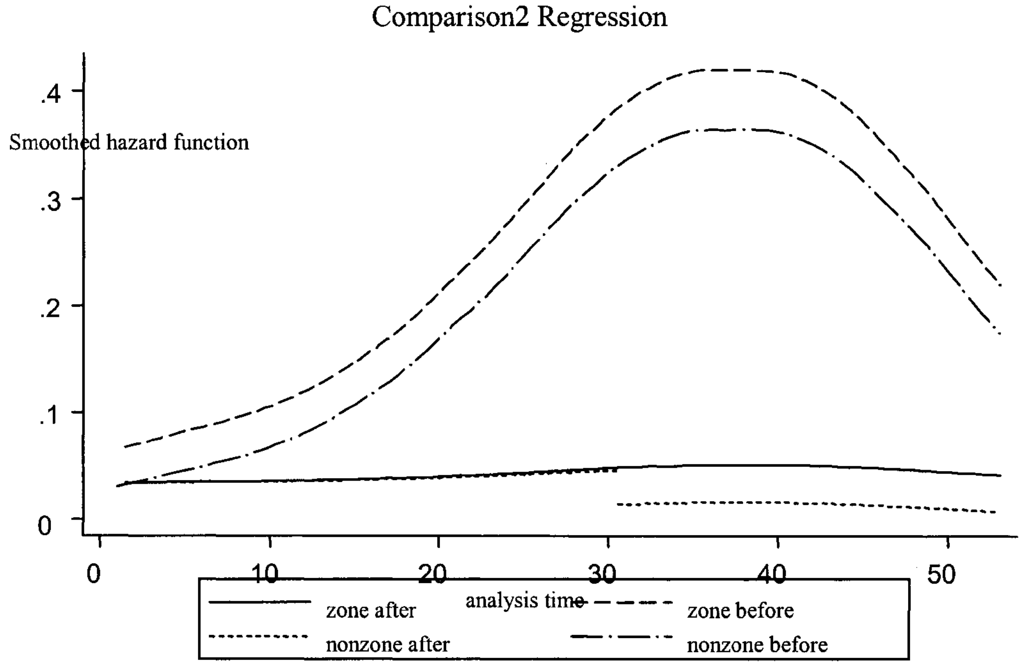
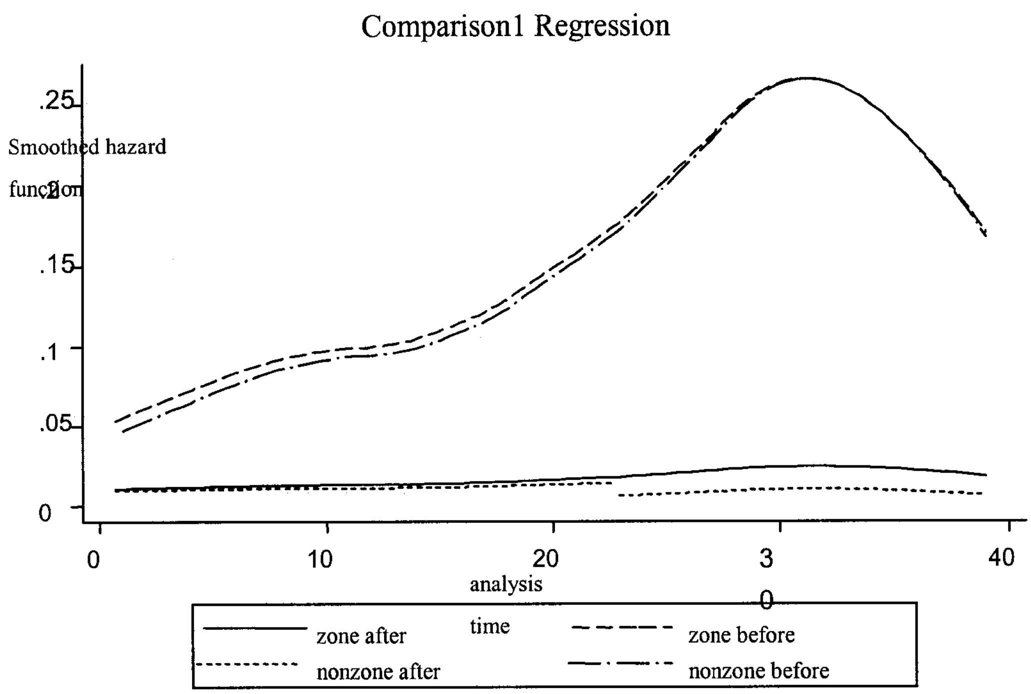


Figure 4.14 Dynamic Changes of Cox Proportional Hazard Functions for Large Firms.



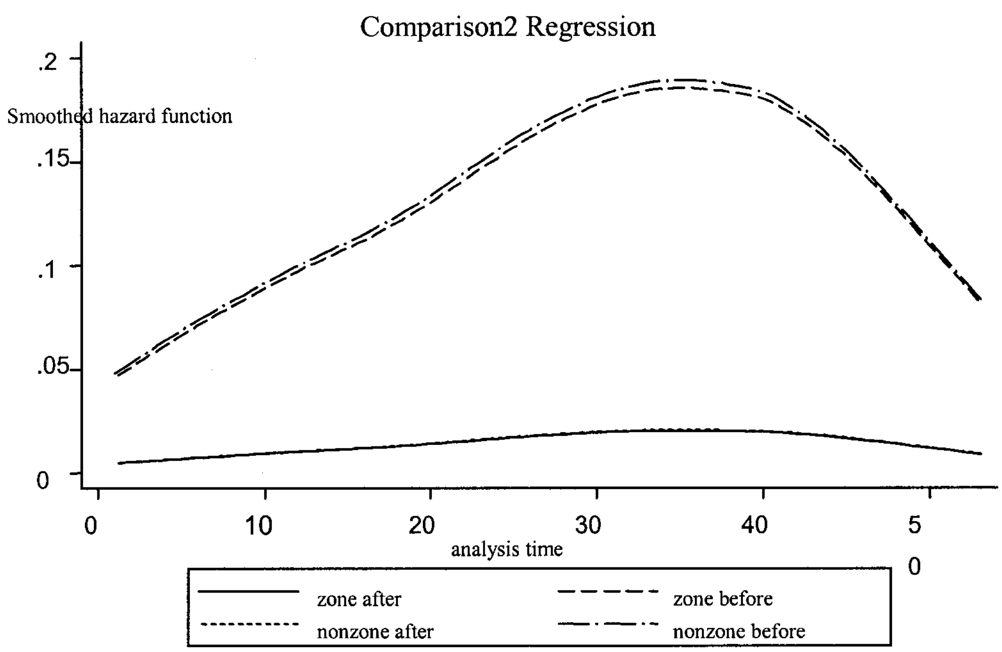


Figure 4.15 Dynamic Changes of Cox Proportional Hazard Functions for Small Firms.

CHAPTER V

SUMMARY, IMPLICATIONS AND FUTURE WORK

1. Summary

It has been nine years since the first round of the "Michigan Renaissance Zone" program went into effect on January 1, 1997. Basically, the Renaissance Zone program takes tax incentives as the major tool to develop and uplift distressed areas by way of stimulating economic growth, lowering unemployment and poverty rates, and boosting income and housing values. Most state and local taxes are waived for businesses and residents in zone areas. Six urban zones, three rural zones, and two ex-military facilities were selected as the first round Renaissance Zones; four urban zones, four rural zones and one ex-military facility were selected as the second round Renaissance Zones. The average duration of the tax incentives is 12-15 years.

Comparing Michigan's Renaissance Zones (RZ) to Enterprise Zones (EZ) commonly used in most other states, are quite similar in the central goal and nature of programs. Michigan's RZ is more aggressive and extensive in that it covers both businesses and residents. Most of the studies on enterprise zones found that the EZ program didn't help create jobs. Research also found that EZ programs are not effective in affecting other economic outcomes as well. Unlike the numerous studies on enterprise zone programs, only a few studies examine the economic or social impact of Renaissance Zones. Sands (2003) and the Citizens Research Council of Michigan (1998) found that Renaissance Zones created jobs and investments. Their studies, however, are basically

survey data summaries and the evaluation periods are short. Thus, a more comprehensive and rigorous evaluation of the RZ program is needed.

This study is to determine whether the RZ program is effective in helping distressed urban areas. The unique ES202 data permits us not only to use establishment level data to examine the impact of the RZ program on firms' behaviors, but also to aggregate over firm level data into zip level data to examine the impact of the RZ program on the employment source. Chapter III examined firms' employment and establishment number effect of the RZ program by using zip level data for new firms, deceased firms, and existing firms. Chapter III also examined firms' employment and real wage effect of the RZ program by applying firm level data. Chapter IV focused on the effect of the RZ program on firms' life duration. Based upon the presumption that firms in different industries and of different sizes are sensitive to tax incentives in different ways, both Chapter III and Chapter IV examined the impact on all firms taken together, and on manufacturing and service firms, as well as on large and small firms separately.

The most commonly used evaluation method is to compare the business outcomes of zone areas due to the RZ program to those of non-zone areas or comparison areas without the RZ program. Selection biases on observed and unobserved variables usually arise from compiling data to conduct program evaluation, and they need to be addressed. To correct for the observed selection bias, this study chooses two comparison groups, the 2^{nd} round RZ (comparison1) and propensity score picked group (comparison2).

Comparison1 matches with the 1st round Renaissance Zone in the sense of meeting the same zone selection criteria and procedures. Comparison2 matched with 1st round Renaissance Zone in the sense of having comparable economic characteristics. Using these two alternative comparison groups can also test if the findings are robust and consistent with different specifications of the control group.

To remove the possible unobserved selection bias, Chapter III applies three model specifications to the estimation on unbalanced panel data: the fixed effect model, random growth rate model, and lagged dependent variable model. Difference-in-Difference tests are applied in Chapter IV to deal with unobserved selection problems. Estimation of each model and test are conducted for all firms taken together, for manufacturing and service firms, and for large and small firms, to examine the effect of the RZ program on firms in different industries and in different sizes.

By using aggregated zip level data, empirical results from Chapter III revealed that the Renaissance Zone programs don't have any impact on the employment of all firms, of new firms, dead firms, and existing firms. Another important finding on aggregated data is that RZ programs cause fewer firms to open up and fewer firms to close down in zone areas than in non-zone areas at the same time.

By using firm level data and dealing with intra group correlations in estimations, the results for all firms taken together are consistent with the results from zip level data. We also found that the Renaissance Zone programs raise employment by around 9% for

service firms and around 3.8% for small firms. For manufacturing firms and large firms, the employment effect of RZ programs is not significant. There is evidence showing that the employment effect of RZ programs changes over time for service, large and small firms.

Contrary to its mostly positive employment effect, the RZ program causes a drop in the real wage by 10.8% for manufacturing firms, 11.7% for service firms, and 6.5% for small firms. The real wage effect is not significant for all firms taken together and for large firms. It is also found that real wage effects change over the time period for manufacturing firms, for service firms and for small firms. The contrary effect between employment and real wage is plausible. Free or lower taxes on businesses reduce firms' operating costs and income tax exemption on in-zone residents, raises their take-home wages, and may lower the wages they expect or ask from firms. Consequently, the firms would hire more labor since it is getting cheaper and needed for output expansion. This result implies that workers living in zone areas tend to be low-skilled and have high supply elasticity. It also implies that the RZ program seems to favor labor over capital.

That the RZ programs have a slightly larger wage impact than the employment effect is consistent with Papke's (1993) finding. Considering all cases together, the results from comparison2 are stronger than the results from comparison1 because comparison2 has longer time periods to allow the lagged effect to appear. The overall effects on service and small firms are stronger than on large and manufacturing firms.

This positive employment effect should not become a zero-sum game if the newly created jobs are low-skilled jobs with low wages.

Chapter IV examined the life duration effect of the RZ program. Based upon Cox proportional hazard function estimations and Difference-in-Difference tests, most cases show that the RZ program does not help to lengthen firms' life duration. RZ policies are found to have a negative effect on life duration of the service firms at the 10% significant level only in the case of comparison1.

2. Policy Implications

Several implications of Renaissance Zone programs can be derived from the findings given in the above summaries. These implications however, may be more pertinent to improve Michigan's Renaissance Zone program than generalizing to other states' EZ programs as this analysis is based on the Michigan case study.

In general, Renaissance Zone programs are effective in influencing the establishment number of dead firms and employment and real wage for some particular types of firms, but not as effective in lengthening firms' life. This result suggests that Renaissance Zone programs will continue to work in improving the employment situation of distressed areas in the post designation years.

The negative impact on the establishment number of new firms implies that RZ programs can combine some assistance programs for start-ups and help them counter and overcome the difficulties they face in starting establishments. Even though the RZ

program is applied uniformly to all firms in different industries or in different sizes, the empirical results suggest that the program is more effective for service firms and small firms. Yet it is not effective for manufacturing firms or large firms with more than 20 employees. The Renaissance Zone program can be further improved by introducing additional incentives such as providing training programs and credit assistance to service firms and small firms. This will make the RZ program more effective to those firms than having only tax incentives available to all firms. For manufacturing firms and large firms, it will be helpful if the program focuses on labor rather capital since RZ programs seem to favor labor over capital. Doing these "custom-made" programs to different industries and firms also helps analysts better evaluate the programs since they can focus on a particular group of firms.

Comparing the impact on employment to the larger impact on wage suggests that RZ programs may place emphasis on the assistance or modifying incentives to influence the wage. This will indirectly affect employment as well.

The dynamic time pattern of employment and wage effects also suggests that the RZ program could be made more flexible for particular sub-zones in terms of stipulating duration of incentives, and tailoring local tax and credit incentives to let firms get more effective assistance when and where they indeed need it. Longer duration of incentives is always good to attract new firms and help existing firms to last longer. It is also good to

have longer observation periods for evaluation. The tax policy may vary in different areas according to their economic characteristics and industry conditions.

One difficulty of evaluation that researchers commonly encounter is the zone boundary definitions. If the zone program or local government can make the zone boundaries follow the census tracts or zip codes, it would be easier to create accurate data sets and utilize rigorous econometric evaluation techniques.

3. Future Work

There are still 3-6 years left for the 1st round zones program designation and 7-10 years left for termination of 2nd round zones. Many effects that have not appeared so far may emerge in the remaining periods. Evaluation of the 1st round programs could help to improve 2nd round programs. The ultimate goal of evaluation is to make the programs work more efficiently, to develop the distressed areas, and to enable state and local governments to collect more taxes and recover revenue losses from the break periods. Thus, continuing and consistent evaluation is needed.

Besides stimulating growth and employment, RZ programs have many other objectives to attain such as lowering the poverty rate, improving investment, increasing housing values and the social and economic environment in distressed areas. Thus, future studies are needed to ascertain if RZ programs have a real effect on these variables. The evaluation of the RZ effect on residents is also very important since, unlike other states' EZ programs, only Michigan's RZ programs give a generous tax break to residents.

After all, the most challenging part of an evaluation study is to find good data to do it. Data problems are also an important reason that many evaluations of EZ programs are not consistent and satisfactory. On the other hand, given the inherently insurmountable data problems, researchers should continue to strive to find better and creative evaluation methods that can overcome part of the data problems and obtain robust conclusions.

Another direction for future research may be to narrow the investigation to an individual sub-zone or a particular city to control for geographical heterogeneities. It is also easier to collect comprehensive data for an individual zone or a city.

REFERENCES

Baltagi, B. (2001). *Econometric Analysis of Panel Data.* New York: John Wiley & Sons.

Baltagi, B., & Chang, Y.J. (1994) "Incomplete Panels," *Journal of Econometrics* 62: 67-89.

Bartik, T.J. (1991). *Who Benefits from State and Local Economic Development Policies?* Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Boarnet, M.G., & Bogart, W.T. (1996). "Enterprise Zones and Employment: Evidence from New Jersey," *Journal of Urban Economics* 40: 198-215.

Bondonio, D., & Engberg, J. (2000). "Enterprise Zones and Local Employment: evidence from the states' programs," *Regional Science and Urban Economics* 30: 519-549.

Bratberg, E., Grasdal, A., & Risa, A.E. (2002). "Evaluation Social Policy by Experimental and Nonexperimental Methods," *Scandinavian Journal of Economics* 104(1): 147-171.

Citizens Research Council of Michigan. (1998). "Michigan Renaissance Zones in the Economic Development Context." Lansing, MI: Citizens Research Council of Michigan.

Cox, D. (1972). "Regression Models and Life Tables." *Journal of the Royal Statistical Society* Series B, 34: 187-220.

Dehejia, R.H., & Wahda, S. (1998). "Causal Effects in Non-experimental Studies: Re-evaluation the Evaluation of Training Programs," NBER working paper 6586. Cambridge, MA: National Bureau of Economic Research.

Dowall, D.E. (1996). "An Evaluation of California's Enterprise Zone Programs," *Economic Development Quarterly* 10(4): 352-368.

Elvery, J.A. (2004). "The Impact of Enterprise Zones on Resident Employment: An Evaluation of the Enterprise Zone Programs of California and Florida," (Doctoral dissertation, University of Maryland, College Park, 2004).

Engberg, J.B., & Greenbaum, R.T. (1999). "State Enterprise Zones and Local Housing Markets," *Journal of Housing Research* 10: 163-187.

Ge, W. (1995). "The Urban Enterprise Zone," *Journal of Regional Science* 35(2): 217-231.

Greene, W.H. (2000). *Econometric Analysis*. East Rutherford, NJ: Prentice Hall.

Greenbaum, R.T., & Engberg, J.B. (1998). "The Impact of State Urban Enterprise Zones on Business Outcomes," Working paper, Center for Economic Studies, Bureau of the Census.

Ham, J.C., & LaLonde, R.J. (1996). "The Effect of Sample Selection and Initial Conditions in Duration Models: Evidence from Experimental Data on Training," *Econometrica* 64(1): 175-205.

Heckman, J., & Hotz, V.J. (1989). "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training," *Journal of the American Statistical Association* 84(408): 862-874.

Heckman, J., & Robb, R. (1985). "Alternative Methods for Evaluating the Impact of Interventions." In J. Heckman & B. Singer, eds., *Longitudinal Analysis of Labor Market Data* (pp. 156-245). New York: Cambridge University Press.

Heckman, J., & Smith, J. (1996). "Experimental and Non-experimental Evaluations," In Gunther Schmid, Jacqueline O'Reilly, & Klaus Schomann, eds., *International Handbook of Labour Market Policy and Evaluation* (pp. 37-88). Cheltenham, UK: Cambridge University Press.

Hsiao, C. (2003). *Analysis of Panel Data*. New York: Cambridge University Press.

Kiefer, N.M. (1988). "Economic Duration Data and Hazard Functions," *Journal of Economic Literature* (June): 646-679.

LaLonde, R.J. (1986). "Evaluation the Econometric Evaluation of Training Programs with Experience Data," *American Economic Review* 76(4): 604-620.

Lambert, T.E., & Coomes, P.A. (2001). "An Evaluation of the Effectiveness of Louisville's Enterprise Zone," *Economic Development Quarterly* 15(2): 168-180.

Michigan Renaissance Zone Act. (1996). PA 376 of 1996.

Moore, W.S. (2001). "The Economic Effects of Enterprise Zone Programs on Local Labor Markets: Two Essays." (Doctoral dissertation, Syracuse University, New York.)

Netzer, D. (1997). "Discussion of Fisher and Peters 'Tax and Spending Incentives and Enterprise Zones.'" *New England Economics Review* (March/April): 131-135.

Nissen, B. (1994). "Enterprise Zones as an Economic Development Tool: the Indiana Experience," NBER working paper No. 3945. Cambridge, MA: National Bureau of Economic Research.

O'Keefe, S. (2004). "Job Creation in California's Enterprise Zones: a comparison using a propensity score matching model," *Journal of Urban Economics* 55: 131-150.

Papke, L.E. (1993). "What Do We Know About Enterprise Zones?" NBER working paper No. 4251. Cambridge, MA: National Bureau of Economic Research.

———. (1994). "Tax Policy and Urban Development: evidence from the Indiana enterprise zone program," *Journal of Public Economics* 54: 37-49.

Peters, A.H., & Fisher, P.S. (2002). *State Enterprise Zone Programs: Have They Worked?* Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Rosenbaum, P.R., & Rubin, D. B. (1983). "The Central Role of the Propensity Score in Observational Studies for Causal Effects," *Biometrika* 70(1): 41-55.

Rubin, M. (1990). "Urban Enterprise Zones: Do They Work? Evidence from New Jersey," *Public Budgeting & Finance* (Winter): 3-17.

Sands, G. (2003). "Michigan's Renaissance Zones: eliminating taxes to attract investment and jobs in distressed communities," *Environment and Planning C: Government and Policy* 21: 719-734.

Sridhar, K.S. (1996). "Tax Costs and Employment Benefits of Enterprise Zones," *Economic Development Quarterly* 10(1): 69-90.

U.S. Department of Housing and Urban Development. (1986). *State Designated Enterprise Zones: Ten Cases Studies*. Washington, DC: HUD.

Wilder, M.G., & Rubin, B.M. (1996). "Rhetoric versus Reality," *APA Journal* (Autumn): 437-489.