ABSTRACT

Title of Document: **IMPACTS OF AN INCENTIVE BASED LAND USE POLICY: AN EVALUATION OF PRESERVATION EASEMENTS.**

Charles Anthony Towe,
Doctor of Philosophy, 2007

Directed By: **Nancy Bockstael**
Professor,
Agricultural and Resource Economics

This dissertation examines the conversion decision of a landowner from an undeveloped or agricultural use to a subdivision in the presence of an active housing market and an active land preservation program. It utilizes a unique panel dataset and incorporates a real options framework to evaluate the impacts of housing market volatility on conversion timing. At the same time, this work evaluates the impact of a preservation program on the timing of conversion. Typical program evaluation of this type focuses on quantity or quality of acres enrolled. This work focuses on the timing of the conversion decision and involves a potential program benefit that is not related to enrollment in the preservation program itself. The benefit of a delayed conversion decision is a desirable outcome for the county even if parcels ultimately convert to a developed state. Hazard models are estimated which account for multiple exit states, i.e. competing risks, of conversion or preservation and correlation among these competing risks is modeled. Results of these models suggest that price volatility, as well as eligibility for the preservation program, significantly delays conversion decisions. The median estimated delay induced by easement eligibility ranges from 7 years to over 20 years depending on parcel size.

However, enrollment in a preservation easement may impact neighboring land use decisions in the presence of spillover effects. That is, an enrolled parcel may attract development in the sense that neighboring parcels become more likely to convert. A propensity score matching procedure is utilized to quantify the spillover effect of preservation activity on future surrounding land conversion decisions. The propensity score estimation approach allows a semi-parametric estimate which controls the non-random selection or endogeneity of preservation activity. Results of this model suggest that parcels neighboring recent preservation activity are almost three times more likely to convert than similar parcels without a newly preserved neighbor.

IMPACTS OF AN INCENTIVE BASED LAND USE POLICY: AN EVALUATION OF PRESERVATION EASEMENTS.

By

Charles Anthony Towe.

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2007

Advisory Committee:
Professor Nancy E. Bockstael, Chair
Professor Marc L. Nerlove
Professor Erik Lichtenberg
Associate Professor Lori Lynch
Professor Gerrit Knaap

Dedication

To Karine, Henri, Oliver, and Luca

Acknowledgements

For the encouragement to return to school, the financial support throughout the most difficult years of this dissertation, and for being a caring and loving mother to our two sons Henri and Oliver, born while completing this work, I want to thank first and foremost to my wife Karine. I am indebted to my father for ingraining in me the work ethic and persistence necessary to complete this marathon effort. I also owe a debt of gratitude for his continual effort to accumulate savings despite what were most assuredly some stretched financial times with three young children. Without this help I would have experienced a vastly different path in my educational and life endeavors. I thank my mother for her optimism and guidance at critical points in my life, from the encouragement to attend graduate school, and her to the assistance at the birth of both my sons.

I also want to thank my advisor, Nancy Bockstael, who exemplifies the model attributes of advisor and colleague that I shall strive to emulate as my career progresses. Professor Bockstael is a detailed and demanding researcher, intuitive, encouraging, and insightful, without her this research would not have been possible. I hope she enjoys her newfound retirement but does not mind a few minor interruptions. Thanks to Marc Nerlove for helpful comments to improve this analysis. I would also like to thank Lori Lynch, Erik Lichtenberg, and Gerrit Knaap for serving on my committee despite their many other commitments.

Finally, for the lively conversations, reality breaks, study help, and friendship I would like to acknowledge my fellow graduate students with whom I shared this graduate school experience. I thank Constant, Heather, and Christina.

Table of Contents

List of Tables

List of Figures

1. Introduction and Policy Environment

There has been much discussion about the pace of land use change in recent decades especially in ex-urban areas of the country.[1] In many areas land use change has taken the form of forest or farmland converting to low density residential use. According to a recent government publication approximately 80% of the nearly 2 million acres of land recently used for new housing is located outside urban areas (Heimlich and Anderson, 2001). Although accurate statistics are difficult to obtain about the rate or amount of conversion in any particular area, the level of concern expressed by local governments in many states provides, at a minimum, indirect evidence for the prevalence of this land conversion issue. Many local governments have responded to this conversion activity by implementing policies to preserve land in openspace (farmland) or by enacting regulations to slow the pace of development; some have done both. Since 1988, over 53 localities have passed more than $111 billion in conservation measures and these referenda have been exceedingly popular with over 75% of such measures passing (Trust for Public Land, 2007).

Because directly regulating development is both politically and legally difficult, jurisdictions are looking toward incentive based mechanisms to manage the pace and pattern of urban growth and the conversion of agricultural land. Under one such mechanism, landowners voluntarily receive payment for agreeing to forego conversion and accept easements placed on their land. Since the first 'purchase of development rights' (PDR) program was implemented in 1974, over 53 state and

[1] Exurban areas are defined as locations outside of metropolitan areas but within their 'commuter-shed'. Virtually all of Howard County meets this definition.

local governments have collectively spent almost $3.723 billion in public funds to preserve nearly 1.67 million acres in the U.S. (American Farmland Trust, 2005). In 2002 the Federal government authorized $986 million in matching funds for farmland preservation for the 2002-2006 period. PDR programs enjoy continued taxpayer support; in 2003 alone, $700 million in state and local ballot measures were passed to provide funding for farm and ranch land protection (Trust for Public Land).

In urbanizing areas where landowners can choose to reap immediate financial windfalls through development, PDR's offer an alternative that allows them to continue farming while receiving remuneration for their development rights. Empirical studies have characterized decisions to participate in PDR programs (e.g., Nickerson, 2000; Duke, 2004), or evaluated efficiency and distributional aspects of these programs (Nickerson and Barnard, 2004; Lynch and Musser, 2001; Lynch and Lovell, 2003).

Given the significant costs involved in preserving farmland – which averages approximately $2,000 per acre nationally (American Farmland Trust) but varies greatly over regions of the country – government agencies are increasingly interested in the effectiveness of PDR programs. A couple of studies have considered the effects of preservation programs on rates of urban development and found limited evidence that these programs slow land conversion rates (Lynch and Carpenter, 2003; Lynch and Liu, 2007). Other studies suggest that PDR programs may actually hasten the development of adjacent parcels by making this land more valuable in residential use, due to a positive spillover effect (e.g., Irwin, 1998; Irwin and Bockstael, 2002; Geoghegan, Lynch, and Bucholtz, 2003; Roe, Irwin, and Murrow-Jones, 2004).

Until recently, no studies had explored the effects of the *existence* of PDR programs on land development decisions themselves. In a paper that arose from preliminary work on this dissertation (Towe, Nickerson and Bockstael, 2008), we consider the impacts of a preservation program on conversion decisions but we implement a class of models that is potentially inadequate in fully describing the conversion decision. This dissertation estimates a class of models that accounts for two primary shortfalls of the previous work, incorporation of multiple exit states and unobservable heterogeneity of landowners and land parcels.

Chapters 2-5 of this dissertation explore how the existence of an option to participate in a PDR program affects landowners' development decisions. This is done by utilizing a theoretically appropriate model that matches the true decision environment where the choice set of eligible landowners includes enrollment in an easement, conversion, or the *status quo*. Even if a landowner chooses not to preserve, the existence of an option to do so may alter the time at which conversion occurs. Results from real options theory suggest that this may be the case – and, in particular, that the existence of the PDR option may *delay* conversion decisions.

With any program, incentive based or regulatory, there are often unintended consequences. Inevitably, the question which arises from these farmland preservation programs concerns the impact on neighboring parcels. If surrounding land use has an effect on the value of a parcel in a given land use, then it follows that surrounding land use will also have an effect on the likelihood of a parcel being developed. In the presence of such *interaction effects*, policies that alter development decisions can alter the likelihood of development of parcels other than those directly affected by the policy. The existence of interactions among neighboring parcels leads logically to path dependence in land use and pattern change. Concisely stated, do perpetual easement programs change the likelihood of development for the surrounding parcels? Previous work attempting to evaluate the impact of openspace on surrounding property values by Irwin (1998) and others suggests this is likely the case. Chapters 6 and 7 address the spillover effect of enrollment in the preservation program on neighboring parcels by utilizing an econometric technique to control for the endogeneity of preservation activity. An increase in the probability of conversion for surrounding parcels might mitigate the ability of the preservation activity to protect the rural landscape.

In summary, many proponents of land preservation focus on directly measurable quantities like the number of acres preserved as a measure of program success, but the benefits and costs of these programs likely extend beyond the quantity of land preserved. More difficult to measure goals, but ones often voiced by local governments, include maintenance of a functioning rural landscape and a curb on urban expansion.[2] This dissertation considers the possibility that these programs may generate benefits beyond those provided by the farmland enrolled in the programs by delaying development and allowing the county time to build infrastructure to meet the demands of recent urban expansion.[3] But it also addresses in part the possibility that the program, by creating pockets of permanently preserved land, may attract surrounding development activity.

---

[2] Both farmland preservation and these additional 'farmland' benefits do come at a cost, however: the foregone benefits associated with development. Whether the one outweighs the other is not at issue in this paper.

[3] This is the stated goal of many land use policies such as Adequate Public Facilities Ordinances.

1.1 Policy Environment

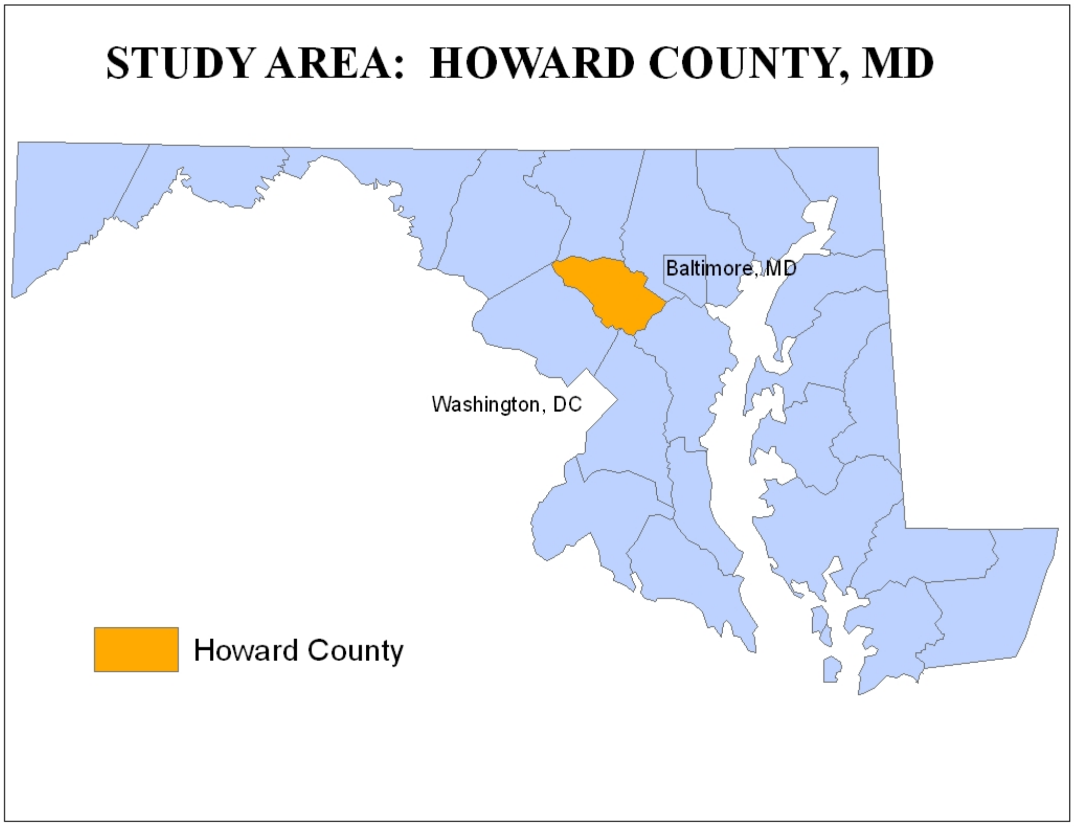
Because of the nature of property rights in the U.S., a limited number of policy instruments are available to the public sector to affect land use pattern and land use change. Land use policies are implemented at the state or local level and so can vary considerably across regions of the country. However, all are subject to challenges under the 'Takings Clause' of the U.S. Constitution, which restricts the degree to which public actions are allowed to affect the market value of a parcel and therefore the extent to which direct land use control is possible.

The study area for the empirical portion of this paper is Howard County in the state of Maryland (see Figure 1.1), so described below are the types of land use controls that are typically implemented in this region. While zoning ordinances serve to restrict the location of commercial and industrial uses, it is typically not possible to prohibit residential development except under very special circumstances (such as particularly extreme environmental conditions). Mentioned here are four common types of policies that attempt to affect the spatial pattern of residential development. In each case, the policies are designed so as to have differential effects across locations within a given locality.

- *Regulations that require different configurations of development in different regions of the locality*. Zoning stipulates the maximum overall density of new residential development in any given area. In recent years, many counties in Maryland facing development pressure have attempted to protect rural areas by 'down-zoning' (reducing the maximum allowable densities) in order to make development less profitable in those areas. Some have also introduced the concept of clustered development. Although the total number of housing lots does not change, clustering either allows or requires smaller housing lots than would be implied by maximum allowable densities, but clustered on one portion of a parcel, leaving the remainder in non-built uses. Zoning regulations can also stipulate that a portion of the parcel be set aside in common open space, even if clustering is not required.

- *Moratoria that temporarily slow development rates in specific areas*. Adequate public facilities moratoria can be used by Maryland counties to close a school district to further development for up to three years if school capacity has been reached.
- *Public works projects that encourage development in some areas by providing more public services*. Chief among these is the provision of public water and sewer service, which reduces infrastructure costs of construction.
- *Programs that support the public purchase of development rights of land parcels in specifically targeted areas.* These include state and locally funded programs to purchase development easements from landowners and thus preserve chiefly agricultural lands, although forested lands can be preserved under these programs as well.

Figure 1.1: Map of Study Area.



Howard County, Maryland is not an extremely large county in area, totaling only 160,000-acres, but it is unique because its location, wealth, and rural history combine to create competing preferences for growth and open space preservation. As shown in Figure 1.1 Howard lies between Baltimore to its east, Washington, D.C. to its south, and the growing city of Frederick to its west. Residents commute to all these employment centers, and as such the entire county is in one or another city's 'commuting shed'. Not surprisingly Howard County has experienced heavy development pressure over the last several decades. A simple review of the census data is quite revealing. Over the course of the study period relevant to this dissertation (1991-2001), the population of the county increased from 187,000 to 266,000 (a 41%

increase) and median home values rose from $206,000 to $425,000 (in 2000 dollars) - a 105% increase. Additional pressure is being exerted by neighboring counties that have 'downzoned' their agricultural areas to allow only extremely low density development (i.e. one house per 15 to 25 acres). In contrast, allowable densities outside the public water and sewer service boundaries in Howard County – that is, land nominally eligible for agricultural preservation – can be developed at densities of one house per 3 to 4.25 acres.

In Howard County, and indeed in most of the U.S., the primary mechanism for land use regulation is zoning which limits the number of units per acre via density requirements, open space requirements, and/or environmental restrictions. As a land preservation mechanism, zoning is not a very useful tool because zoning regulations are impermanent and, in most cases, cannot entirely prohibit land conversion.[4] Prohibitive, or even highly restrictive, zoning is likely to be challenged in court if landowners are not adequately compensated. Other than zoning and offering the preservation easement option, the county has relied on adequate public facilities ordinances to manage the pace and pattern of development. These ordinances allow the county to postpone, temporarily, new subdivision construction in any planning zone with insufficient school (and, more recently, road) capacity until new infrastructure can be built.

In this policy environment the Howard County PDR program purchases development rights from landowners in perpetuity and thus offers a mutually agreeable means for achieving permanent land preservation. In general, PDR

[4] Prohibitions on land conversion for environmental reasons are possible in some parts of Maryland – particularly along the Chesapeake Bay - but none of these areas exist in Howard County.

programs sever the development potential from the land while allowing the landowner to pursue any other permitted use of the land. As with many 'purchase of development rights' programs, Howard County's program purchases easements that prohibit conversion of land to specific non-agricultural uses, with the easement attached to the land in perpetuity, thus applying to all future land owners. The county's program is somewhat unique, however, in that the enrollment process is not bureaucratically cumbersome. In contrast, the state of Maryland's agricultural preservation program (MALPF – Maryland Agricultural Land Preservation Foundation), in operation since 1977, requires landowners to enroll in an agricultural district prior to selling easements. This constitutes a burden in a rapidly developing landscape. By the late 1980's, the terms of the county's program had become so favorable relative to the state's program that Howard County landowners universally chose the former over the latter. The county program paid over 3 times the price per acre offered under the MALPF program.

1.1.1 Preservation Program Details

When Howard County instituted the PDR program in 1980, about 34 percent of its 161,408 acres were in farmland. The goal of the program was to enroll 30,000 acres. Over the decades of the 1980's and 90's, more than 16,000 acres (at a cost of approximately $193 million dollars) were preserved in a PDR program while approximately 20,000 acres of the county were developed in residential uses.[5] These 16,000-plus preserved acres represent about 10% of Howard County's land.

[5] With the exception of some MALPF preservations in the early years, most of these 16,000 acres were preserved in the Howard County preservation program.

To qualify for the county PDR program, a parcel must be at least 100 acres; parcels at least 25 acres qualify if adjacent to at least 50 acres of preserved farmland. Eligibility requires 50% of land to be in the best soil classes and 66% in the top four of six land capability classes, as defined by the NRCS.[6] In addition, only parcels not served by public sewer and water are eligible. The price a landowner can expect to receive for an easement in the county PDR program is based on a published, publicly available, formula. For example, the county pays a higher price for parcels with better soils, more surrounding agricultural land, less erosion or drainage problems, and more actively farmed land in the production of food or fiber. The amount of public road frontage also adds value to a parcel in enrollment, for an example price formula worksheet see Appendix A.

The county ranks the applications based on the same criteria as in the pricing formula together with subjective information on the parcel's contribution to the farming industry (for example, farms with feed distribution facilities are ranked higher) and its viability in farming. Landowners are entitled to develop "family lots" while enrolled in the preservation program at a density of one lot per 50 acres enrolled. These family lots are meant to encourage farm transfers between generations, but there are no restrictions on the sale of these lots so landowners willing to forgo or limit this entitlement receive higher rankings. Parcels whose owners have offered to sell their easements are ranked on the basis of the above considerations and the county extends offers until funds are exhausted in each year. Deadlines for application are typically in November and decisions are made by the

[6] These eligibility requirements remained constant during the study period, but were slightly modified in 2003.

county in the following spring. To address environmental concerns each preserved farm must file a conservation plan with the county.

Over the life of the Howard County Agricultural Preservation Program, funding has been an issue. Parcels were preserved during the 1980's under a lump sum payment option which limited the amount of enrollment due to the significant payments coming from the county. In 1988 significant funds were appropriated (55 million) and again in 2000 (15 million), leading to easement purchases in 1990-97 and 2002 to the present, respectively.

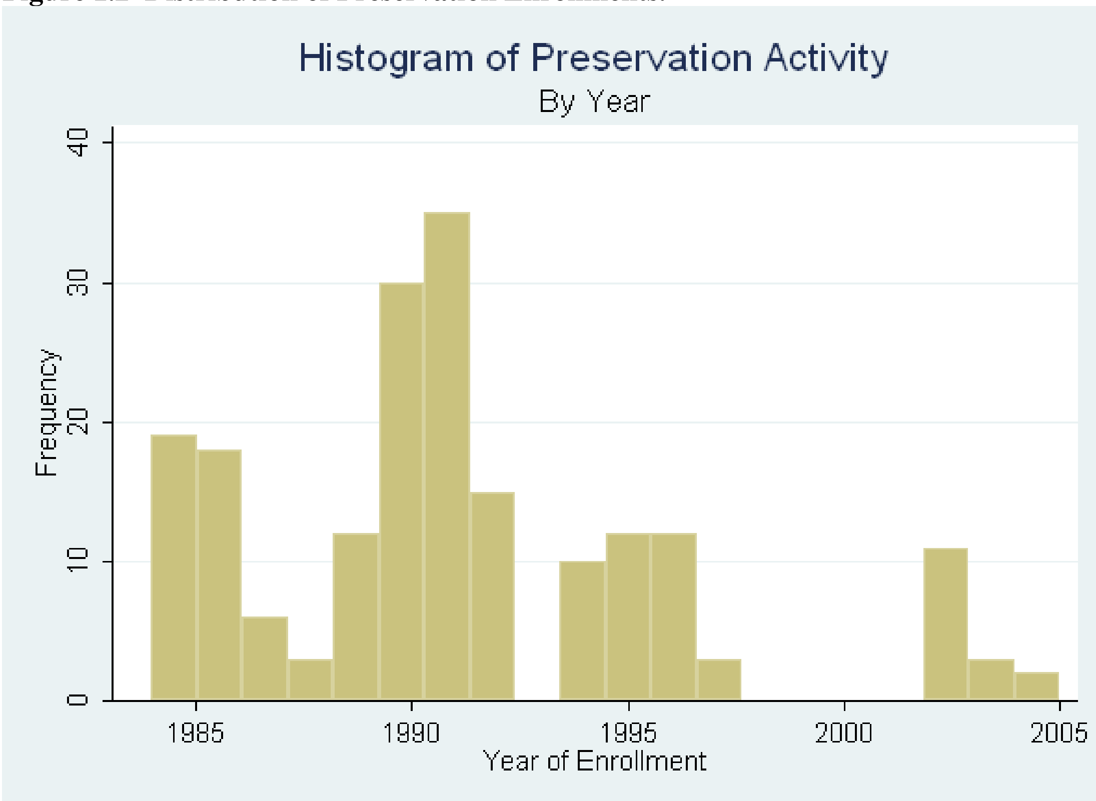
In addition to the funds injected into the program, financing and payment changes were made in 1988. First, the financing system was converted from a one-time payout to a tax free installment payout for 30 years and a balloon payment for the full easement amount at term end. From the landowners' perspective this greatly eased the tax consequences of enrollment and from the county's perspective this allowed the financial outlay to be spread across many years enabling more enrollments in each year. For example, a landowner with 100 acres receiving $6,000 an acre would initially have received a one-time easement payment of $600,000. Under the new terms, the landowner would receive a tax exempt payment each year of 6.5% of the easement value and at the end of the term a balloon payment of the full easement value, resulting in payments of over 1.7 million to the landowner but spaced out over 30 years. The county finances the payments by buying a bond to cover the principal amount, resulting in a first year outlay of approximately $60,000 rather than $600,000. This new system has enabled the county to purchase many more easements per year than prior to the changes.

The second major change concerned the funding source. As of 1988 the funding was tied to a 0.25% tax on real estate sales transactions, so that funding was secured via activity in the development market and future funding could be reliably forecast. Additionally a majority of the 5% conversion tax on land losing the preferential agricultural tax due to conversion has been dedicated to the PDR program. Since 1988 no general fund monies have been used to purchase development rights. The maximum payment per acre was set at $6,600 in 1988[7], adjusted to $20,000 in 2001, and is currently at $40,000 an acre.

From 1998 to 2000 the program had exhausted forecasted funds and no land was preserved.[8] From the very start of the program, the county's budget constraint was binding. Applicants whose parcels received a relatively low subjective ranking were either unable to preserve or experienced delays in the timing of preservation. Figure 1.2 shows the distribution of preservation activity over time. Arguably, recent decades represent the first time in history that the value of land in exurban counties exceeds the value of the productive resources of the land. This PDR program is designed to offer existing landowners the ability to extract some of the gains in land value that are ordinarily only accessible by converting land to development.

[7] For comparison the estimated development value was $15,000 per acre at this time per the county documentation proposing the changes to the financing structure.
[8] A budgeting issue prevented enrollment in 1993 as well.

Figure 1.2 Distribution of Preservation Enrollments.



2. Theoretical Framework

In order to analyze the effects of a land preservation program on development decisions, a theoretical model of the timing of development decisions serves as a necessary starting point.

2.1 The Traditional Net Present Value Rule and Extensions

Land conversion occurs when the land use is changed from an undeveloped or agricultural state to a developed state. The traditional economic model used to evaluate land conversion decisions implies a landowner will switch land use when the discounted stream of returns to development exceeds the discounted returns to the *status quo* land use — either agriculture, forestry, or a natural vegetative state. This is a net present value (NPV) approach (Carrion-Flores and Irwin, 2004; Parks, 1995; Brownstone and De Vany, 1991; Stavins and Jaffe, 1990).

Each period in which the land remains in the *status quo* state the landowner is viewed as making a decision about his land. The decision to subdivide is the first step in an irreversible development process and thus is the important decision to model for land use conversion. Subdivision is expensive to the landowner because it requires a change in tax status as well as legal, regulatory, and drafting fees. An alternative end state for undeveloped land in the study area is enrollment in the county agricultural easement program. In what follows any reference to 'preservation' refers to enrolling in an easement program in perpetuity[9] and to 'conversion' or 'development' refers to subdividing a parcel into *housing* lots unless explicitly stated otherwise. The small amount of land that is zoned 'commercial/industrial' is ignored, as commercial development is not an option in areas where preservation is possible.

Putting the preservation option aside for the moment, the traditional conversion decision rule is the net present value rule which suggests a landowner will change land use at the moment the return to conversion is greater than the discounted sum of future agricultural returns.[10] The net present value rule (NPV) prescribes conversion when

$$D(i,t) - C(i,t) > \sum_{s=0}^{\infty} \beta^s A(i,t+s). \quad (1)$$

β^s is the discount rate and $A(i, t+s)$ is the return to agriculture for parcel i in period $t+s$, so that the right hand side of (1) is the discounted net present value of all future agricultural returns. $C(i,t)$ is the conversion cost which may include real estate fees and infrastructure costs, and $D(i,t)$ is the return to the landowner from subdividing. This return might come as a lump sum or a stream of payments. A one-time payment occurs if the landowner sells directly to a developer. However if the landowner contracts with a developer or undertakes the conversion himself, the return is a stream of payments over the course of the lot sales. Unfortunately, detailed transaction specific data are not available, making payment structures impossible to distinguish,

[9] Throughout the term 'parcel' is used to refer to the original, undeveloped land unit and 'lot' to refer to each of the subdivided land units.

[10] For the moment the analysis will ignore the fact that land not in agricultural use or developed use (simply "undeveloped" land) has a negative return stream based on the NPV approach in an accounting sense, although it may generate utility to its owner. Since forest use is an agricultural use, the "undeveloped" category is a small percentage of land in the exurban area.

and from here on the model is presented in terms of a one-time payment to the landowner.

In many exurban areas of Howard County, development returns net of conversion costs can be expected to exceed the discounted stream of agricultural returns for most parcels, yet we do not observe all parcels immediately converting to development. In such areas, the NPV conversion rule implies more land conversion than is actually observed. This rule does not take into account expectations that future development returns might be growing at a rapid rate, making postponement desirable even in the face of high current returns to development. This possibility can be captured in a modification of the above rule, such that conversion occurs if the net returns to development today exceed the expected net returns if development is postponed one period. Development will occur under this rule if:

$$D(i,t) - C(i,t) > A_t(i,t) + \beta\{E[D(i,t+1)] - C(i,t+1)\} \tag{2}$$

where *E[]* is the expectation operator. This framework implies that all parcels will eventually be profitable for development and follows from the expectation of no real growth in agricultural returns in the region or growth that is slow relative to growth in development returns as to be trivial. In exurban areas it may be reasonable to assume that as developable land becomes scarce the growth in development returns will swamp returns to other uses. The expression in (2) mimics a stochastic dynamic programming approach and has formed the basis of previous research on development (e.g. Irwin, 1998), where postponement (or the 'wait' decision) was attributed to expected ongoing increases in development returns due to land scarcity and development pressure from growing populations and/or incomes.

The alternative end state – preservation – can be introduced into this framework. The returns to preservation equal the value of holding the land in perpetuity. This is most easily represented as the present value of an infinite stream of agricultural returns, but may also include non-monetary utility-generating motives such as the value of holding a large tract of land for aesthetic or recreational use or the value of preserving a family farm. Defining *y* as the present value of expected returns from the landowner's optimal decision, and ignoring the non-measurable/non-monetary returns from preservation, the decision rule is now based on the maximum value function:

$$(3)\ y = max\{\sum_{s=0}^{\infty} \beta^s A(i,t+s) + e(i),\ E[D(i,t)] - C(i,t),\ A(i,t)+\beta(E[D(i,t+1)]-C(i,t+1)\}.$$

The first term in (3) is the monetary return to preservation, equaling the returns to agriculture in perpetuity plus the easement payment, *e(i),* which varies over parcels. The second term is the expected net development return if development is initiated in the current period. The third term represents the returns from agriculture in the current period plus the discounted expected net returns from postponing development until the next period.

2.2 A Real Options Model of Land Conversion

A key element of the models in expressions (2) and (3) is the term representing the value of waiting. The value of waiting to make an investment is analogous to a financial option in which having the option to make a decision in the future is of value. Many economists have compared the development decision to the exercise of an option (see Capozza and Li, 1994; Capozza and Hensley, 1990; Geltner et al., 1996; Plantinga, Lubowski, and Stavins, 2002). Real options are like financial options but pertain to real assets such as land.

Unlike a NPV conversion rule, a real options approach allows uncertainty to influence decisions. Specifically, the option to invest in the future has value which helps explain why undeveloped parcels exist even when development returns exceed returns from the current use. Three characteristics define a real option:

- The option, once exercised, is irreversible.
- The decision can be delayed.
- Uncertainty exists about future payoffs.

Land conversion exhibits all these features. First, developed land is generally not converted back to agricultural land. Second, the decision can be delayed and, in most cases, cannot be removed from the parcel owner by right, eminent domain cases being the exception. Finally, the payoffs are uncertain because future property values are uncertain.

In most real options models of land use and in the model being proposed here, the uncertainty associated with decisions is concentrated in the returns to future development. The landowner is assumed to have far less uncertainty over returns to current uses (which may be effectively zero or may experience little variation over time) and to easement payments (as they follow published county formula). However future returns to development – and especially growth in those returns – may be highly variable over time, depending on regional growth in population, employment, and incomes, as well as changes in interest rates and in the demographic composition of the population. Therefore the primary driver in this model is the uncertainty in development returns.

The basic real options story is outlined in many sources (see Dixit and Pindyck, 1994, Ch. 5), so the focus of this discussion is on the key elements relevant to the empirical model to be described in the next chapter. The problem is one of choosing the optimal time to invest in a project with return of *D* and an investment cost of *C*. *D* is assumed to evolve over time following a geometric Brownian motion with drift:

$$dD = \alpha D dt + \sigma D dz \quad . \tag{4}$$

In equation (4), α is the 'drift' (i.e. the rate of growth) in expected returns, σ is the standard error of the investment value, and dz is an increment of a Weiner process or the continuous time equivalent of a random walk. Equation (4) implies that the current value of the project is known, but future values are uncertain. [11] The change in development value is assumed distributed log normal with a variance that grows linearly with the time horizon.[12]

---

[11] In the land conversion model, the drift and variance parameters are time varying. This does not change the interpretation.

[12] Employing techniques described in Marathe and Ryan (2005) the assumption of log normality is validated. This is done by testing whether the change in inflation adjusted house prices departs from a log normal distribution. Specifically, the difference in the log mean sales prices from year t and t-1, by tract, does not violate the null hypothesis of normality using a Shapiro-Wilk test or visually using Q-Q plots.

In this study D represents the gross return to the landowner from subdividing the parcel and selling the resulting lots to households. D is a function of parcel, neighborhood, and regional characteristics that are likely to influence development returns. The value of the option to convert land in the future is defined by the function $F(D)$:

$$F(D) = \max_{t} E[(D(i,t) - \tilde{C})e^{-\rho t}], \tag{5}$$

where T is the optimal time of conversion, $\tilde{C}$ is the cost of conversion including opportunity costs of foregoing future agricultural returns, and ρ is the discount rate.[13] $\tilde{C}$ is assumed to vary little in real terms over the foreseeable time horizon. The option will be exercised when the return to investment exceeds the expected capital appreciation in the value of the option.

The solution to the problem must satisfy several conditions, including continuity restrictions and an 'absorbing boundary' condition - if the option value goes to zero it stays at zero. In the land use context, a zero option value would imply that the development option is no longer available, which is typically not possible unless the landowner enters a preservation easement. Dixit and Pindyck derive the solution to the optimal timing decision as:

$$D^{*} = \frac{\psi}{\psi - 1}\tilde{C} \ , \tag{6}$$

[13] It is necessary for $\rho > \alpha$. That is, the impatience embodied in the discount rate must exceed the mean increase in return. Otherwise, a landowner would always find it optimal to wait to invest.

where the term pre-multiplying $\widetilde{C}$ represents the wedge between the real options investment rule and the neoclassical (NPV) investment rule. The term ψ is a function of the drift (α), standard error (σ), and a discount rate (ρ) and is shown by Dixit and Pindyck to be positive and greater than 1. Therefore, in a world of growing development returns, the real options rule represents delayed development relative to the NPV rule.

Dixit and Pindyck derive the comparative static results that are the basis for inclusion of the variance and drift variables in this empirical application. They show that ψ is decreasing in both drift (α) and standard error (σ). Since $\partial D^* / \partial \psi = -(\widetilde{C} + A) / (\psi - 1)^2 < 0$, a decrease in ψ implies a larger wedge between investment return and cost. This increases the hurdle to development and delays the optimal time to convert. The comparative static results from the options framework imply that increases in the variance and drift of the returns to development decrease ψ and thus will tend to delay conversion decisions. The theory of real options is quite elegant and intuitive despite the mathematical complexity however testing the implications of this theory has proven quite elusive perhaps due to the intense data requirements necessary. The next chapter describes the necessary empirical framework for testing the theoretical predictions from real options theory.

3. The Empirical Framework

The initial hypothesis concerns whether or not preservation eligibility affects the timing of the development decision. In the event that a statistically significant effect is found, it is also of interest to quantify the magnitude of the impact. The last chapter framed the decision process for the landowner in the context of an options model where the landowner had three choices at any given time - convert, preserve, or wait. This section presents an empirical model capable of estimating the preservation eligibility impact on the conversion decision as well as the impact of price volatility in the time dimension. Although the easement price does not fluctuate explicitly with market conditions, qualification for the easement program does present an additional option which is expected to enter the decision process of the landowner when making land use decisions.

Many land use studies evaluate conversion decisions utilizing discrete choice models as a function of parcel level attributes (Bockstael, 1996; McMillen, 1989; Kline and Alig, 1999; Landis and Zhang, 1998). This approach provides insights on how parcel attributes affect the probability of conversion but does not account for the dynamic environment in which conversion decisions are made. Duration models, on the other hand, are particularly useful for studying factors affecting the occurrence and timing of decisions and are increasingly applied in a land use context (Mayer and Somerville, 2000; Irwin and Bockstael, 2002; Bulan, Mayer, and Somerville, 2002; Hite, et al., 2003).

Duration models are employed because the addition of a time dimension allows for more sophisticated preservation program evaluation – something more than just counting acres preserved. Also, duration models can incorporate time varying covariates which help account for the dynamic environment in which land use decisions are made. Duration models explicitly take account of the fact that an action taken in period t implies the action was not taken in any previous period, $T<t$. This model will be used to test the impacts of the preservation option on the timing of conversion and to test the comparative static results from real options theory. In order to be more confident in the results, several obstacles must be overcome - most importantly the impact of unobserved heterogeneity and the assumption of non-random censoring.

This chapter will briefly cover the basics of duration analysis, then highlight the issues of unobserved heterogeneity and non-random censoring and mention how each will be addressed in estimation. The desired model is one that is robust in the presence of unobserved heterogeneity and allows the existence of a competing exit state, or risk. In the lingo of duration models, preservation and development outcomes are referred to as "risks" and the observed failure event is the act of subdivision or the act of enrollment in the preservation program. The fact that multiple risks exist tends to greatly complicates analysis but leads to a richer model that closely mimics the decision process outlined in the previous chapter. Again, the primary goal of this estimation is to quantify the impact of eligibility on development timing in the real options framework. However, an important secondary goal is to specify an appropriate parametric functional form for prediction and simulation of future land conversion patterns.

3.1 Duration Analysis – the Basics

An exhaustive review of the concepts of duration models can be found elsewhere.[14] What follows is a brief summary of the basics and terminology required to present the proposed empirical model. Suppose one is concerned with a random variable *t*, the time until an event, and one wishes to know the influence of specific covariates *x* on *t*. An application of least squares to this type of problem suffers from three major problems – it requires data aggregation that will drop time varying covariates, it cannot handle censored observations (observations that do not experience the 'event'), and it might predict meaningless negative durations (event occurrence before time zero).

Duration models were developed to address these limitations. Observations (spells) are realizations of an underlying random process which can be characterized by the probability density function (*pdf*)

$$f(t) = \Pr(t \leq T < t + dt)$$

and the corresponding cumulative density,

$$F(t) = \int_0^t f(s)ds = \Pr(T \leq t),\ t \geq 0,$$

where $T \geq 0$ denotes the duration until failure and *t* denotes a particular value of *T*. By assuming *f(t)* has only nonnegative support eliminates the possibility of negative time durations.

[14] For detailed surveys see Kalbfleisch and Prentice (1980); Keifer (1988); Lancaster(1990)

The survival function, $S(t)=1-F(t)$, is the complement of the cumulative distribution function (*cdf*) and is the mathematical representation of the likelihood of surviving until time *t*. Thus $S(t)=\Pr(T>t)$. The survival function serves as the contribution to the likelihood function for observations that do not fail during the time under study. These observations are called 'right-censored' in the literature. Observations that "fail" contribute the value of their *pdf* to the likelihood function.

There are two additional functions of interest: the hazard function, $\lambda(t)$, and the integrated hazard, $\Lambda(t)$. The hazard function is the instantaneous probability of failure in the interval *dt* assuming survival up to time *t*:

$$\lambda(t)=\Pr(t\le T<t+dt\mid T\ge t)=\frac{f(t)}{S(t)}. \tag{7}$$

The discrete analog to (7) is

$$\lambda(t)=\lim_{\Delta t\to 0}\frac{\Pr(t\le T<t+\Delta t\mid T\ge t)}{\Delta t}. \tag{8}$$

The integrated hazard is given by $\Lambda(t)=\int_0^t \lambda(s)ds$ and is the total accumulated risk an individual has been exposed to by time *t*.[15]

To facilitate estimation, it is necessary to incorporate covariates. This is typically accomplished by specifying the individual hazard as

$$\lambda_i(t)=\lambda_0(t)\kappa(X), \tag{9}$$

where $\kappa(x)$ is the systematic part typically specified as $exp[X_i\beta]$ and $\lambda_0(t)$ is the baseline hazard common to all observations. This general form is called the proportional hazards specification because the effect of covariates is to shift the

---

[15] It is easily shown that $S(t)=exp[-\Lambda(t)]$. That is, the survival function is directly related to the sum of all previous hazards.

hazard proportionally. It is, by far, the most popular model utilized in the hazard literature. Since duration models do not aggregate data across time, incorporating time varying covariates in this framework is straightforward.

The most common approach to estimation is maximum likelihood. Observations are divided into two groups: observed failures and censored observations. As observed failures enter the hazard via their probability density functions and censored observations enter through their survival functions, the general form of the log likelihood function for N observations is written as

$$\ln L = \sum_{i=1}^{N} d_i \ln[f(t_i, X_i)] + (1 - d_i)\ln[S(t_i, X_i)]. \tag{10}$$

X_i is observation i's vector of observed covariates and d_i is an indicator variable equal to 1 if the i^{th} observation fails during the study period and 0 if the observation is right-censored. From this formula, it is easy to see how hazard models utilize information from censored observations via the likelihood contribution of the survival function. This formula ignores the possibility of time varying covariates. Including them amounts to adding 'spells' to the data, where a 'spell' is defined as an interval of time and the associated quantities relevant to each observation during that interval. That is, an observation will contribute multiple spells of data, one for each time interval over which covariates remain constant.[16]

There are important explicit assumptions involved in estimating a traditional hazard model, most noticeably the choice of baseline hazard specification. The next section describes an array of hazard models which will be utilized to select the final

[16] One should estimate standard errors using appropriate robust techniques that drop the independence assumption between observations.

form of the dependent competing risks. This discussion will begin by concentrating on the parametric assumptions involved in estimating hazard models and the pros and cons of imposing various parametric assumptions. These techniques will be utilized during estimation to impose necessary restrictions on the general competing risks model which will be introduced at the end of this chapter. First the specification of the baseline hazard, $\lambda_0(t)$, will need to be determined. Then unobserved heterogeneity will be incorporated. Last the dependent competing risks model can be estimated imposing the necessary distributional assumptions.

3.1.1 Parametric Baseline Models

If strong prior theoretical or empirical grounds exist to imply a particular form for the baseline hazard, a parametric function for $\lambda_0(t)$ can be imposed. By imposing a particular baseline hazard specification the researcher is restricting the shape of the baseline hazard and may even impose a specific form of duration dependence. Duration dependency is best thought of as the shared, or baseline, probability of failure for the members of the dataset in response to the passage of time. Specifically, if the longer one survives without failure implies a lower probability of failure for the next time interval then the duration dependency is negative. However, if a similarly lengthy survival implies a high probability of failure in the next time interval then the duration dependency is positive. Examples of negative duration dependency include infant mortality or post operative infection because the longer an infant survives outside the womb the more likely they are to survive to the toddler stage and in many cases the probability of post operative infection declines with the passage of time after surgery. Examples of positive duration dependency are adult mortality or failure of a mechanical object because as we age our probability of survival decreases and similarly a mechanical object, such as a light bulb or a mechanical gear, has a higher probability of failure as time passes.[17]

Among the class of proportional hazards, the most popular specification for the baseline is the Weibull, $\lambda_0(t) = pt^{p-1}$, characterized by a monotonically decreasing or increasing hazard rate with one shape parameter, *p*. A Gompertz baseline hazard, $\lambda_0(t) = \exp(pt)$, also allows for monotonic duration dependency while an exponential baseline, $\lambda_0(t) = 1$, produces a "memory-less" hazard which has no dependency on time and obviously no additional parameters to estimate.[18] Specification of a parametric baseline hazard allows for more efficient estimation and prediction of survival times for censored parcels in the analysis.

Unfortunately, in this application there is no reason to assume a form of duration dependency. Duration dependence can be expected to be negative if, as time progresses, observations remaining in the risk set are less and less likely to 'fail'. There is at least one reason why this might characterize the land use conversion case. Parcels that develop early will tend to have attributes with high development value. Thus the composition of the risk set will be changing over time, with an increasing proportion characterized by attributes with less desirable attributes for development. This phenomenon suggests negative duration dependence. However market pressures may conceal or mitigate this negative effect. As the supply of developable parcels

---

[17] Duration dependency can also exhibit a minimum, a bathtub shape, or a maximum, a humped shape, but these models are not proportional hazard models and can be estimated with semi-parametric proportional hazards.

[18] The exponential is so named because the covariates are incorporated using an exponential and the form the of hazard presented in equation (9) reduces to $\lambda_i(t) = \exp(X\beta)$.

decreases, the rising returns to conversion of remaining land may mitigate the negative effects of declining quality. Because of these countervailing forces there is no strong theoretical reason to expect a specific shape of the baseline hazard or to expect the baseline to be monotonically increasing or decreasing throughout the study period. However, semi-parametric proportional hazard specifications allow the baseline hazard to take any shape without restriction. The next section explains how these models are estimated.

3.1.2 Semi-Parametric Baseline Models

If theory offers no insight about the shape of the baseline hazard, a safe approach is to allow semi or non parametric estimation of this function. A semi-parametric method to accomplish this uses a piecewise exponential baseline hazard specification. In this case the baseline is allowed to vary freely from one time interval to another but is constant across observations within time intervals. The piece-wise exponential baseline hazard is specified as:

(11) $$\lambda_0 = \sum_{m=1}^{M} h_m \delta_m \text{ where } \delta_m = 1 \text{ for } a_{m-1} \leq t < a_m \text{ and} = 0 \text{ otherwise.}$$

In (11), the a_m's represent a series of temporal breakpoints, and the h_m's represent the baseline hazard rates in each of the m intervals. The breakpoints can be set such that λ_0 follows the periodicity of the data or partitioned such that an equal number of failures occur in each period. The key weakness of this specification is the lack of predictive power beyond the last time period in the data. Because h is allowed to vary in a non-systematic way over time, there is no way of predicting future outcomes that is not heavily influenced by the value of the h_m in the last defined period.

3.1.3 Non-Parametric Baseline Model

If one is interested only in the impacts of covariates, a method developed by Cox (1959) using a partial likelihood approach is appropriate. The common baseline hazard is treated as a nuisance parameter and factored out of the likelihood function. To see this, note that for any two observations, *i* and *j*,

$$\frac{\lambda_i(t)}{\lambda_j(t)} = \frac{\lambda_0(t)}{\lambda_0(t)} \exp\{(x_{i1} - x_{j1})\beta_1 + \ldots + (x_{ik} - x_{jk})\beta_k\}, \tag{12}$$

and the baseline hazard, being the same for everyone, cancels out. Estimation is accomplished using the 'partial' likelihood function, where the term 'partial' denotes the fact that estimation of the baseline is not attempted. This method is based on the assumption that the intervals between successive duration times (failure times) contributes no information regarding the relationship between the covariates and the hazard rate (Collett, 1994). It is the order of the failure times, not the interval between failure times, which contributes information to the partial likelihood function.

Consider a data set in which there are N observations of which N_f fail during the study period and N-N_f are censored. The likelihood function for the Cox model is the product of N_f terms – one for each failure, in which the contribution of the i^{th} failure is given by:

$$\Pr(t_i = T_i \mid R(t_i)) = \frac{\exp(X_i \beta)}{\sum_{j \in R(t_i)} \exp(X_j \beta)} \tag{13}$$

where t_i is defined as the time period of the i^{th} failure and $R(t_i)$ is the set of all observations still at risk at time t_i. Expression 13 is the probability that, given that a failure occurs in period t_i, it is the observation i among the set of observations still at risk that is the one that fails. Taking the product of these conditional probabilities yields the partial likelihood function:

(14) $$L_p = \prod_{i=1}^{N} \left[\frac{\exp(\beta' X_i)}{\sum_{j \in R(t_i)} \exp(\beta' X_j)} \right]^{d_i},$$

where $d_i=1$ if the observation is uncensored and $d_i=0$ if it is right-censored (i.e. the observation does not 'fail' during the study period and remains in the risk set). This is a conditional logit likelihood function, often referred to as a fixed effects model, where the "effect" that is "fixed" is the risk period. Note that the likelihood function does not include an explicit term for the censored observations, although they are represented repeatedly in the denominator as they remain in the risk set throughout.

If the data have many 'ties', in the sense that multiple observations fail in the same time period, then problems arise in composing the risk set, as it is typically impossible to know which observation failed first. There are methods to handle ties, but in general when data contain many ties the Cox method should be used with caution. However, since the Cox model is primarily concerned with the impact of covariates, it provides an appealing specification test for estimation based on a parametric functional form of the baseline hazard. Because parametric specifications are usually somewhat arbitrary but necessary if simulation into the future is desirable, it is useful to compare parametric results to the results of the Cox model. Such a comparison allows investigation of the impact of the parametric choice, and in particular whether the imposed baseline is altering the covariate effects. It is for this purpose that the Cox model will be estimated.

3.2 Unobserved Heterogeneity

Despite the great detail afforded by the land use data available in Howard County, there are still significant unobservable individual or parcel specific factors that will impact the conversion or preservation decision. As mentioned in the previous section in order to control for heterogeneity in the observables, as it impacts the duration dependence, these observables are simply included in the model. However, heterogeneity that is not captured by the observed explanatory variables within the conversion hazard may imply that more desirable parcels will likely convert first leaving less desirable parcels in the sample longer, resulting in a length-biased sample and potentially incorrect negative duration dependence.[19] To make the model robust in the presence of influential unobservables, an estimation approach that can accommodate a distribution of unobservable "random effects" will be utilized. In this policy environment unobservables arise from at least two sources related to the parcel, through *parcel* attributes and *landowner* attributes.

Unobservable *parcel* attributes include the type of activity on the parcel. In the dataset it is known that a parcel is in an agricultural use but there is no knowledge of the type of agricultural activity.[20] It would be logical that certain farm types are more likely to enroll in a preservation program while simultaneously less likely to convert. For example, vegetable and equine operations might be more apt to preserve

[19] The length-bias sample issue is prevalent in stock samples. These data are a stock sample because developable land is not added during the study period.
[20] Publicly available data do not contain parcel level crop or livestock activity.

than soybean/corn or forestry operations, possibly because the former are more profitable in Maryland than the latter. Other unobservable but important attributes might include aesthetic characteristics such as elevations with views that might make a parcel more valuable in housing and thus prone to conversion. There may also be unobservable parcel attributes that affect both preservation and development likelihoods, such as drainage problems or odd parcel shapes. These factors may preclude parcels from receiving high scores by preservation authorities and make them unlikely candidates for conversion as well.

Unobservable *landowner* attributes will also affect the hazards of development and/or preservation. Landowners with intensive investment in the operation may be less inclined to convert because of sunk investment costs, but these same parcels might be more inclined to protect their investment by enrolling in an easement program. Landowners close to retirement age may tend to develop while those in need of cash to pay off farm debt or send children to college may be more likely to develop *or* preserve relative to the *status quo*. All of these individual landowner attributes potentially impact the duration variables of interest but are not available in most land use datasets. Models that fail to account for the influence of unobserved heterogeneity can lead to inconsistent estimates, incorrect standard errors, and misleading inference concerning duration dependence, all of which will be discussed in detail in the next chapter.

3.2.1 Incorporating Unobserved Heterogeneity

In univariate (single risk) duration models, unobserved heterogeneity arises because of the inability of a researcher to obtain all the relevant covariates that govern the duration under study. Unobserved heterogeneity can lead to problematic inference from inconsistent parameter estimates, incorrect standard errors, and misleading estimates of duration dependence. Concerning duration dependence, the most serious problem is this: unobserved heterogeneity tends to produce estimated hazard functions that decline in time, even when the true hazard is not declining for any individual in the sample (Allison, 1997).

Lancaster (1990) and Kalbfeisch and Prentice (1980) discuss issues surrounding unobserved heterogeneity and suggest that a parametric distribution be multiplicatively included in the proportional hazards specification as a potential modeling solution:

(15) $$\lambda_i(t) = \lambda_0(t)\exp(X_i\beta)\upsilon_i .$$

In duration models υ_i is often called "frailty", although υ_i is more familiarly recognized as a random effect (see Nickell (1979), Flinn and Heckman (1982) for implementations). Models that specify a parametric baseline and a parametric frailty are called mixture models because the "error" is essentially a mixture of two distributions. The "frailty" term is drawn from a distribution with density $g(\upsilon)$ and is assumed uncorrelated with any covariates in the model. The imposition of a random effect is a contentious and perhaps heroic assumption, because of this assumption of zero correlation between the random effect and the observed covariates. In panel data models the uncorrelated nature of the random effect is testable, but in single spell duration data it is not (Heckman and Singer, 1982).[21]

If one is willing to assume a distribution for the individual frailty parameter, estimation of these models amounts to estimation of one (or more) additional parameters describing the frailty distribution (see Klein and Moeschberger, 1997, or Box-Steffensmeier and Jones, 1997, for overviews). Log normal distributions are often used such that $g(\upsilon) = g(\upsilon;\theta)$ is distributed log normal with a unit mean, necessary for identification, and a variance, θ.[22] This approach allows one to test the hypothesis that $\theta = 0$, which suggests no undue influence of unobserved heterogeneity.

Heckman and Singer (1984) is an often cited work for the nonparametric inclusion of unobserved heterogeneity in the duration model. They show that including parametric heterogeneity is potentially too limiting and prove that in the class of mixed proportional hazard models the nonparametric (maximum likelihood) estimator of the heterogeneity distribution is a discrete one. Their results illustrate the flexibility of discrete distributions to mimic a wide range of mixture duration distributions (van den Berg, Lindeboom, and Ridder, 1994). In applications where a fixed number of supports are used, the locations and population proportions at each

[21] However, with the addition of time varying covariates which effectively induces a panel dataset, a Hausman test might be applicable, though not currently established.

[22] Other distributions include *t*, inverse Gaussian, Gamma, and power variance model. Technically, any continuous distribution with a positive support with unit mean and a finite variance can be used.

support are estimated jointly. The number of supports should increase as the data allow (Gritz, 1993; Ham and LaLonde, 1996).

On the other hand, Han and Hausman suggest that the non-parametric unobserved heterogeneity estimation proposed by Heckman and Singer is not completely necessary. The results from Heckman and Singer, and similar results from Honore (1990), are derived from a model with a parametric baseline hazard and non-parametric unobserved heterogeneity. In contrast, Han and Hausman impose a gamma distribution for the unobserved heterogeneity and a semi-parametric piecewise exponential baseline hazard suggested by Meyer (1986) and conclude that the non-parametric unobserved heterogeneity is not necessary. As with the specification of the baseline hazard the specification of a distribution for unobserved heterogeneity should be tested against the least parametric approach available, in this case the finite mixture distribution, as validation that the imposed distributional assumptions are not influencing the estimated results.

3.3 Non-random censoring / multi-state models

To this point univariate hazard models have been assumed, i.e. models with one exit state – either development or preservation. An obvious extension, and a necessary one for this analysis, allows for multiple exit states. Examples of multiple hazards from the general hazard literature include exit from an unemployment spell to a part time *or* full time job, and exit from a healthy state to diabetes *and/or* heart disease. In the land use context some parcels can 'exit' the *status quo* state by enrolling in the preservation program or by converting to development, and both possibilities need to be explicitly taken into account.

It is possible, even within the context of a univariate model, for observations to exit the sample by means other than the failure event under study. Thinning of the sample through time for reasons unrelated to the exit event of interest is called panel attrition. The univariate model will continue to be a valid one if the incidental exits are independent of the modeled failure event. This independence assumption is commonly called *random censoring* or *non-informative censoring* in the literature. Independence is conditional on observed covariates and is an implicit assumption of all univariate hazard analyses.[23]

Applying a univariate model can lead to misleading results when exit to another state is not an independent process. The unemployment example provides a clear violation of this assumption. If unobserved ability influences exits to part time work or school then the processes are not independent of the failure event of interest: full time employment. The land conversion decision in the presence of a preservation program is another good example of potentially dependent exit states. There is no reason to assume that exit to preservation is independent of the development decision, even when conditioned on observables. For one thing, landowners whose circumstances require them to liquidate assets are more likely either to develop or preserve than to remain in the *status quo* state.

With multiple states, the hazard model must be reframed. For each observation, *i*, a draw from the latent *K*-dimensional distribution may be represented

[23] This is the hazard model's version of the conditional independence assumption. If a researcher can identify and measure the underlying risk factors that produce the dependency, then accounting for them explicitly in the model maybe sufficient to ensure this condition.

as a *Kx1* vector T_i, where K is the number of possible exit states Each element in this vector, T_{ki}, is an exit time and each k is an exit state. In some applications the entire vector may be observable which implies the exit into one state does not preclude exits into a second, third, or K^{th} state. Based on the statistical nomenclature, 'competing risks' models are a subset of these more general models.

In the competing risks models, exits are mutually exclusive. The best example, though slightly morbid, is the exit from a post-operative state. One might observe patient *i*'s death from an infection, but this patient was at risk from a myriad of other sources (e.g. heart disease, cancer), only one of which could kill him. This analysis falls into the competing risks models because exits to preservation are precluded from conversion and conversion exits cannot subsequently enroll in the preservation program.[24] Given that the nature of the land use problem is one in which only two alternative exit states exist, the remaining discussion is presented assuming two mutually exclusive states.

When exit states are mutually exclusive, the entire distribution of survival times, $S(t_1, t_2)$, is not observable. As a consequence competing risk models are considered latent variable models. The researcher observes $T = \min(T_1, T_2)$ along with the cause of failure outcome, O. The data (T, O) are referred to as the identified minimum. In the absence of regressors the joint distribution (T_1, T_2) is not identified by (T, O) (Cox, 1959, 1962; Tsiatis, 1975). In particular, for any joint distribution of dependent failure times there is a joint distribution of independent failure times that

[24] Although both reversals are technically possible it is unlikely a housing development will revert to agriculture and preserve and as designed the preservation easements are written in perpetuity.

will produce the same identified minimum.[25] Because it was believed that all dependent competing risks models were unidentified, it was common practice to assume independence for estimation (Gordon, 2002). However, erroneously assuming independence leads to incorrect inference. In addition, the fact that durations, T_k k=1, 2, are related is often an important issue in its own right. Generally, independence does not make sense if individual behavior influences the decision to enter multiple end states.

Identification *with regressors* was first established by Heckman and Honore (1989) they proved that models with dependent risks are identified if there is sufficient variation in the latent failure times with regressors. Abbring and van den Berg (2003) prove identification for a class of slightly more restrictive but more popular models called mixed proportional hazard (MPH) models. By focusing on MPH models the authors impose fewer restrictions on the domain of the covariates, X. Loosely speaking, X must have two continuous variables that are not perfectly collinear and that act differently on each hazard in the two-risk world (van den Berg, 2005).

The general specification for a MPH competing risks model with unobserved heterogeneity is

$$\begin{aligned} \lambda_1(t \mid x, V) &= \omega_1(t)\exp(x'\beta_1)V_1 \\ \lambda_2(t \mid x, V) &= \omega_2(t)\exp(x'\beta_2)V_2 \end{aligned} \tag{16}$$

where $\omega_k(t)$ is the baseline hazard associated with each risk. Each risk is modeled as a mixed proportional hazard where V is unobserved. For each risk the composition of survivors changes selectively with time, as the more ‘frail’ exit quicker than the ‘less

[25] This non-identifiability theorem was established for models in the absence of covariates.

frail' via the distribution of unobserved heterogeneity, so identification from $(T \mid X)$ is non-trivial and is not always possible. Assuming independence implies that $(T_1, T_2|X)$ are independent and is equivalent to running two univariate hazards - one for the preservation risk and one for the conversion risk. Allowing for dependence then implies that $(T_1,T_2|X,V)$ are independent where the end states exhibit dependence through the distribution of V_1 and V_2 , not directly through X.

Van den Berg (2005) gives an intuitive summary of the identification results which are paraphrased here. Assuming at least two continuous regressors, one can manipulate $exp(x'\beta_1)$ while keeping $exp(x'\beta_2)$ constant. If $(T_1, T_2|X)$ are independent then the observable hazard rate of T_2 at $t>0$ given $T_1 \geq t$ does not vary with changes in $exp(x'\beta_1)$. However, the MPH model presented in (16) allows for dependence of $(T_1, T_2|X)$ via the unobservables. For example, changes in $exp(x'\beta_1)$ change the distribution of the unobservable V_1 among survivors at time t due to the fact that X and V are dependent conditional on survival $T_1 \geq t > 0$ even though they are independent unconditionally.[26] Now if V_1 and V_2 are dependent this change affects the distribution of V_2 among the survivors at t which in turn affects the hazard of T_2 at t given $T_1>t$, i.e. the observation has not exited via the first exit pathway. Thus the variation in T_1 with $exp(x'\beta_1)$ for a given $exp(x'\beta_2)$ is informative on the dependence of the durations. In the most flexible competing risks model the unobservables are serving two functions. They allow for dependency across risks, and they control for the inherent selection problem due to unobserved heterogeneity within each risk.

[26] This is simply another description of the unobservable heterogeneity impact presented in the previous section.

To summarize, the dependent competing risks models, though first thought to be unidentified, have subsequently been proven identified by including regressors with varying restrictions on covariates depending on the class of model. This dissertation will utilize the mixed proportional hazard model discussed in detail by Abbring and van den Berg (2003) and van den Berg (2005) and rely on identification from the continuous variables in the model which will be discussed in detail in the next chapter. Additionally, in the next section, which presents the full model, a potential exclusion restriction will be illustrated even though exclusion restrictions are not necessary in this class of models. None of the identification results presented above require such an exclusion restriction as the same set of covariates can be allowed to affect both durations.

As one can see from this introduction to the necessary components of a competing risks hazard model implementation, there are many issues and distributional decisions to make in constructing a general, theoretically consistent model to estimate land conversion decision with multiple exit states.

3.4 A Land Use Application of the Competing Risks Model

Utilizing data on the timing of preservation, T_p, and the timing of conversion, T_c, combined with parcel attributes, X, the proposed hypotheses, preservation qualification's influence on development decisions, will be tested and the size of any effect can be estimated. This work closely follows Lillard (1993), Fallick and Ryu (2007), Gordon (2002), McCall (1996), Steele (2003), Deng (2000) and Abbring with van den Berg (2003). However it is the first such implementation in a land use context.

The hazard for the preservation rate as given by

$$\lambda^{p}(t \mid X, z, v_{p}) = \lambda_{0}^{p}(t) I(X) \exp\left(X\beta_{p} + \alpha' z\right) v_{p} \tag{17}$$

where *I(X)* is an indicator function that equals 1 if the parcel is known to be eligible for the preservation program as defined by eligibility criteria and characteristics of the parcel, $\lambda_{0}^{p}(t)$ is the baseline hazard of preservation, and $\lambda^{p}(t \mid X, z, v_{p})$ is the instantaneous hazard rate. Covariates in *X* include parcel attributes and locational attributes of surrounding land use. The covariate *z* is a dummy variable equal to one in the years the program was funded and zero otherwise. Finally the parameter v_{p} in (17) is the parameter representing unobserved heterogeneity in the preservation hazard process.

The conversion hazard is as follows,

$$\lambda^{c}(t \mid X, Y, v_{c}) = \lambda_{0}^{c}(t) \exp(\beta_{c}' X + \gamma' Y) v_{c} \ , \tag{18}$$

where X includes all variables in common with the preservation hazard, *Y* includes variables expected to influence the conversion hazard through conversion costs or development pressures, including the options variables described in the theoretical model, and v_{c} is the unobserved heterogeneity parameter specific to the conversion hazard. Variables included in *X* and *Y* can, and do, change across time. However the unobservable parameters, v_{p} and v_{c} , are observation-specific and not time-specific. As has been true throughout, time subscripts are omitted to reduce notational complexity, but *X, Y,* and *z* all contain time varying covariates.

In the competing risks framework these hazard rates will be jointly estimated. The funding dummy present in the preservation hazard should impact the preservation decision but not the conversion decision directly and thus acts as an exclusion restriction. In a similar framework, Fallick and Ryu (2003) argue that a term such as *I(X)* serves as an additional exclusion restriction because the eligibility criteria is forced to have a larger impact on the preservation hazard due to prior knowledge of the world. The specification implies eligible parcels can be delineated *a priori* using observable covariates. In this case the preservation eligibility criteria are well documented by the county and involve observable data.

The system of equations in log form is rewritten as

$$\begin{aligned} \ln \lambda^c(t) &= \beta_0' H_c(t) + \beta^{c\prime} X + \gamma' Y + \ln v_c \\ \ln \lambda^p(t) &= \varphi_0' H_p(t) + \beta^{p\prime} X + \alpha' z + \ln v_p \end{aligned} \tag{19}$$

where the baseline hazards $\lambda_0^c(t)$ and $\lambda_0^p(t)$ are replaced by functions of time, $\beta_0' H_c(t)$ and $\varphi_0' H_p(t)$, which can be estimated parametrically, by defining $H_i(t)$, or non-parametrically. Parametric estimation involves using a distributional assumption on the baseline hazard, and non-parametric estimation is most often accomplished via a piecewise linear baseline suggested by Han and Hausman.[27] Selection of these distributional assumptions will be described in the next section. To estimate the dependent competing risks model the parameters v_p and v_c are allowed to be correlated and estimate the correlation parameter, $\rho_{v_c v_p}$, jointly in the model.

[27] In fact, Han and Hausman suggest that non-parametrically estimating the baseline hazard absorbs some variation being picked up by the unobserved heterogeneity term in Heckman and Singer. They argue that the restrictive parametric baseline hazard imposed by Heckman and Singer is a contributing factor in the finding of influential unobserved heterogeneity. This suggests a potential tradeoff between a parametric restriction on the baseline versus a parametric restriction on the heterogeneity term.

To compose the likelihood function there are three cases to consider—the parcel converts, the parcel preserves, or the parcel does not change status.

i.) The contribution to the likelihood function for parcels that convert, where *t* is the conversion period, is given by the following[28]:

$$\Pr(t-1 < T_c \le t, T_p > t-1) = E_{v_c, v_p}\left[\Pr(t-1 < T_c \le t, T_p > t-1 \mid X, Y, z, v_c, v_p)\right];$$

ii.) the contributions for parcels that preserve (where *t* is the period in which the preservation takes place) is:

$$\Pr(t-1 < T_p \le t, T_c > t-1) = E_{v_c, v_p}[\Pr(t-1 < T_p \le t, T_c > t-1 \mid X, Y, z, v_c, v_p)]$$

iii.) and the contribution for parcels that remain in the current state for the duration of the study period is:

$$\Pr(T_p > t, T_c > t) = E_{v_c, v_p}[\Pr(T_p > t, T_c > t \mid X, Y, z, v_c, v_p)],$$

where *E* is the expectation operator.

The integrated hazard rates for preservation and conversion are defined as

(20) $\Lambda^p(t \mid X, z, v_p) = \int_0^t \lambda^p(s \mid X, z, v_p)ds$ and

$$\Lambda^c(t \mid X, Y, v_c) = \int_0^t \lambda^c(s \mid X, Y, v_c)ds.$$

Assuming variables in *X*, *Y*, and z are constant within each year of the data, even though they may vary across years, these integrations reduce to the summations,[29]

---

[28] Technically the probability $\Pr(t-1 < T_c \le t, T_p > t-1)$ is an approximation of the true probability, $\Pr(t-1 < T_c \le t, T_p > T_c)$. This approximation is used because the true probability, which is the probability of receiving a preservation and a conversion offer in the same year, is quite involved to compute and would be a rare occurrence.

[29] Due to the discrete nature of the data this assumption must be made.

$$\Lambda^p(t \mid X, z, v_p) = \sum_{s=1}^{t} \lambda^p(s \mid X, z, v_p) \text{ and } \Lambda^c(t \mid X, Y, v_c) = \sum_{s=1}^{t} \lambda^c(s \mid X, Y, v_c).$$

The survivor function is equal to the exponentiated negative integrated hazard, so that the survivor functions are

$$\begin{aligned} S^p(t \mid X, z, v_p) &= \Pr(T_p > t \mid X, z, v_p) = \exp[-\Lambda^p(t \mid X, z, v_p)] \\ S^c(t \mid X, Y, v_c) &= \Pr(T_c > t \mid X, Y, v_c) = \exp[-\Lambda^c(t \mid X, Y, v_c)] \end{aligned}.$$

Now if conditional independence based on the observed data and inclusion of unobservables in the model is assumed, the probabilities i. through iii. can be rewritten as

i′) $$\begin{aligned} &\Pr(t-1 < T_c \le t, T_p > t-1 \mid X, Y, v_p, v_c) = \\ &\quad [S^c(t-1 \mid X, Y, v_c) - S^c(t \mid X, Y, v_c)] S^p(t-1 \mid X, z, v_p) = \\ &\quad\quad f^c(t \mid X, Y, z, v_p, v_c) \end{aligned}$$

ii′) $$\begin{aligned} &\Pr(t-1 < T_p \le t, T_c > t-1 \mid X, z, v_p, v_c) = \\ &\quad [S^p(t-1 \mid X, z, v_p) - S^p(t \mid X, z, v_p)] S^c(t-1 \mid X, Y, v_c) = \\ &\quad\quad f^p(t \mid X, Y, z, v_p, v_c) \end{aligned}$$

iii′) $\Pr(T_p > t, T_c > t \mid X, z, v_p, v_c) = S^p(t \mid X, z, v_p) S^c(t \mid X, Y, v_c)$

Since the realization of (v_c, v_p) is not observed the expectation of these quantities must be taken with respect to the stochastic nature of (v_c, v_p). In the hazard literature this is accomplished by making a parametric assumption or using a bivariate discrete distribution following Heckman and Singer. For this presentation, bivariate normal distribution is assumed to estimate the competing risks model.[30]

[30] Distributional assumptions will be described and validated, where possible, in the next section of this chapter.

The heterogeneity components are assumed jointly normally distributed as follows

$$\begin{pmatrix} v_c \\ v_p \end{pmatrix} \sim N\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_{v_c}^2 & \sigma_{v_c v_p} \\ \sigma_{v_c v_p} & \sigma_{v_p}^2 \end{pmatrix}\right) \text{ where } \sigma_{v_c v_p} = \rho_{v_c v_p} \sigma_{v_c} \sigma_{v_p} .$$

This assumption requires the data to be modeled as continuous data which is often done in hazard models if the distribution of failures within the discrete step is not important to the analysis. In this dissertation information on the month of conversion will be used and continuity assumed, so this assumption is not overly restrictive.

Embedded in this model is the assumption that, conditional on the unobserved heterogeneity, the marginal density functions for the failure times are independent. Defining the term $\chi(t)$ as representing the covariate paths of *z*, *Y*, and *X* from the beginning of the study period, the distribution of failure times for parcel *i* is given by

$$f_i^p(t^p, \chi(t^p), v_p) = S_i^p(t^p, \chi(t^p), v_p) \lambda_i^p(t^p, X(t^p), z(t^p), v_p)$$

for a parcel that preserves, and

$$f_i^c(t^c, \chi(t^c), v_c) = S_i^c(t^c, \chi(t^c), v_c) \lambda_i^c(t^c, X(t^c), Y(t^c), v_c)$$

for a parcel that converts. Given this conditional independence, censored parcels (those remaining in the risk set at the end of the study period) are represented by their survival functions

$$S_i^p(t^p, \chi(t^p), v_p) S_i^c(t^c, \chi(t^c), v_c) .$$

The likelihood function in this case is

$$(21)\; L = \prod_{n=1}^{N} \int_{v_p} \int_{v_c} \lambda_n^p(t^p, \chi(t^p), v_p)^{\delta_{pn}} \lambda_n^c(t^c, \chi(t^c), v_c)^{\delta_{cn}}$$

$$* S_n^c(t^c, \chi(t^c), v_c) S_n^p(t^p, \chi(t^p), v_p) f(v_p, v_c) dv_p dv_c$$

where $\delta_{pn} = 1$ if the parcel preserves and $\delta_{cn} = 1$ if the parcel converts. The term $f(v_p, v_c)$ is the *pdf* of the bivariate frailty distribution. This parameterization of the frailty distribution requires numerical integration to compute the likelihood function. This framework allows the impact of covariates to be measured in the time dimension and in the presence of the preservation program where the preservation exit is modeled explicitly and can influence the outcome of the conversion hazard. That is, the decision to exit the risk set by preserving the parcel is not assumed to be simply a case of random censoring. Estimation for these complex models is accomplished using aML, statistical software specifically designed to estimate correlated outcomes and joint models (Lillard and Panis, 2003).

This general competing risks model addresses both unobserved heterogeneity and non-random censoring and can be very general. However a parametric assumption is still required for the baseline hazard and the heterogeneity parameters to facilitate estimation. The next section will describe the data used in the model and in subsequent chapter will illustrate the various methods to inform selection of the baseline distribution and the distribution of unobserved heterogeneity.

4. Data

4.1 Risk Set and Outcome Variables

The data for this study include all parcels in Howard County, Maryland that, as of the end of 1990, were eligible to be subdivided into at least three new housing lots. The latter criterion avoids counting the development of family lots as a conversion of farmland to residential use. The process by which these parcels were identified was quite complex and included two components. The first component involved identifying all actual subdivision activity during the study period and the second required classifying parcels that had not been developed during the study period as either potentially ‘developable’ or not developable.

The process of identifying subdivisions began with an examination of a series of snapshots (taken in 1993, 1995, 1997 and 2001) of the tax assessment data base for Howard County for clues that would link disappearing parcels with newly appearing housing lots. A variable called a ‘record creation date’ became a key feature of the investigation, as it is this variable that helps establish the date (month and year) at which an identified parcel subdivision took place. Because the record creation date variable was not included in the data base prior to 1991, the study period must begin at that point. Prior subdivisions are identifiable, but their conversion dates are not known with precision.

Subsequent to this initial investigation using the non-spatial tax assessment data base, a geocoded version of the Howard County tax map (as of 2003) became available, including the digitized boundaries of parcels and account identifiers that linked to tax assessment data base attributes. This new data source allowed a means to check and correct prior subdivision assignments. It also provided digitized boundaries of all parcels that made up the final observation set. A second source of subdivision data came directly from the Howard County planners office which included the actual county database used to track large subdivisions from application to approval. Though this list was not geocoded or well-organized, some account identifiers facilitated cross-checking of previous subdivision assignments. The final product of this first component was a map of subdivision activity that took place between 1991 and 2001, including the boundaries of the final housing lots and the reconstructed boundaries of the original pre-subdivision parcels.

Once the actually developed parcels had been identified, all potentially developable parcels had to be added to the observation set. These were defined using attribute data for the parcel and existing zoning regulations. Undeveloped parcels eligible for inclusion in the observation set were those zoned in a way that allowed for residential development and those with capacity for at least three additional housing units, given maximum density regulations applicable to the parcel. Parcels with existing houses were included as long as they met this criterion. Zoning criteria were obtained from Howard County's 1992 zoning ordinance. The zoning categories relevant to each parcel were determined from the tax assessment data base and, where missing, from the digitized zoning map available from the Maryland Department of Planning.[31]

[31] Current data are available from http://www.mdp.state.md.us/.

Further eliminations were made from this initial set. Included in these deletions was land preserved through preservation or conservation activity prior to 1991, as well as wildlife sanctuaries, parks and other prior public acquisitions. Parcels were also deleted from the observation set if their shape precluded reasonable subdivision. The resulting dataset represents the county landscape as of 1990 and the conversion history from 1991 to 2001. The conversion (failure) time is defined as the date the lots of a subdivision were recorded. In the case where a parcel converted but left a portion of land large enough to be further subdivided this portion is redefined as a new parcel and remains in the risk set.

While all parcels eligible for preservation must also be developable, not all potentially developable parcels had the option to preserve, only those meeting quite specific eligibility criteria. Because the 100 minimum acre limit was relaxed for parcels adjacent to already preserved or protected land, smaller parcels that became eligible as adjoining parcels were preserved were added to the eligibility pool during the study period where appropriate.

The final data set includes 1,756 parcels totaling 43,300 acres. The distribution of these parcels is displayed in Figure 4.1. Of these parcels 258 were eligible for preservation at some time during the study period and 59 enrolled in the preservation program. Each of these sets of parcels is shown in Figure 4.2. The distribution of the 463 subdivided parcels is shown in Figure 4.3. Of the subdivided parcels in the final data set, 57 were eligible to preserve which illustrates the competing nature of preservation and conversion in the county. This geocoded spatial data set represents a unique and rich view of the land conversion process across space and time in a rapidly developing county.

Figure 4.1 Distribution of the "at risk" parcels

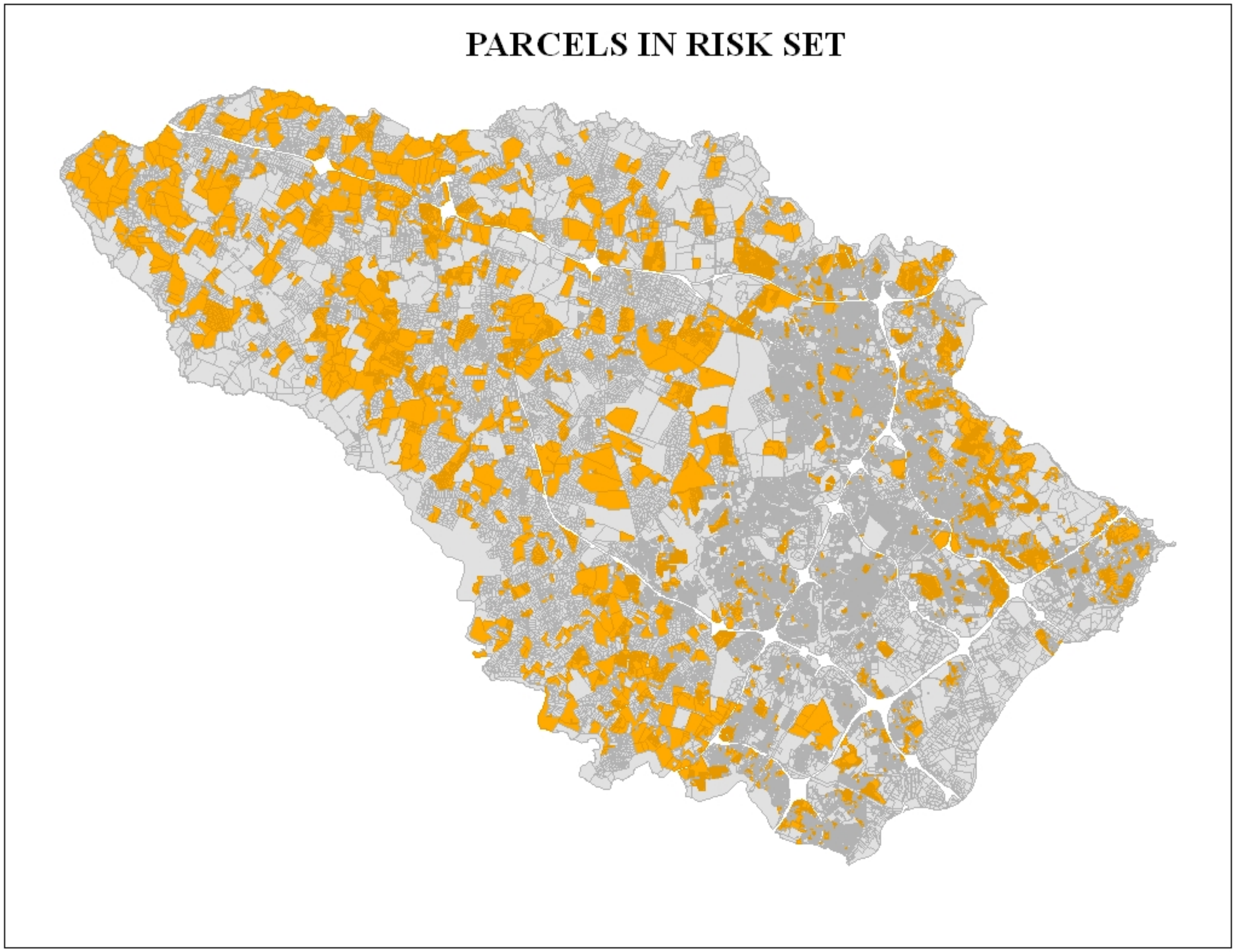


Figure 4.2 Distribution of preservation eligibility and activity

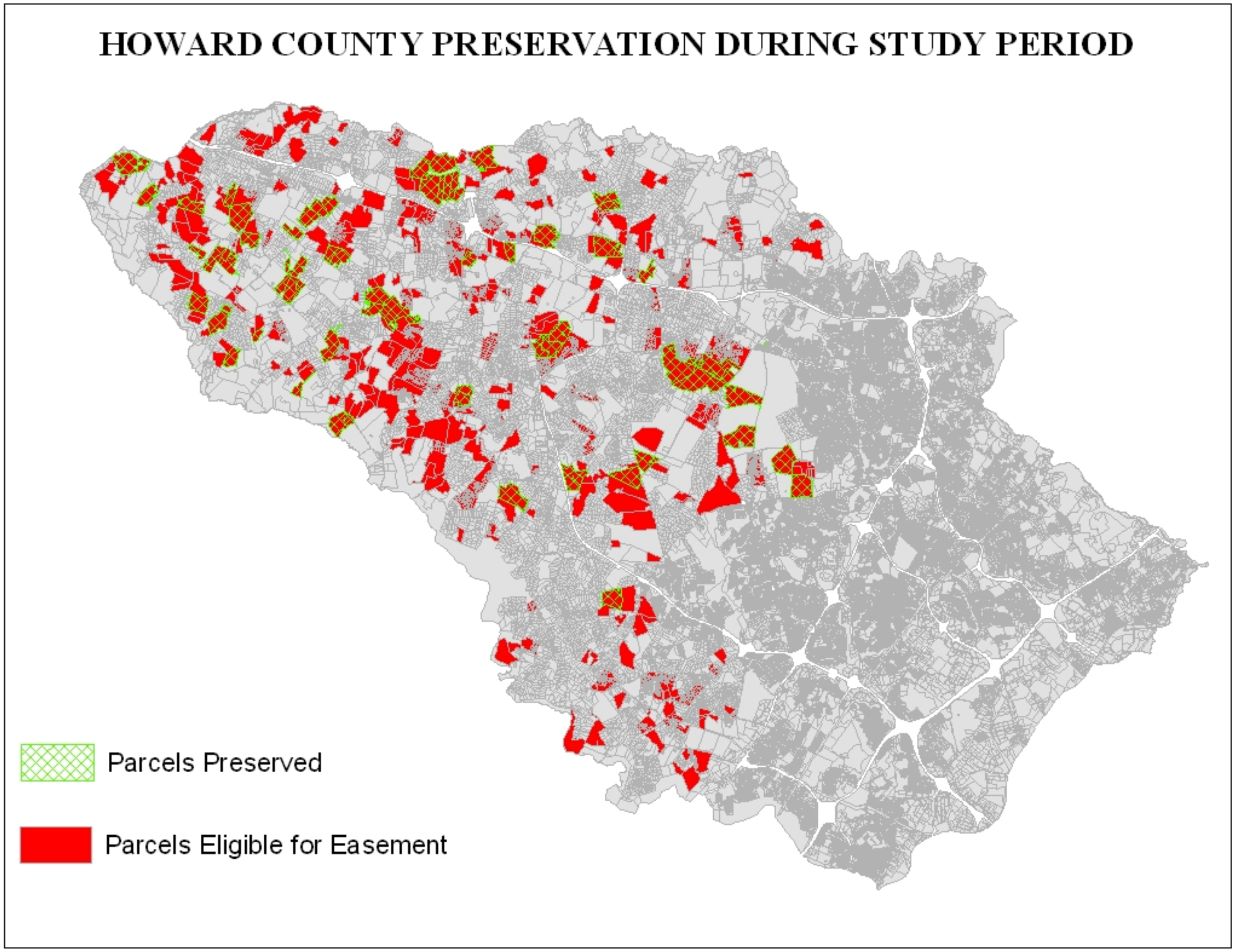
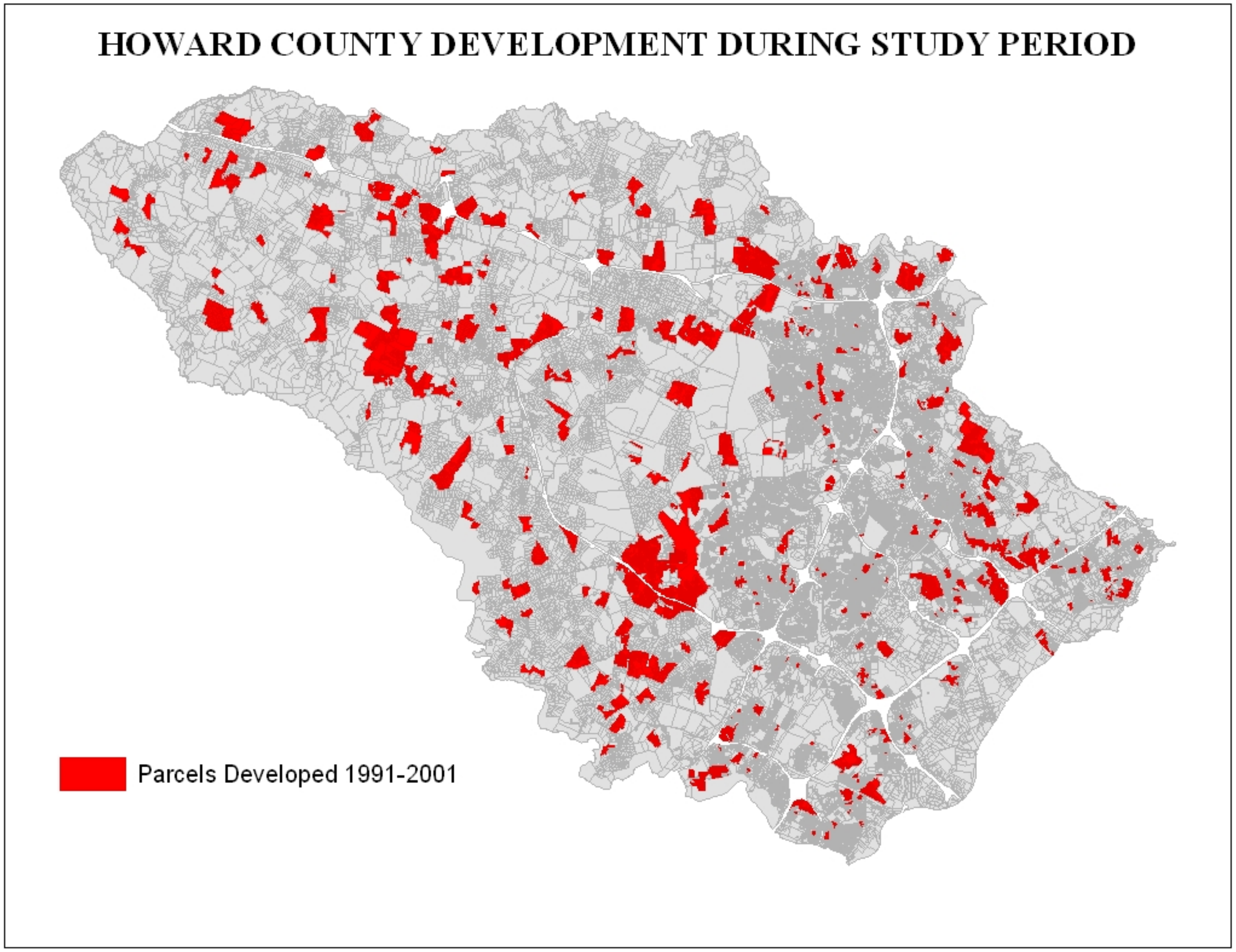


Figure 4.3 Distribution of conversion activity



4.2 Explanatory Variables

Data to populate the *X* and *Y* vectors for the model developed in Chapter 3 were obtained from a number of sources. Variables pertaining to the *X* and *Y* vectors for the development risk will be discussed first followed by a discussion of the *X* vector for the preservation risk.

Consistent with the story presented in earlier chapters, factors that vary over parcels and are expected to shift the hazard of development include characteristics that make the location more or less desirable for residential use, those that affect agricultural productivity (and thus opportunity costs), and those that cause some parcels to be developable at lower cost. Within the first category, arguably the most commonly considered are commuting costs to major employment centers – Baltimore, MD (*distBA*) and Washington, D.C. (*distDC*). These distances were measured from the centroid of each parcel in the observation set, along the county roads network, to the centroid of each of the two cities using ARC/INFO software. The geocoded roads network used is a product of the Maryland Department of Transportation.

If surrounding land use has an effect on the value of a parcel in a given land use, then it follows that surrounding land use will also have an effect on the likelihood of a parcel being developed. In the presence of such *interaction effects*, policies that alter development decisions can alter the likelihood of development of parcels other than those directly affected by the policy. Simply stated surrounding land uses will impact the value of a parcel in development because people care about what is near to their house. People select homes based, in part, on the surroundings. Some prefer forests while others parks, and some prefer to live in densely populated areas, while others prefer less congestion. Additionally, the existence of interactions among neighboring parcels leads logically to path dependence in land use pattern change. For these reasons measures of the surrounding land uses are included as variables in the hazard model to control for these interaction effects with neighboring parcels.

In measuring the surrounding land use variables, land uses are aggregated into ten categories: developable land with an existing house (e.g. a farmstead) (*sluDevWithHs*), developable land without an existing house (*sluDevNoHs*), commercial/industrial/institutional use (*sluComm*), subdivided land (but not yet built on) (*sluSubdiv*), preserved land (*sluPreserved*), private not developable openspace (*sluOpen*), roads (*sluRoad*), protected land (e.g. publicly supplied open space) (*sluProtected*), and "fully developed" land in residential use (the normalized category). The surrounding land use measures are calculated as percentages of land within a 100 meter buffer around the true boundary of each parcel and are calculated using the parcel boundary GIS data from Howard County, reconstructed to reflect land uses in each year. Land use designations are assigned to each parcel in Howard County based on parcel attribute data, largely from the tax assessment data base. Specifically, the existence of structures and their date of construction can be determined from the Howard County GIS layers and from the Tax Assessment database (TA). Residential uses are designated by the TA's land use code and by mass appraisal data in the TA database that describes each house. Non-residential land uses are further categorized based on the TA's land use code and TA variables describing the specific use of properties that are commercial or tax exempt at a highly refined level. As examples, exempt classes include schools, churches, government office buildings, public works operations, etc. Commercial and industrial uses include retail stores, manufacturing plants, warehouses, office buildings, etc.

Although surrounding land use results are sensitive to the use of larger radii, e.g. 400 and 800 meters, estimated coefficients associated with the main variables of interest in the models turn out not to be sensitive to the buffer size. The surrounding land use measures are updated in each analysis year as neighboring parcels are converted, preserved, or built upon.

Each parcel in the risk set is assigned to a Census block group and tract by overlaying the 1990 Census maps on the parcel boundary map. Where parcels overlap Census areas, the area with the largest share of the parcel is assigned. Two variables are then constructed using the Census boundaries. First, by overlaying the recent sales data from the Howard County tax assessment database on the Census block group map a spatially distinct variable for recent construction activity (*devRate*) is constructed. This variable is calculated as the percentage increase in housing stock by Census block group from the previous year and is included to capture the influence of recent construction activity.[32] The recent construction activity serves at least two purposes – a) it picks up some desirability not fully measured by the distance or surrounding land use attributes and b) it proxies for areas of the county into which the planners' office may be attempting to funnel new development. The second Census

[32] This is calculated as new construction in year *t* divided by existing units as of the beginning of year *t*.

related variable is the density of housing (*popDen*), measured as number of housing units per acre at the Census tract level and included to proxy for congestion and other amenities/disamenities of the landscape that are correlated with density of residential housing at a larger spatial scale than the immediate neighborhood.

To proxy for construction costs a measure of parcel slope (*steep*), the percentage of the parcel with a slope greater than 15%, is calculated from the natural soils maps from NRCS. Steep sloped parcels are less likely to be suitable land for agriculture. The impact on development is unknown because steep slopes may proxy for parcels with scenic views, but steep slopes increase construction costs and can invoke erosion control regulations. Other construction related variables are obtained from the site development engineering tables of the Soil Survey Geographic Database (SSURGO) from NRCS. These are the road suitability (*notRoadSuit*) and septic suitability (*notSepticSuit*) variables where both are represented in the dataset as percentage of the parcel in the "very limited" category. The classifications combine data on soil type, slope, permeability, bedrock depth, saturated zone depth, and frost action to estimate these construction indices.

The 1990 Maryland Department of Planning (MDP) maps provide data for the percentage of each parcel's land cover in forest (*forested*) and agricultural (*agriculture*) use. Agriculture is defined as field, forage, or row crops and forest cover as deciduous, evergreen, or mixed forest. The status of sewer construction (*sewerPlnd*) is also available from MDP data and is coded as a dummy variable equal to one if sewer service does not exist but is planned for the near future. The variable (*hasHouse*) is constructed using a Howard County GIS layer of structures and the Tax

Assessment database. The variables is set equal to one for parcels with an existing house, with updating taking place each year of the analysis to reflect new home construction. This variable for existence of a house is expected to negatively influence the conversion decision, particularly for small parcels, due to the unobserved location of the house on the parcel. For example, a parcel that can yield 3 additional units based on acreage and zoning is included in the dataset, but if there is an existing house and it is located centrally on the parcel may not be possible to add three new housing units without removing the existing structure. Additionally, parcel owners who live on their property have recreational and aesthetic uses for their land and thus may be less likely to subdivide than owners who do not live on their parcels. The final 'construction cost' variable is the 3 month Treasury bill rate (*intRate*), included as an indicator of the cost of carrying the land from the time the development process is initiated until the lots are sold. This last variable varies only over time but not across observations.

A number of regulations affect various aspects of the above factors. These regulations are treated as exogenous, given that the Howard County Comprehensive Plan and zoning codes were passed in 1990 and remained static through the study period. The effects of zoning are measured by the number of lots that the parcel can be divided into (*numLots*) and the existence of open space set-aside requirements (*reqOpenSpace*). Finally, in a few planning areas in the county, development activity was capped by adequate public facilities moratoria in some of the study period years. The variable (*Apfo*) equals zero or one, respectively, for any year in which the parcel is in a planning area constrained by an adequate public facilities moratorium relating to school capacity for none or any part of the year. This variable is updated as adequate public facilities moratoria are introduced and phased out. The *Apfo* data are compiled using legislative records and a subdivision tracking database from the Howard County Planning Department.

To capture the effect of returns in an agricultural use on the hazard of development, the percent of the parcel in each of four soil quality classes (*classes 1 through 4)* from the land capability class (LCC) are included*.* LCC is a composite index from USDA representing many factors such as slope, soil type and others that are important to the suitability of land for agricultural use. The effect of these variables is measured relative to the worst soils for agriculture (the excluded soil category). Although the soil classifications are intended to proxy for potential agricultural returns, good agricultural soils can also be favorable soils for development, so the expected effect on the hazard rate is ambiguous. Another measure relating to agricultural returns is parcel size (*acres* and $acres^2$). The likely effect this has on the hazard rate is also ambiguous, as economies of scale may be evident in both farming and development.

Variables that impact preservation eligibility (the *X* vector) are included in the preservation hazard. Recall these requirements are defined by the county exogenously based on soil quality, acreage, viability as a farm operation, and contribution to the farm sector. Thus variables such as acreage (*acres*), agricultural or forest use (*agriculture* or *forested*), terrain of the parcel (s*teep*), and soil classes (*class1-4*) are included in the hazard model for preservation

Interaction effects with surrounding land uses are expected to be prevalent in the preservation hazard as well as in the conversion hazard especially since many of the preservation requirements are enhanced or depend upon the neighboring land uses. It is assumed that landowners may self-select for preservation due to development or preservation activity on surrounding parcels, and additionally, that the county may choose to enroll a parcel which applies for the preservation program based on the activity surrounding the applying parcel, i.e. development pressures or agricultural uses. Thus surrounding land use measures are included in the preservation hazard.

A dummy variable for the presence of an existing house is included (*hasHouse*) as is a dummy variable equal to one in years the county has funding (*funded*) for the preservation program which is the z variable described in the previous chapter. These years are 1991-92, 1994-96, and 2000-01. As mentioned in the first chapter the program exhausted forecasted funds in 1993 and again in 1997-99. Even in the years of limited funding the program remained active and was never considered for cancellation.[33]

The options related covariates, defined in detail in the next section, are not included in the preservation hazard. The compensation formula for the preservation option does not change during the time period and thus presents no variability to impact the preservation time. The full variable list is given in Table 4.1 for both the X and Y matrices which pertain to both the development and preservation hazards. Summary statistics for all the explanatory variables used in this analysis are presented in Table 4.2 and a detailed listing of sources is presented in Appendix B. Time

[33] The state preservation program was an active option throughout the period as well.

varying covariates are updated on a yearly schedule because it is often the case that exact months of changes are not available.

Table 4.1 Variables used in Competing Risks model, by Risk (or Hazard)

Variables	Preservation Hazard	Conversion Hazard
distBA	x	X
distDC	x	x
sluDevWithHs	x	x
sluDevNoHs	x	x
sluComm	x	x
sluSubdiv	x	x
sluPreserved	x	x
sluOpen	x	x
sluRoad	x	x
sluProtected	x	x
sluExempt	x	x
popDen		x
numLots		x
reqOpenSpace		x
Funding	x	
Opportunity costs		
class1	x	x
class2	x	x
class3	x	x
class4	x	x
Agriculture	x	x
Acres	x	x
hasHouse	x	x
Conversion costs		
Steep		x
Forested		x
notRoadSuit		x
notSepticSuit		x
sewerPlanned		x
intRate		x
Apfo		x
Options variables		
Drift measure		x
Variance measure		x
Easement		x

Table 4.2 Summary Statistics

Variable	Description	Mean	S.D.	Min	Max
	Options Variables				
Easement*	Qualified for easement	0.093	0.287	0.000	1.000
Variance measure*	Standard error of sales price	16.751	5.695	8.022	30.909
Drift measure*	Drift in sales price	0.645	3.860	-11.025	11.831
	Returns to Development				
distBA	Distance to Baltimore, in km	28.349	10.555	10.935	72.959
distDC	Distance to DC, in km	47.827	9.072	29.511	68.782
sluDevWithHs*	% surrounding land use not fully developed with a house	17.451	19.651	0	96.169
sluDevNoHs*	% surrounding land use not fully developed with a house	11.067	14.268	0	97.781
sluComm*	% surrounding land use in commercial / institutional	3.669	9.087	0	75.369
sluSubdiv*	% surrounding land use subdivided	3.885	6.045	0	69.353
sluPreserved*	% surrounding land use enrolled in preservation program	4.850	13.576	0	94.182
sluOpen*	% surrounding land use in openspace	2.676	6.987	0	93.385
sluRoad*	% surrounding land use in roads	11.028	10.904	0	71.265
sluProtected*	% surrounding land use in protected status	6.747	13.429	0	95.154
sluExempt*	% surrounding land use in tax exempt status	2.409	6.981	0	66.841

devRate*	% of new construction added by block group	4.371	3.687	0	20.292
popDen	# households per acre by census tract, 1990	.538	.458	.069	2.363
numLots	# lots allowed per zoning regs	16.637	35.492	3	700
reqOpenSpace	=1 if open space required, 0 if no	.882	.322	0	1
	Opportunity Costs				
class1	% of parcel with class 1 soils (prime)	1.177	4.824	0	66.971
class2	% of parcel with class 2 soils	44.862	31.481	0	100
class3	% of parcel with class 3 soils	32.234	28.986	0	100
class4	% of parcel with class 4 soils	9.597	18.515	0	100
Agriculture	% of parcel in crops	29.084	37.171	0	100
Acres	Parcel size, in acres	2.231	3.844	.751	798.465
hasHouse	existing house on parcel	.640	.480	0	1
	Construction Costs				
steep	% of parcel with steep slopes	16.504	31.213	0	100
forested	% of parcel in forest cover	36.695	36.650	0	100
notRoadSuit	% of parcel not road suitable	43.952	30.413	0	100
notSepticSuit	% of parcel not septic suitable	60.905	34.404	0	100
sewerPlanned*	Sewer planned in next 10 years	.149	.357	0	1
intRate*	Annualized 3 month T bill rate	4.55	.901	2.998	5.820
APFO*	=1 if restricted by adequate public facilities moratoria	.134	.341	0	1
	Number of observations	16,116(1756 parcels)			

* - Time varying covariate

Sources: Maryland Department of Planning; Maryland Department of Assessments and Taxation; Howard County Department of Planning; U.S. Census Bureau

4.2.1 Options Variables

The principle empirical task is to test whether the presence of the easement option delays development. The variable (*Easement*) equals one in the years a parcel is eligible to sell a preservation easement *and* in which a county preservation program budget exists to purchase easements. This variable is updated for parcels that became eligible during the study period due to prior preservation of adjacent parcels.

The drift and variance variables described in Chapter 2 are intended to capture the effects of uncertainty on development timing decisions. Landowners/developers are assumed to form expectations on returns from development based on recent new house sales in the same geographic and socioeconomic vicinity. Therefore, the drift and variance variables are constructed using a separate dataset of sales of new and existing houses – all of which were built within the last 10 years. Sales in which price exceeded two standard deviations from the Census tract average for the year were omitted in order to eliminate the undue influence of outliers whose special characteristics were not measurable. After eliminating these outliers, non-arms length sales, and clearly mistyped entries, 37,085 observations remained.

The drift variable for any given tract and year was calculated as the average rate of growth in deflated lot price for sales within the tract over the 3 previous years[34], corrected for some principle sources of price variation. For example, a landowner/developer forming expectations on the drift and variance in returns in

[34] Pooled lagged sales prices from 2, 3, 4, and 5 years were also tested with remarkably consistent results in terms of the magnitude of variance. As one would expect the drift calculation stabilized and moved off zero as a mean value with the inclusion of more years.

order to make an investment decision in 1996 is assumed to use information on housing sales within the relevant Census tract from the years 1993, 1994, and 1995. The covariates included to account for systematic price variation are the natural logs of each of the following - distance to Washington DC, lot size, square-footage of house - as well as an index for quality of construction, the age of the home at the sale data, and a dummy variable for townhouse. These covariates are represented by the ***W*** vector in equation (22) below.

A separate drift and variance value is calculated for each area of the county (defined by 15 groups of Census tracts) and each year of the analysis (11 years from 1991 through 2001), by estimating 15 regressions for each year of the analysis, one for each Census tract. Thus the impact of the ***W*** variables on price is allowed to vary across the tracts and years within the county. To isolate the drift an implicit temporal effect for each tract is estimated. Specifically, for any analysis year t and census tract the following OLS regression was estimated:

$$(22) \qquad \ln(deflatedSP_{i})_n = \beta_{0n} + \beta_{1n} lagyear_{in} + \vartheta W_{in} + \varepsilon_{in} \quad \forall\ \mathrm{n} = 1,\ldots 15$$

Each regression included all qualifying sales for years $t\text{-}1$, $t\text{-}2$, and $t\text{-}3$. The variable $deflatedSP_i$ is defined as the inflation adjusted sales price in 2000 dollars for the i^{th} sale, and $lagyear_i$ equals s if the i^{th} sale took place in year $t\text{-}4\text{+}s$ $(s\text{=}1,2,3)$. A total of 165 regressions were estimated. The coefficient on (*lagyear*), β_{1n}, becomes the measure for the drift parameter for the n^{th} Census tract group and the t^{th} year of analysis of the regression.

The variance measure is defined as the sum of squared residuals:

$$\frac{\sum_{i=1}^{L_n}(\hat{deflatedSP}_{in} - deflatedSP_{in})^2}{L_n - k} \forall \text{ n} = 1,..,15,$$

where $\hat{deflatedSP}_{in} = \exp\left\{\hat{deflatedSP}_{in} + 0.5\left[\varepsilon_i^2 /(L_n - k)\right]\right\}$ is the variance adjusted expected sales price, L_n is the number of observations in tract n, and k is the number of regressors. This variance is calculated for each of the 165 regressions (15 tracts and 11 analysis years) in the dataset. The absolute *level* of variance is not the desired variable but the percentage of variance relative to sales price is, so this measure is standardized by dividing by the mean sales price in the respective tract.

The average drift for the entire sample is 0.645% and the average standard deviation is 16.75%. Of course, few observations in a year can lead to a high variance - but this is appropriate as it is a signal of the limited information on recent sales with which current landowners can develop their expectations.[35]

In summary, this analysis does not depend on commonly used sources of national land use data, but instead on micro, parcel level data spanning a time horizon of approximately one decade. This extremely rich dataset of time invariant attributes including parcel characteristics and location characteristics combined with time varying attributes such as the options variables, land use, interest rates, development rates, and recording dates for preservation or conversion decisions provides information on the re-construction of the landscape as of the end of 1990 as well as the pattern and timing of land conversion from 1991 to 2001. This dataset allows estimation of a data 'hungry' general model of the timing of land conversion and

[35] Specifications used in this analysis assume constant and homogeneous discount rates (ρ) across landowners, as data limitations preclude controlling for variation in landowner discount rates. The frailty specifications should remove some of the noise from this unobservable.

preservation decisions. The appropriate model incorporates the dynamics inherent in the process of land use change which are often overlooked when estimating land conversion decisions.[36]

---

[36] In fact, many land use studies use the 'current' landscape as the source of data, failing to account for the direct correlation of the housing units within one subdivision. For example a twenty unit subdivision is not twenty different decisions, it is one decision.

5. Empirical Implementation and Results

A necessary step in evaluating the impact of the preservation option on the conversion decision in the competing risks framework is the selection of an appropriate baseline hazard specification and a specification for the unobserved heterogeneity. The best way to select these distributions is to evaluate each risk individually and statistically eliminate specifications, if possible, or heuristically eliminate specifications, if not, by comparing output of parametric and nonparametric models.

5.1 Model Selection – Conversion Risk

The starting point for model selection is to determine if a time dependent baseline hazard is necessary. If not, the exponential baseline, which imposes a time invariant baseline hazard, can be employed. Ideally, a researcher would like to understand a process so completely that there exists no remaining dependency on time, but this is rarely if ever possible.

Throughout this section the estimated models will incorporate covariates as described in Chapter 4. Although the coefficient estimates are important, the coefficient impacts should not be directly interpreted until appropriate specification tests are performed. At this point in the model selection process the covariates are serving two purposes. First and foremost, they control for observable variation in parcels. But secondly, an examination of the differences in coefficient estimates across models, from nonparametric to fully parametric, provides the researcher with an indication of sensitivity to the parametric assumptions imposed by each model.

To determine if a time dependent baseline hazard is necessary a Weibull model, which allows a monotonic baseline, is compared with the exponential baseline, which has no dependency on time. Because the exponential is nested in the Weibull, a likelihood ratio test can be used. Based on this test the null hypothesis of an exponential, "memoryless", hazard is rejected in favor of a time dependent baseline at a critical level of less than 1%.[37]

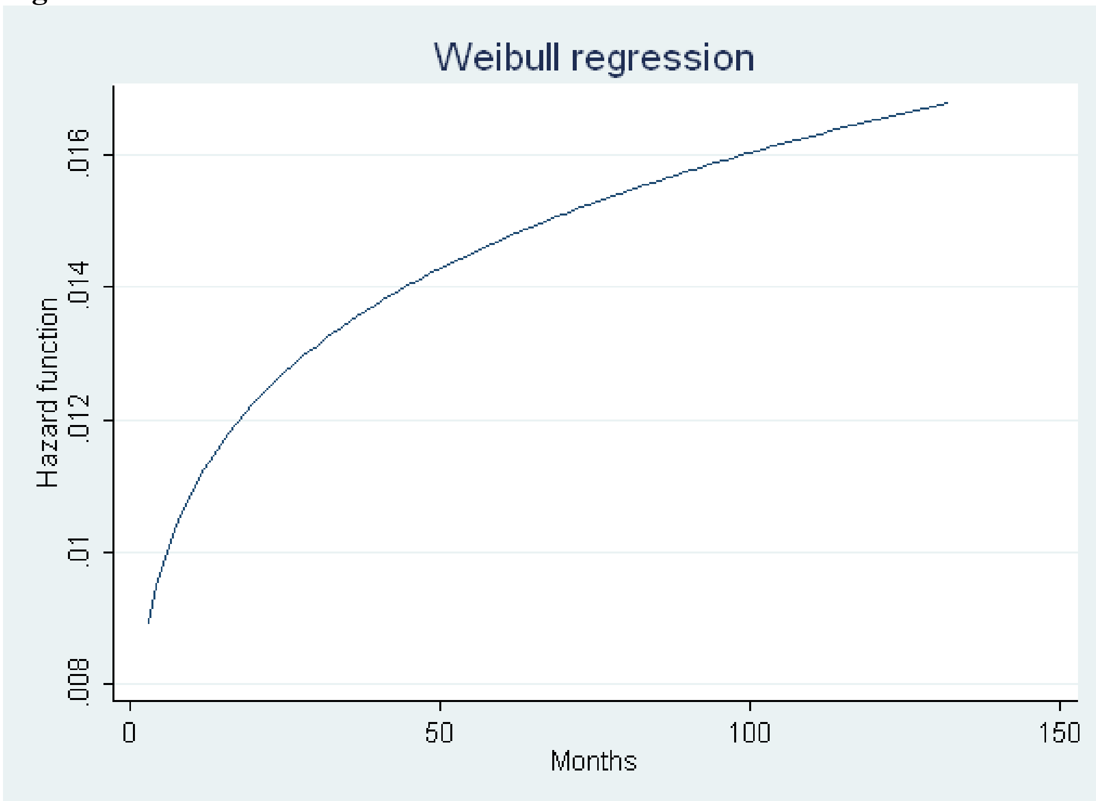
The baseline estimate for the Weibull model is presented in Figure 5.1. The extreme curvature of the Weibull specification in the early periods is unconvincing in the land use context and probably an artifact of the Weibull's behavior around time, *t=0.* It is not likely that parcels in 1990 have a very small baseline hazard rate which increases sharply through the early months. This is especially true as the data for this analysis begin in 1990, not at the beginning of a parcel's lifetime. It seems logical that the hazard rate at the beginning of the study period should be a non-zero rate. The Gompertz specification allows the hazard rate to take non-zero values in the interval around *t=0* and is also a monotonic hazard specification. In Figure 5.2 the estimated Gompertz and Weibull baseline hazard results are portrayed and illustrate the difference in the two in the early years of the study.[38]

---

[37] The null hypothesis is rejected using a likelihood ratio test, chi2(1) = 10.69 at 0.0011.

[38] Also included in Figure 4 are results from the exponential, which imposes a constant baseline hazard. These results are included for a comparison in scale.

Figure 5.1: Weibull Parametric Baseline Hazard Conversion Risk.



The behavior of the Weibull at in the neighborhood of *t=0* was also analyzed by Ridder and Woutersen (2003) who suggest restrictions on the Weibull in mixed proportional hazard models necessary to reach convergence rates similar to the Gompertz. Interestingly, their restrictions amount to bounding the baseline away from 0 or ∞ in the small interval around *t=0*. These facts, combined with the fact that the coefficient estimates associated with model covariates are quite similar between the Weibull and Gompertz models as illustrated in Table 5.1, suggest the Gompertz specification is most appropriate. However, before finalizing the choice of a baseline hazard specification, sensitivity of coefficient estimates under less parametric versions of the baseline hazard is necessary. If coefficient estimates are not stable across parametric and nonparametric estimators it is possible the parametric assumptions are inappropriate.

Figure 5.2: Conversion Hazard - Parametric Baselines.

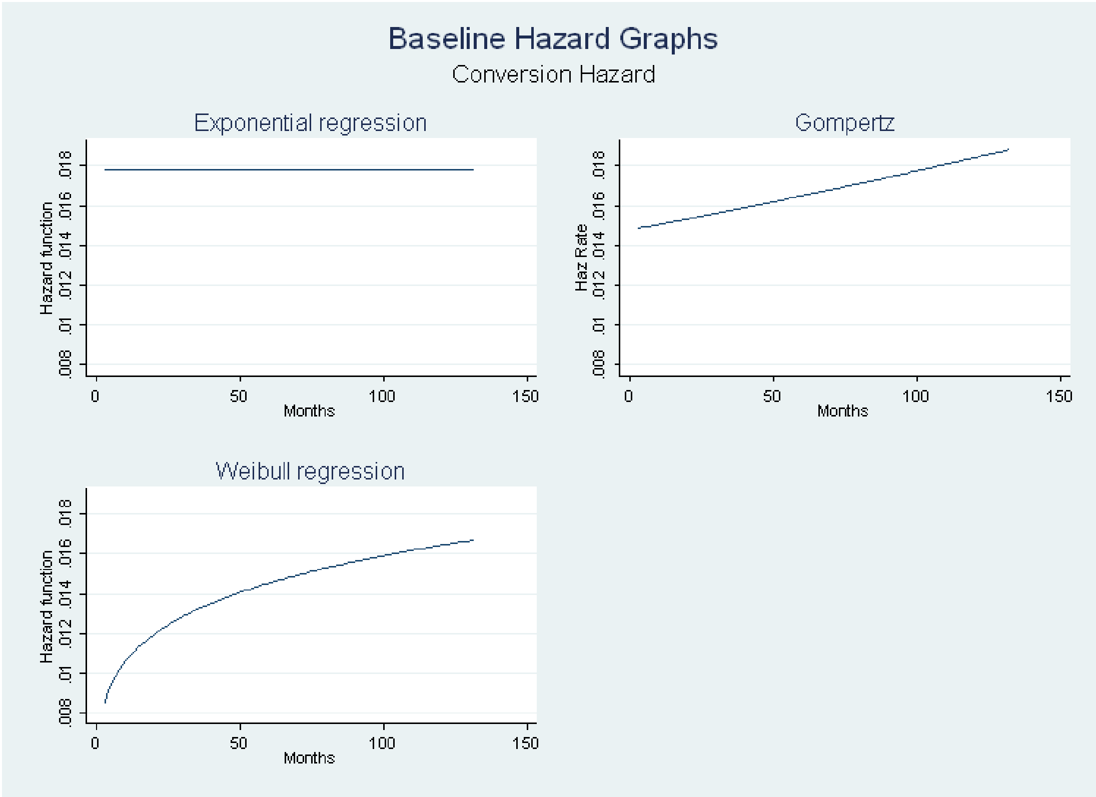


Table 5.1 Parametric Baseline Hazard, Conversion Risk

	Gompertz		Weibull		Exp	
Variables						
Options variables						
Easement	-0.4855	**	-0.4549	*	-0.5123	**
Variance measure	-0.0252	*	-0.0192		-0.0299	**
Drift Measure	-0.0037		0.0072		-0.0042	
Development Returns						
distDC	-0.0103		-0.0093		-0.0112	*
distBA	0.0054		0.0052		0.0057	
sluDevWithHs	-0.0222	**	-0.0222	**	-0.0221	**
sluDevNoHs	-0.0114	**	-0.0102	**	-0.0120	**
sluComm	-0.0113	**	-0.0112	**	-0.0113	**
sluSubdiv	0.0095		0.0118	*	0.0082	
sluPreserved	-0.0201	**	-0.0206	**	-0.0198	**
sluOpen	0.0117		0.0117		0.0119	*
sluRoad	0.0070	*	0.0075	*	0.0066	
sluProtected	-0.0002		-0.0001		-0.0002	
sluExempt	-0.0232	**	-0.0235	**	-0.0231	**
devRate	0.0497	**	0.0567	**	0.0459	**
popDen	0.1735		0.1993		0.1574	
numLots	0.0003		0.0005		0.0002	
reqOpenSpace	-0.1377		-0.1224		-0.1489	
Opportunity costs						
class1	0.0107		0.0105		0.0110	
class2	0.0030		0.0030		0.0029	
class3	0.0010		0.0011		0.0009	
class4	0.0007		0.0009		0.0006	
agriculture	-0.0032		-0.0035		-0.0030	
acres	0.2685	**	0.2676	**	0.2703	**
hasHouse	-0.9187	**	-0.9319	**	-0.9096	**
Conversion costs						
steep	-0.0060	**	-0.0062	**	-0.0059	**
forested	-0.0015		-0.0016		-0.0015	
notRoadSuit	-0.0050	**	-0.0050	**	-0.0050	**
notSepticSuit	-0.0003		-0.0004		-0.0003	
sewerPlanned	-0.7178	**	-0.7045	**	-0.7392	**
intRate	-0.1253	**	-0.1333	**	-0.1098	**
apfo	-0.2014		-0.2564		-0.1375	
Constant	-4.2134	**	-5.1200	**	-4.7333	**
	0.0018		0.1636	**		

** - significant at 5%, * - significant at 10%

5.1.1 Semi-parametric and Nonparametric Specifications

A sensitivity analysis is performed by comparing the fully parametric Gompertz model's coefficient and baseline hazard estimates with estimates from models that incorporate semi-parametric baseline specification and models that abstract from the baseline hazard altogether (i.e. the Cox model). When the pattern of coefficients' signs and significance are similar across models, one can feel more comfortable with the parametric baseline specification.

Semi-parametric estimation of the baseline hazard uses the piecewise exponential specification described in Chapter 3 but allows the resulting "memoryless" baseline to vary across pre-specified intervals of time. The piecewise specification has the advantage of removing temporal unobserved heterogeneity not already represented by time varying covariates in the data. Estimation of this model requires dividing the study period into time intervals which can be done in any of a number of ways. Time intervals are typically groups of sequential time periods in the data, grouped in intervals defined by calendar time or into intervals where an equal number of failures occur. The former method is implemented here by allowing the baseline hazard to be constant over all observations within a calendar year but vary between years. This is accomplished by including a dummy variable for each year in the exponential baseline model. Splitting the data on calendar time puts different numbers of failure events and observations in each interval. The latter procedure of grouping the data such that an equal number of failure events fall into each interval allows the data to 'choose' the interval width. For this method dummy variables are defined such that a fixed percentage of observed failures fall into each time interval.

For this analysis, both approaches are considered. In order to choose the number of intervals for the grouping by number of failures approach, three different sized groups of failures are estimated. The resulting Akaike Information Criteria (AIC) statistic is compared across these three specifications, as well as with the specification which incorporates yearly dummies, in order to select the best fitting model.[39] The intervals for the percentage of failures approach are tested for 4, 6, and 8 groups of failures, and the AIC values from these models are 2622, 2620, and 2614 respectively. The AIC value using the calendar year approach is 2622. Figure 5.3 displays the baseline hazard estimates from each of these models and includes the Gompertz baseline for comparison. From these interval based piecewise estimates, it is easy to see how a few months of high conversion activity in the middle of the study period are represented in the baseline hazard estimation. However, one could argue that the general pattern of the baseline estimates is flat or increasing in time as in the Gompertz baseline. In the remainder of the paper all references to the piecewise model will refer to the model with eight intervals grouped by the number of failure events.

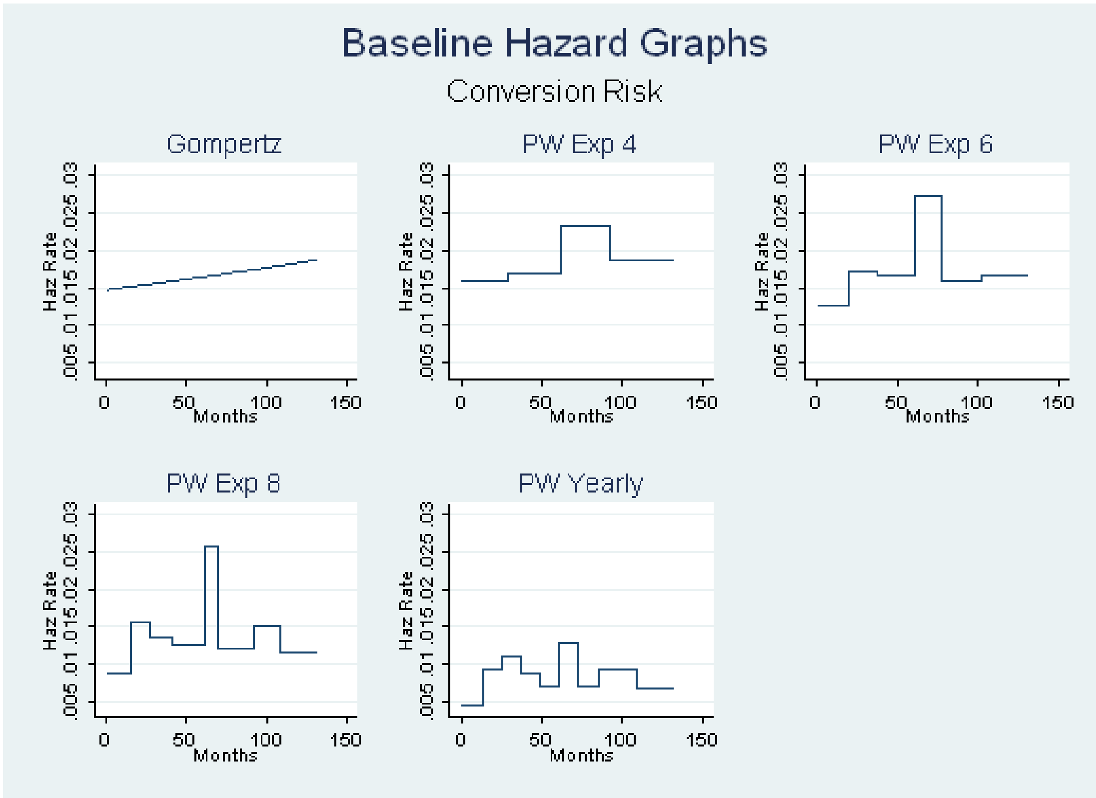
---

[39] Non-nested models such as the Weibull and Gompertz can be compared using the Akaike Information Criteria (AIC) proposed by Akaike (1974) defined as,

$$AIC = -2\ln L + 2(k + c)$$

where $\ln L$ is the log-likelihood value, k is the number of covariates in the model, and c is the number of ancillary parameters. Although the best fitting model typically has the largest log likelihood, the AIC is designed to penalize models with excessive parameters. The most preferred model has the lowest AIC value.

Figure 5.3: Piecewise Baseline Hazards Conversion Risk.



The specification of a baseline can be heuristically validated by comparing the coefficient estimates against the nonparametric alternative, the Cox model. Loosely speaking, if there are dramatic differences in coefficient estimates between the nonparametric and parametric baseline specifications, the imposition of the parametric baseline should be questioned further. If the coefficient estimates are not sensitive to the selection of the parametric model then the researcher can be more confident in the results. Table 5.2 displays the coefficient estimates from the piecewise model and the Cox model. Not only are the estimates similar to one another but the pattern, size, and significance of the estimated coefficients are very similar to those from the fully parametric Gompertz specification presented in Table 5.1. The only major difference is the coefficient on the options parameter measuring drift, which is negative in the parametric Gompertz model and positive in the less parametric models, though not significant in either. The coefficient on easement eligibility and the variance measure are consistently negative and significant in parametric, semi-parametric, and nonparametric models.

Table 5.2 Non/Semi Parametric Results, Conversion Risk

Baseline specification	Cox		PW Exp	
			8 intervals	
Variables				
Options variables				
Easement	-0.5277	**	-0.5289	**
Variance measure	-0.0259	**	-0.0265	*
Drift Measure	0.0185		0.0093	
Development Returns				
distDC	-0.0111	*	-0.0111	*
distBA	0.0054		0.0054	
sluDevWithHs	-0.0219	**	-0.0218	**
sluDevNoHs	-0.0106	**	-0.0107	**
sluComm	-0.0114	**	-0.0113	**
sluSubdiv	0.0108		0.0106	
sluPreserved	-0.0203	**	-0.0201	**
sluOpen	0.0120	**	0.0117	
sluRoad	0.0069		0.0068	*
sluProtected	-0.0003		-0.0002	
sluExempt	-0.0237	**	-0.0235	**
devRate	0.0549	**	0.0544	**
popDen	0.1880		0.1873	
numLots	0.0003		0.0003	
reqOpenSpace	-0.1365		-0.1344	
Opportunity costs				
class1	0.0114		0.0113	
class2	0.0032		0.0032	
class3	0.0011		0.0011	
class4	0.0010		0.0010	
agriculture	-0.0032	*	-0.0032	
acres	0.2745	**	0.2737	**
hasHouse	-0.9207	**	-0.9196	**
Conversion costs				
steep	-0.0062	**	-0.0061	**
forested	-0.0016		-0.0016	
notRoadSuit	-0.0050	**	-0.0050	**
notSepticSuit	-0.0005		-0.0005	
sewerPlanned	-0.7655	**	-0.7625	**
intRate	0.3237		-0.0823	
Apfo	-0.0388		-0.0459	
Constant			-4.7333	**

** - significant at 10%, * - significant at 5%

Results from these baseline hazard specification tests suggest the use of a Gompertz parametric baseline may not be inappropriate, but since the focus of this analysis is on interpreting the coefficient estimates it is probably best to choose the less parametric piecewise model. Should simulation or prediction be important, the Gompertz specification likely provides the necessary structure to permit an acceptable degree of confidence in the results. The remainder of this chapter will explore both the piecewise exponential and the Gompertz baseline hazard specifications.

5.1.2 Unobserved Heterogeneity

A second distributional assumption is required for each risk to implement the dependent competing risks model. This assumption pertains to the distribution of unobserved heterogeneity. Unobserved heterogeneity is likely to exist in any study of human decision making, and as described in Chapter 3 there are many sources of unobserved heterogeneity likely to influence land conversion decisions including parcel attributes and landowner attributes. This section will demonstrate the existence of unobserved heterogeneity in the conversion hazard and attempt to validate a functional form for its distribution.

Recall from equation (15) that the accepted way of including heterogeneity involves a multiplicative term added to the baseline proportional hazard specification which follows a known distribution and adds an estimated parameter for the variance of this distribution to the models. There are many distributions commonly used to represent individual specific unobserved heterogeneity and, as with the baseline specification, there is no reason to choose one over the other. The selection of the final specification for unobserved heterogeneity for the dependent competing risk is accomplished by investigating several distributions and comparing results with nonparametric unobserved heterogeneity. Because it is not possible to validate one specification versus another directly, this part of the model selection process involves analyzing the sensitivity of parametric results versus the nonparametric alternative in the context of the heterogeneity distribution.

Gamma, Log Normal, and finite mixture distributions are estimated for this exercise using both parametric (Gompertz) and semi-parametric (piecewise exponential) forms of the baseline hazard suggested from the previous section. These distributions are selected for the unobserved heterogeneity parameter because a) the Gamma distribution is a popular choice in the existing literature, b) the log normal is more general than the Gamma parametric distribution in this context, and c) the finite mixture is nonparametric and can mimic many distributions (Weinke, et. Al. 2005; van den Berg, Lindeboom, and Ridder 1993).[40] In fact, in the case of a single risk hazard model the piecewise exponential baseline with a finite mixture heterogeneity distribution is the least parametric full information maximum likelihood estimation that can be implemented. Recall that adding a distribution for the heterogeneity term in these models forms a new class of models referred to as mixed proportional hazard models where the 'mixed' term refers to the mixture of the baseline distribution and a distribution for the unobserved heterogeneity. In the next two paragraphs results from the Gamma heterogeneity distribution will be presented though similar arguments can be made for the Log Normal heterogeneity distribution.

[40] Log normal distributions require numerical integration to evaluate the likelihood function. This is accomplished using a Gauss Hermite Quadrature with 24 integration points.

Results from the Gompertz baseline model in the presence of unobserved heterogeneity are presented in Table 5.3. The estimated heterogeneity parameter (the variance of the heterogeneity distribution) is significant in each model suggesting that unobserved heterogeneity exists in these data and should be accounted for in the competing risks framework. A graphical representation of the influence of unobserved heterogeneity on the resulting baseline hazard is presented in Figure 5.4. It appears that the model which ignores unobserved heterogeneity may produce a negatively biased baseline hazard estimate. Incorporating unobserved heterogeneity in the model seems to alleviate negative bias in the baseline hazard as evident in Figure 5.4. Figure 5.4 compares the baseline hazards from the model which ignores unobserved heterogeneity to a model with Gamma unobserved heterogeneity. The baseline hazard which ignores unobserved heterogeneity is less positively duration dependent than the Gamma heterogeneity model. However, a comparison of coefficient estimates between the parametric and nonparametric unobserved heterogeneity models suggests that the distribution of the unobserved heterogeneity does not significantly alter parameter estimates.

Table 5.3 Gompertz specification with heterogeneity, Conversion Risk

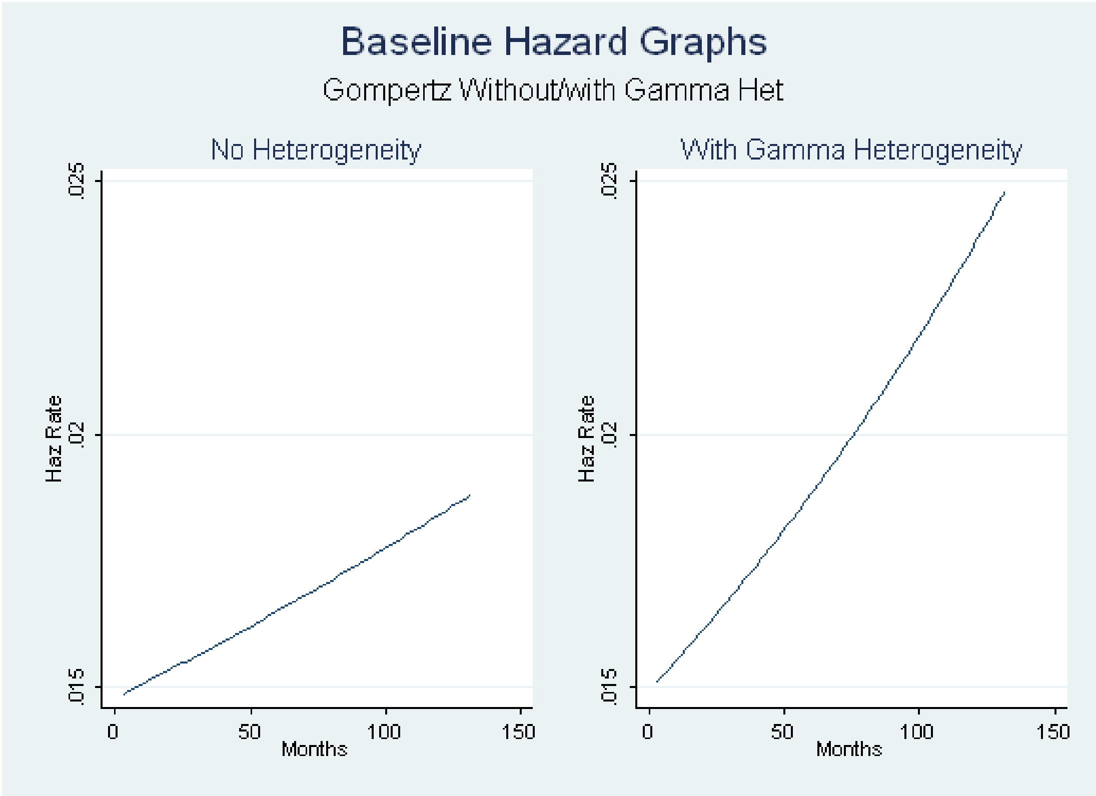
Baseline specification	Gompertz		Gompertz		Gompertz		Gompertz	
Heterogeneity specification			**Gamma**		**Log Normal**		**Finite Mixture**	
Variables								
Options variables								
Easement	-0.4855	**	-0.5436	**	-0.6051	**	-0.5015	*
Variance measure	-0.0252	*	-0.0282	**	-0.0358	**	-0.0368	**
Drift Measure	-0.0037		-0.0058		-0.0075		-0.0048	
Development returns								
distDC	-0.0103		-0.0097		-0.0116		-0.0117	*
distBA	0.0054		0.0067		0.0061		0.0063	
sluDevWithHs	-0.0222	**	-0.0243	**	-0.0244	**	-0.0234	**
sluDevNoHs	-0.0114	**	-0.0130	**	-0.0145	**	-0.0134	**
sluComm	-0.0113	**	-0.0147	**	-0.0159	**	-0.0189	**
sluSubdiv	0.0095		0.0104		0.0099		0.0101	
sluPreserved	-0.0201	**	-0.0222	**	-0.0224	**	-0.0210	**
sluOpen	0.0117		0.0156	**	0.0177	**	0.0206	**
sluRoad	0.0070	*	0.0088	*	0.0099	*	0.0085	*
sluProtected	-0.0002		0.0010		0.0014		0.0006	
sluExempt	-0.0232	**	-0.0244	**	-0.0248	**	-0.0247	**
devRate	0.0497	**	0.0484	**	0.0447	**	0.0397	**
popDen	0.1735		0.2350		0.2164		0.2264	
numLots	0.0003		0.0008		0.0008		0.0022	**
reqOpenSpace	-0.1377		-0.1665		-0.1789		-0.1728	
Opportunity costs								
class1	0.0107		0.0109		0.0117		0.0136	
class2	0.0030		0.0024		0.0024		0.0020	
class3	0.0010		0.0006		0.0006		0.0002	
class4	0.0007		-0.0003		-0.0007		-0.0014	
agriculture	-0.0032		-0.0032		-0.0032		-0.0023	
Acres	0.2685	**	0.3082	**	0.3242	**	0.2993	**
hasHouse	-0.9187	**	-0.9975	**	-1.0263	**	-0.9658	**
Conversion costs								
steep	-0.0060	**	-0.0070	**	-0.0071	**	-0.0069	**
forested	-0.0015		-0.0016		-0.0018		-0.0012	
notRoadSuit	-0.0050	**	-0.0056	**	-0.0057	**	-0.0057	**
notSepticSuit	-0.0003		-0.0001		-0.0003		-0.0001	
sewerPlanned	-0.7178	**	-0.7747	**	-0.8447	**	-0.8935	**
intRate	-0.1253	**	-0.1256	**	-0.1067	*	-0.1063	*
apfo	-0.2014		-0.2148		-0.1246		-0.1273	
Constant	-4.2134	**	-4.2021	**	-4.1589	**	-6.1146	**

Shape parameter	0.0018		0.0038	**	0.0019		0.0007	
Heterogeneity parameter			.0.4789	**	0.8384	**		
Fixed point 1							-0.6000	
point 2							2.4612	**
Weight 1							-0.8648	**

** - significant at 5%, * - significant at 10%

As discussed in Chapter 3, the presence of unobserved heterogeneity, when excluded from the analysis, often leads to spurious negative duration dependence because observations with ‘large’ draws from the unobserved heterogeneity distribution will tend to fail early in the study. As time moves forward, the surviving observations will be comprised of observations with ‘smaller’ draws from the unobserved heterogeneity distribution. In this case smaller draws imply a smaller hazard and thus these observations will tend to be in the sample longer. If unobserved heterogeneity is not modeled the baseline hazard is likely to exhibit negative duration dependence even if the true duration dependence is not negative for any observation in the sample.

Figure 5.4: Gompertz baseline hazard - Gamma Heterogeneity - Conversion Risk.



Similar to the Gompertz baseline hazard, the piecewise hazard model produces a negatively biased baseline hazard when unobserved heterogeneity is ignored. Table 5.4 presents the results from these models and Figure 5.5 displays the impact on the semi-parametric baseline hazard when Gamma unobserved heterogeneity is introduced. In the piecewise model the heterogeneity parameters are also significant, implying that even with some temporal heterogeneity removed by the semi-parametric baseline there remains significant individual level unobserved heterogeneity in the data. Again, the estimated coefficients are relatively insensitive to the choice of parametric unobserved heterogeneity distribution when compared to the nonparametric finite mixture distribution.

Table 5.4 Piece wise exponential models with Heterogeneity, Conversion Risk

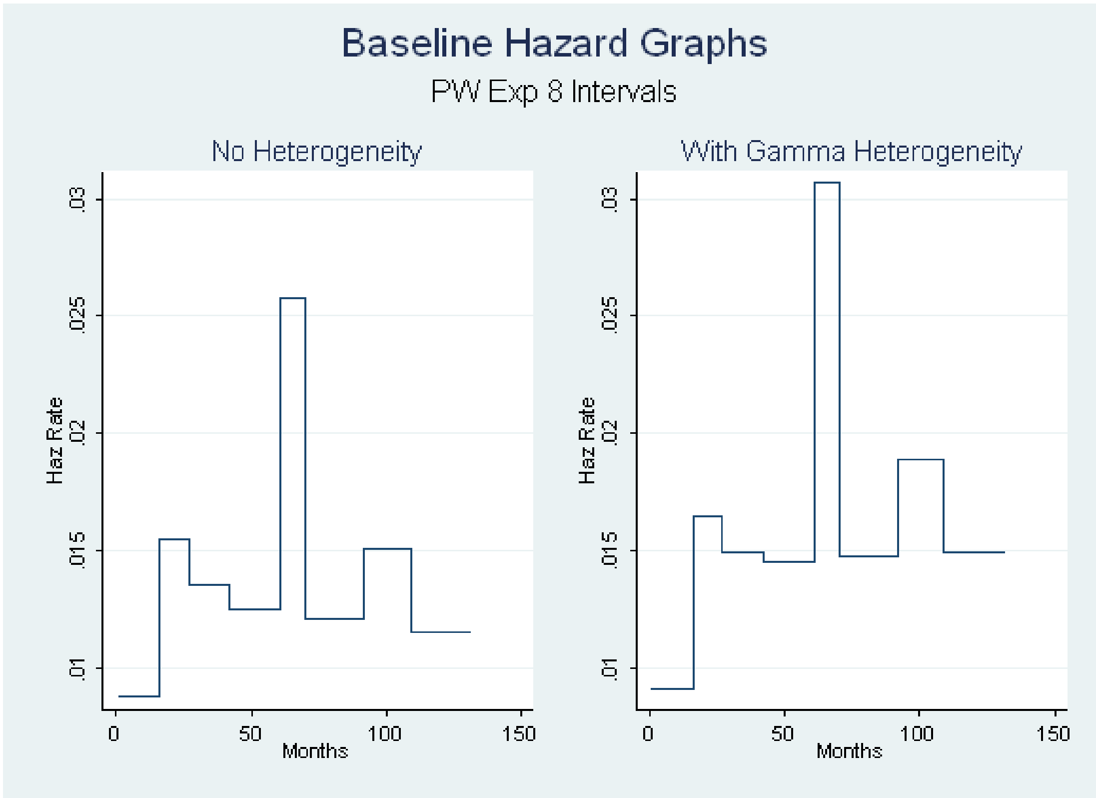
Baseline specification	PW Exp		PW Exp		PW Exp		PW Exp	
Variables			**Gamma**		**Log Normal**		**Finite Mix**	
Options variables								
Easement	-0.5289	**	-0.6050	**	-0.7247	**	-0.5645	**
Variance measure	-0.0265	*	-0.0298	**	-0.0358	**	-0.0358	**
Drift Measure	0.0093		0.0081		-0.0192		-0.0170	
Development Returns								
distDC	-0.0111	*	-0.0105		-0.0113		-0.0117	
distBA	0.0054		0.0066		0.0062		0.0058	
sluDevWithHs	-0.0218	**	-0.0239	**	-0.0262	**	-0.0234	**
sluDevNoHs	-0.0107	**	-0.0122	**	-0.0160	**	-0.0133	**
sluComm	-0.0113	**	-0.0148	**	-0.0184	**	-0.0189	**
sluSubdiv	0.0106		0.0118		0.0122		0.0107	
sluPreserved	-0.0201	**	-0.0221	**	-0.0246	**	-0.0210	**
sluOpen	0.0117		0.0160	**	0.0197	**	0.0209	**
sluRoad	0.0068	*	0.0088	*	0.0119	*	0.0087	*
sluProtected	-0.0002		0.0012		0.0019		0.0006	
sluExempt	-0.0235	**	-0.0246	**	-0.0268	**	-0.0246	**
devRate	0.0544	**	0.0535	**	0.0494	**	0.0423	**
popDen	0.1873		0.2534		0.2649		0.2437	
numLots	0.0003		0.0008		0.0014		0.0021	*
reqOpenSpace	-0.1344		-0.1610		-0.1746		-0.1610	
Opportunity costs								
class1	0.0113		0.0115		0.0113		0.0137	
class2	0.0032		0.0027		0.0022		0.0020	
class3	0.0011		0.0007		0.0004		0.0001	
class4	0.0010		0.0000		-0.0009		-0.0012	
agriculture	-0.0032		-0.0032		-0.0036		-0.0023	
acres	0.2737	**	0.3167	**	0.3608	**	0.3072	**
hasHouse	-0.9196	**	-1.0001	**	-1.1322	**	-0.9741	**
Conversion costs								
steep	-0.0061	**	-0.0071	**	-0.0081	**	-0.0070	**
forested	-0.0016		-0.0017		-0.0022		-0.0014	
notRoadSuit	-0.0050	**	-0.0056	**	-0.0063	**	-0.0057	**
notSepticSuit	-0.0005		-0.0003		-0.0006		-0.0002	
sewerPlanned	-0.7625	**	-0.8351	**	-0.9177	**	-0.9030	**
intRate	-0.0823		-0.0875		-0.1418		-0.1396	
apfo	-0.0459		-0.0523		-0.0180		-0.0170	
Constant	-4.7333	**	-4.7035	**	-4.2616	**	-5.9036	**
Heterogeneity parameters								

Heterogeneity parameter			0.4883	**	1.0961	**		
Fixed point 1							-0.6000	
point 2							2.4255	**
Weight 1							-0.8229	**

** - significant at 5%, * - significant at 10%

Based on these results unobserved heterogeneity appears to be present in these data, but the functional form of the heterogeneity distribution does not significantly impact the resulting coefficient estimates. Based on this fact the log normal distribution will be used in the dependent competing risks models because it is more tractable than either the Gamma or the finite mixture when correlating across risks where a bivariate distribution is required. The analysis, to this point, suggests that it is most appropriate to model the conversion hazard using a piecewise exponential baseline (8 intervals) and with unobserved heterogeneity in order to determine the impact of the preservation program. However, if a fully parametric model is required the Gompertz baseline hazard could also be utilized for the competing risks model without great reservation. Excluding one short period of intense activity, picked up by the piecewise exponential, the Gompertz baseline hazard and the piecewise baseline are quite similar. To this point, none of the models account for the second exit state available to landowners in this study area, the exit to preservation. This will be handled by the dependent competing risks model, but before estimating the competing risks model a similar set of distributional assumptions are required for the preservation hazard in the single risk context.

Figure 5.5: PW Exp – 8 intervals baseline hazard - Gamma Heterogeneity Conversion Risk.



5.2 Model Selection - Preservation Risk

Because many of tools necessary to select the functional forms for the preservation risk are identical to the conversion risk, and given the detail devoted to these in the previous section, this section will briefly describe the preservation hazard and justify the restrictions placed on this model. As in the conversion hazard the first step is to choose a baseline hazard specification. Fully parametric baseline hazard models are estimated and a likelihood ratio test is performed to compare a 'memoryless' baseline hazard versus a monotonic baseline. Unlike the conversion hazard case, the null hypothesis of a time invariant baseline hazard (i.e. the exponential distribution) is not rejected for these data.[41] This suggests an increasing or decreasing baseline hazard specification such as the Gompertz or Weibull is not necessary.

As in the conversion hazard, the nonparametric Cox model is used to validate that the functional specification of the baseline hazard is not dramatically influencing the estimated coefficients. Table 5.5 displays the results from the exponential and the Weibull model compared to the Cox model. The estimated coefficients are similar across these models and allow the selection of the exponential baseline for the preservation hazard to be made with some degree of confidence.

[41] The data fail to reject the null hypothesis using a likelihood ratio test, chi-squared (1) = 2.44 at 0.1183.

Table 5.5 Preservation Hazard – Exponential Baseline

Preservation Hazard variables	Exponential		Weibull		Cox		Exp-Log Normal Heterogeneity	
Funded	2.2576	**	2.4040	**	^		1.7175	*
Acres	0.0656	**	0.0667	**	0.0873	**	0.1258	**
distDC	0.0293		-0.0020		0.0019		0.0519	
distBA	-0.0049		0.0306		0.0357	*	-0.0089	
sluDevWithHs	-0.0061		-0.0077		-0.0048		-0.0130	
sluDevNoHs	0.0262	**	0.0279	**	0.0197	*	0.0197	
sluComm	0.0110		0.0122		0.0128		0.0006	
sluSubdiv	0.0198		0.0250		0.0126		-0.0078	
sluPreserved	0.0083		0.0064		0.0130		0.0134	
sluOpen	-1.5834	*	-1.5525		-1.8276		-2.5948	
sluRoad	-0.0893	**	-0.0854	**	-0.0925	**	-0.1312	*
sluProtected	-0.0288		-0.0275		-0.0231		-0.0431	
sluExempt	0.0024		0.0024		-0.0003		-0.0017	
class1	-0.0667	**	-0.0644	**	-0.0651	**	-0.0994	**
class2	-0.0986	**	-0.0946	**	-0.0919	**	-0.1372	**
class3	-0.0959	**	-0.0917	**	-0.0925	**	-0.1305	**
class4	-0.0607	**	-0.0584	**	-0.0628	**	-0.0931	**
steep	-0.0217		-0.0232		-0.0218		-0.0316	
forested	0.0289		0.0297		0.0254		0.0272	
agriculture	0.0457	*	0.0461	*	0.0401		0.0496	
hasHouse	-2.3673	**	-2.3368	**	-2.1688	**	-3.2487	**
Preservation Constant	-4.1289		-5.7950	*			-1.2830	
Heterogeneity parameter							1.4451	**
Number of obs =			257 (2009 observations)					
No. of failures =			59					

^ - does not vary over observations thus cancels out of Cox partial likelihood
** - significant at 5%, * - significant at 10%

Finally, unobserved heterogeneity is incorporated using the log normal distribution and the results are presented in the last column of Table 5.5. Once again the heterogeneity parameter is significant though not overly influential on the sign or significance of the parameter estimates.[42] To complete the assumptions necessary to estimate the competing risks model the exponential baseline with log normal heterogeneity is assumed an appropriate model. Now the full competing risks models can be specified and estimated.

5.3 Competing Risks Models and Results

In most empirical hazard applications it is common to assume other potential exit states are randomly censored, and thus observations that exit via pathways other than the transition under study do not impact the failure of interest to the researcher. That is, these additional exits are assumed independent of the failure event of interest. But in many, if not most, cases the competing events are likely to share common causes, so that their event times can rarely be assumed independent. Such correlation should be modeled to avoid bias.

Up to this point, non-random censoring has been assumed by each single risk model discussed in this chapter, a modeling strategy that is correct only if exits due to preservation are independent of the conversion decision and vice versa. In the analysis of the conversion (preservation) risk, a preservation (conversion) decision simply causes an observation to drop out of the risk set. Because the attributes that are likely to influence preservation are similar to the attributes that influence conversion this random censoring assumption is potentially an erroneous one. To address this problem the preservation decision is modeled jointly with the conversion decision in a true competing risks model.

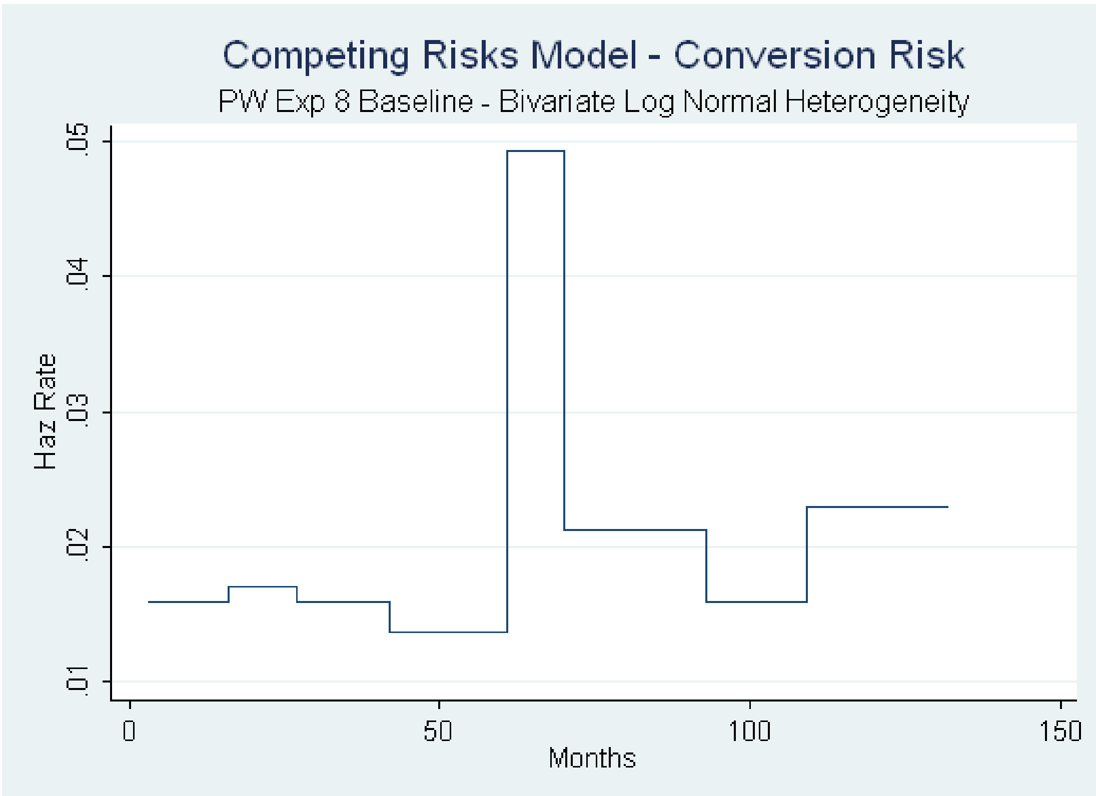
[42] The sample size prevents estimation of a finite mixture distribution for unobserved heterogeneity.

The choice of distributions required to estimate the empirical model is made based on the exploratory work of the previous sections. A piecewise exponential baseline hazard and a Gompertz baseline hazard are considered in turn for the conversion risk. An exponential baseline hazard is specified for the preservation risk. Both a log normal and a bivariate log normal are considered for the heterogeneity distribution. Again, the log normal is selected for tractability and because the single conversion risk results were similar between the Gamma, log normal, and the (nonparametric) finite mixture distributions of heterogeneity.

This log normal heterogeneity distribution is incorporated in two forms in the dependent competing risks model. First, the univariate heterogeneity distribution restricts the heterogeneity parameter to be the same in each risk. This representation is consistent with a world where unobservables describe the landowner's propensity to "move" or enter into some new land agreement (be it preservation or development). This may be the case where a financial situation requires the landowner either to sell the property or to sell an easement (i.e. enroll in the preservation program). Second, the bivariate case allows heterogeneity to be present in each risk in different degrees and correlates the dependency across risks via a jointly estimated correlation parameter. This discussion will focus on the results from the bivariate case because the univariate case does not differ dramatically.

With the distributional assumptions based on the single risk hazards in hand, the competing risks model developed in Chapter 3 is estimated and the results are reported (see Table 5.6). Before discussing the coefficient estimates it is useful to view the resulting baseline hazard estimates for the conversion hazard which this model produces.[43] Figure 5.6 displays the conversion hazard using the piecewise exponential baseline, where an exponential baseline for the preservation hazard and bivariate heterogeneity are implemented.

Figure 5.6: Competing Risks Piecewise Exponential Baseline Hazard, Conversion Risk.



This semi-parametric baseline function from the competing risks model is similar in shape to the estimates from the single case, given in Figure 5.5, but the magnitude is amplified. The baseline hazard from the conversion risk with heterogeneity exhibits a maximum of approximately .03, while in the competing risks model the maximum is closer to .045. Similarly, the minimum value of the baseline is shifted up, from less than 0.010 in the single risk case to almost 0.015 in the competing risk case. This difference in magnitudes is evident by comparing the difference in median conversion

[43] Recall the preservation hazard is constant thus an uninteresting graph of the baseline would result.

time by model. In the single risk case the median conversion time from the piecewise model with log normal heterogeneity is 33 years while the median conversion time is 24 years in the competing risks model.

Overall, the variables for unobserved heterogeneity within each risk are significant, similar to the single risk models. This suggests heterogeneity within risks exists and is indicative of the need to model these elements even though this application is in a data rich modeling environment and has used a flexible econometric specification. However, the correlation between these unobserved components is not significant. This could be a consequence of the choice of the heterogeneity distribution, which may be incapable of detecting this competing risk correlation, or it may suggest that cross risk correlations are adequately captured by the variation in the observable data. Unfortunately, these explanations cannot be validated. The competing risks model remains the most realistic model to mimic the real world decision process facing landowners in this study and will be utilized to discuss the coefficient estimates.

5.3.1 Competing Risks Results – Discussion of Coefficients

The empirical model developed in Chapter 3 and the data discussion in Chapter 4 includes several covariates intended to control for differences in development profitability and preservation likelihood across parcels at risk. The discussion of the estimated coefficients begins with these covariates, not because these explanatory variables are themselves of central importance, but because plausible results in these controls provide support for interpretations with regard to the options variables.

Recalling the discussion in Chapter 4, variables are included in the model if they are likely to have a bearing on the returns from development, the construction costs of development, and/or the opportunity costs of converting a parcel to development. Also included are covariates that might alter the appeal of a parcel as an easement sale by the county. Explanatory variables that affect both the returns to development, as well as the ranking the county is likely to place on the parcel should it be offered for easement sale, include measures of surrounding land uses. Relative to the normalized category – developed residential land – several categories of surrounding land use have a depressing influence on development. Specifically, commercial and institutional neighbors appear to have a depressing effect on development likelihood, as does neighboring land that is developable or has been preserved – all relative to residential development. Only open space has a (relatively) positive effect as a neighbor on development, as do existing roads (that offer road frontage valuable for development). As will be explored in the next few chapters, these results should not be accepted too literally, as there is an inherent sample selection bias lurking in this model. But for now, this is not an important consideration, as the description of surrounding land use is included as a control only at this point in the analysis.

Other controls related to parcel location include the recent rate of new home construction measured by *devRate* which is positive and significant. This variable varies by block group and serves as a proxy for unobservables which influence the rate of construction and, indirectly, the approval rate of subdivisions in the neighborhood of each parcel. The rate of subdivision and thus new construction activity may be due to unobservables in the county planning office where development approvals may be based in part on some unpublished county prerogatives. This measure also proxies for attractive locations of the county based on unobservables that developers may perceive, but are not visible to researchers. As with other control variables that may relate to the spatial distribution land conversion, direct interpretation of this variable as a policy relevant measure is not appropriate because this model is not designed to control for selection issues which likely exist in placement of new conversions. See Irwin and Bockstael (2002) for a detailed discussion.

Variables which represent the opportunity costs of development, including acreage and the presence of an existing house, are associated with significant coefficients. Larger parcels are more likely to convert at a rate of 3-4% more per acre. [44] The larger parcel is more likely to be a viable agricultural parcel but from the developer's perspective a larger parcel is potentially a more attractive investment because the marginal costs are likely decreasing in the number of units, at least in construction costs. Administrative costs are likely to be fixed at least over ranges of subdivision project size, contributing to economics of scale even when these additional bureaucratic costs are included. The presence of an existing structure significantly delays conversion and is either an opportunity cost, if the structure is viable in housing, or a construction cost, if the unit will need renovation or removal for the subdivision plan to proceed. Not surprisingly, the soil classification variables

[44] The variable *Acres* enters the model as observed acres divided by ten.

tell us little about the conversion decision, in part because soil quality impacts both agricultural returns and cost of development.

As for other measures related to construction costs, land poorly suited for road construction or septic systems is found to impact conversion timing negatively, which implies developers may be leaving this land in the sample longer while focusing on lands cheaper to convert. a higher value for the percentage of the parcel with very steep slopes tends to delay the conversion decision. Although potentially producing views, steep slopes reduce the yield of units per parcel and increase the costs related to septic system placement, erosion control, and landscaping.

As discussed in Chapter 4, variables relating directly to the preservation hazard include parcel attributes that define qualification for the easement program. These include acreage and land use of the parcel itself, as well as surrounding land uses because parcels can qualify or receive preferential treatment based on the land uses of neighbors. Many of the estimated coefficients are in line with expectations. For example, acreage has a positive and significant effect suggesting larger parcels are more likely to preserve. Among the surrounding land uses preservation and developable land without a house increase the hazard of preservation.

Other coefficients exhibit potentially counterintuitive results. For example, the percentages of land in good agricultural soil classes are significant and negative. However, parcels with very good agricultural soils may represent viable agricultural operations with no financial need to encumber the land with a preservation easement. The program specific variable for funding is significant and positive as expected. This variable controls for the fact that parcels only enroll in years when funding is available.

Finally, the presence of a house decreases the likelihood of preservation. This may seem counterintuitive unless one considers the family lot rules of the easement program. Based on the county's rules, the landowner is allowed one family lot per 50 acres of preserved land. Consider a landowner who owns 105 acres. He has the incentive first to split the land into a large 100 acre parcel and his 5 acre housing lot prior to enrollment. At that point, the landowner could enroll the 100 acre parcel in the preservation program and retain the option to build two family units instead of just one. Thus it is possible that the nature of family lot allowances on preserved land increases the probability the landowner will not preserve the *actual* land on which an existing structure sits, but will take some initial action first, and as a result preservations will tend to show up in the data set as largely parcels with no housing structures.

5.3.2 Competing Risks Results – Options Variables

The variables of particular importance to this investigation are those associated with options – the two real options variables (drift and variance) and the dummy variable denoting the option to sell an easement. As presented in the theoretical section, the options pertaining to the fluctuation in housing prices, the drift and variance, should delay conversion decisions because the landowner will, in many cases, expect the return from waiting to be larger than the return to immediate conversion. Compared to a net present value model where conversion takes place when the development return is greater than the conversion cost, the options framework requires that development return exceed cost by some margin equivalent to the value of the option to develop in the future. In essence, an opportunity cost of development today is the foregone returns of development tomorrow at a potentially more advantageous price. Thus in areas where the variance in price is large, a landowner who includes current volatility into a forecast of the next period's return may delay the conversion decision expecting an even better return in the next period. Similarly, for parcels qualified to enroll in a preservation easement, conversion today implies forfeiture of both the option to convert in the future and the option to preserve in the future. The expected result of a viable second option such as preservation is to delay conversion decisions.

The coefficient related to the variance of the real option is consistently negative and significant across model specifications. A one percent increase in the price variation implies a 3 percent reduction in the hazard rate of conversion and a one standard deviation change in variance implies a 15% reduction. These results accord with the comparative statics from the real options literature and suggest that price volatility increases the propensity of the landowner to delay conversion decisions. The options variable related to the drift is consistently positive though never significant.

The results pertaining to the second option are embodied in the estimated coefficient associated with easement eligibility (*Easement*). This coefficient is consistently negative and significant which suggests that preservation eligibility has an effect on the development decision and the effect is that of delaying development.

The magnitude of this effect is better seen by taking the exponential of the coefficient, thus converting the coefficient to a hazard ratio.[45] In this case the hazard ratios range from 50 percent in the piecewise model to 55 percent in the Gompertz model, implying that the rate of conversion for easement eligible parcels is approximately 45% to 50% less than what might be expected without an easement program.

The coefficient itself suggests the importance of the easement but the reduction in the hazard rate may not be extremely useful to policy makers as the connection between the hazard rate and actual time periods is not transparent. Table 5.7 reports the predicted median conversion times for censored parcels in the analysis, broken down by easement qualification and parcel size. The first three columns display the predicted outcome using the piecewise specification for the conversion baseline hazard which projects the value from the last interval of the piecewise model into the future. This is a potential drawback of the piecewise specification. The results presented in the last three columns of Table 5.7 pertain to the Gompertz specification.

[45] Strictly speaking, coefficients in unobserved heterogeneity models have the interpretation of hazard ratios only at t=0. As time progresses observations that are ‘ more frail’, as defined by the unobserved heterogeneity parameter, experience failure and are removed from the surviving population thus altering the distribution of unobserved heterogeneity among parcels remaining the sample. This complicates the direct interpretation of the coefficients.

Table 5.6 Predicted Median Conversion Times (by Parcel size)

	PW Exp Baseline		
Acres	Qualify for Easement	Not Qualified for Ease	Difference
>100 acres	14.97	7.54	**7.43**
75-100 acres	18.4	9.27	**9.13**
50-75 acres	28.95	14.57	**14.38**
25-50 acres	55.74	28.07	**27.67**
	Gompertz Baseline		
>100 acres	23.15	14.1	**9.05**
75-100 acres	26.14	16.09	**10.05**
50-75 acres	35.6	22.65	**12.95**
25-50 acres	55.11	37.24	**17.87**

Note: All values in years and all 'differences' are significant at 5%.

Comparing the median conversion time across models for large parcels (>100 acres), the piecewise baseline competing risks model predicts median conversion time for the non-qualified parcels to be approximately 7.5 years and for the parcels qualified for the easement to be approximately 15 years. The Gompertz specification produces a median conversion time of 14 nonqualified) versus 23 (qualified), for the same set of parcels. These large parcels are potentially the most interesting cases because they qualify for the easement without additional 'help' from adjacent parcels and because they yield the largest subdivisions and thus the greatest pressure on county services. However, differences in predicted conversion times persist over all size classes for both specifications and, range from 7.43 to 27.67 for the piecewise baseline model and 9.05 to 17.87 for the Gompertz baseline model.

Figures 5.7 and 5.8 display the histograms of predicted median conversion times by easement qualification where both graphs are scaled so the sum of the areas equals one. The graphs exhibit different shapes, due to the baseline specification, but the distributional difference between qualified and non-qualified parcels within each graph is apparent. The distribution of qualified parcels is shifted to the right compared to those not qualified. These results suggest that the existence of a preservation program, and thus the option to preserve, may actually slow development of eligible farmland even if that farmland eventually converts to residential use.

Figure 5.7: Predicted Median Conversion Times from Piecewise Bivariate Heterogeneity Competing Risks for the Conversion Risk.

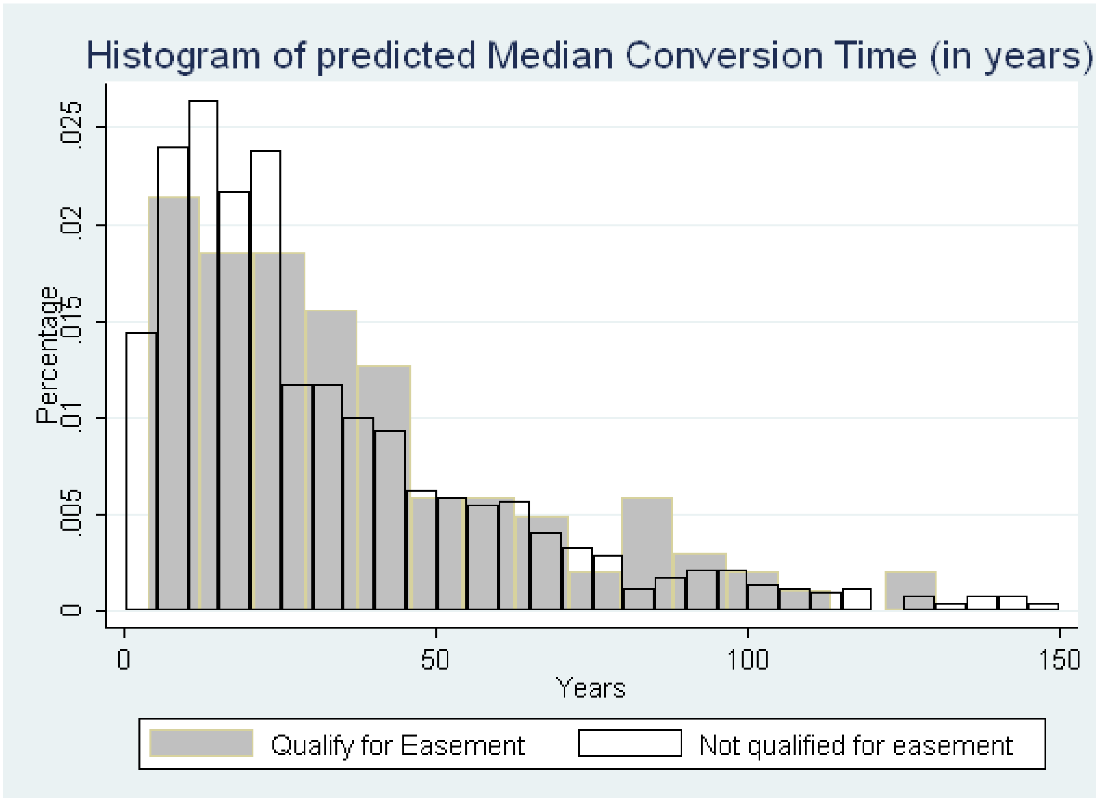
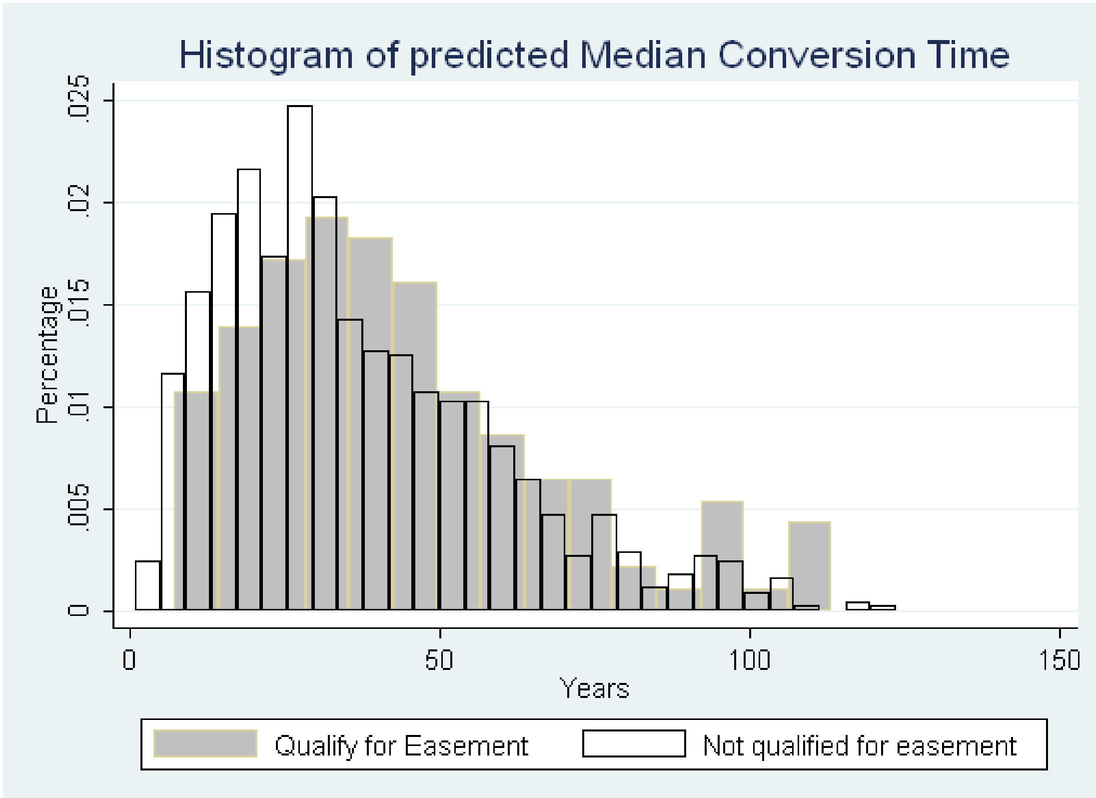


Figure 5.8: Predicted Median Conversion Times from Gompertz Bivariate Heterogeneity Competing Risks for the Conversion Risk.



Generally the results of the competing risks models are similar to the models which incorporate heterogeneity, as well as to the Cox model. Although the pattern of sign and significance is similar, it seems that as restrictions are removed in each step of the process, from fully parametric models to heterogeneity models to the competing risks models, the coefficient impacts are larger in absolute magnitude. As the results pertain to the primary variables of interest, i.e. the options variables, these more complicated (and more theoretically correct) estimation techniques lead to the same qualitative answers, which provide a comforting level of robustness in the conclusions drawn.

Table 5.7 Competing risks models

Baseline spec	PW Exp		PW Exp		Gompertz		Gompertz	
Variables (conversion risk)	Univariate Log Normal		Bivariate Log Normal		Univariate Log Normal		Bivariate Log Normal	
Options variables								
Easement	-0.6969	**	-0.6860	**	-0.6139	**	-0.5884	**
Variance measure	-0.0370	**	-0.0360	**	-0.0372	**	-0.0358	**
Drift Measure	-0.0187		-0.0184		-0.0086		-0.0069	
Development Returns								
distDC	-0.0112		-0.0114		-0.0115		-0.0116	
distBA	0.0059		0.0057		0.0060		0.0056	
sluDevWithHs	-0.0259	**	-0.0252	**	-0.0252	**	-0.0241	**
sluDevNoHs	-0.0155	**	-0.0149	**	-0.0152	**	-0.0142	**
sluComm	-0.0178	**	-0.0170	**	-0.0167	**	-0.0155	**
sluSubdiv	0.0117		0.0114		0.0101		0.0099	
sluPreserved	-0.0220	**	-0.0222	**	-0.0216	**	-0.0210	**
sluOpen	0.0192	**	0.0185	**	0.0184	**	0.0172	**
sluRoad	0.0108	*	0.0105	*	0.0100	*	0.0093	*
sluProtected	0.0018		0.0016		0.0015		0.0013	
sluExempt	-0.0263	**	-0.0257	**	-0.0256	**	-0.0248	**
devRate	0.0493	**	0.0486	**	0.0450	**	0.0445	**
popDen	0.2651		0.2514		0.2305		0.2110	
numLots	0.0009		0.0008		0.0007		0.0006	
reqOpenSpace	-0.1506		-0.1587		-0.1638		-0.1625	
Opportunity costs								
class1	0.0128		0.0124		0.0126		0.0124	
class2	0.0023		0.0024		0.0023		0.0025	
class3	0.0004		0.0005		0.0005		0.0006	
class4	-0.0010		-0.0008		-0.0010		-0.0007	
agriculture	-0.0033		-0.0033		-0.0031		-0.0031	
acres	0.3560	**	0.3454	**	0.3361	**	0.3194	**
hasHouse	-1.1357	**	-1.0890	**	-1.0816	**	-1.0275	**
Conversion costs								
steep	-0.0080	**	-0.0076	**	-0.0075	**	-0.0070	**
forested	-0.0021		-0.0020		-0.0019		-0.0018	
notRoadSuit	-0.0062	**	-0.0060	**	-0.0059	**	-0.0057	**
notSepticSuit	-0.0005		-0.0004		-0.0003		-0.0003	
sewerPlanned	-0.9012	**	-0.8796	**	-0.8570	**	-0.8299	**
intRate	-0.1403		-0.1384		-0.1071	*	-0.1059	*
Apfo	-0.0109		-0.0098		-0.1302		-0.1224	
Constant	-4.2218	**	-4.1436	**	-4.2109	**	-4.1207	**

Table 5.7 Competing Risks Models, continued.

Baseline spec	PW Exp		PW Exp		Gompertz		Gompertz	
Variables (preservation risk)	Univariate Log Normal		Bivariate Log Normal		Univariate Log Normal		Bivariate Log Normal	
Funded	1.9142	**	1.7686	*	1.7994	*	1.9753	**
Acres	0.1073	**	0.1229	**	0.1231	**	0.1011	**
distDC	0.0409		0.0494		0.0497		0.0388	
distBA	-0.0096		-0.0096		-0.0095		-0.0091	
sluDevWithHs	-0.0140		-0.0145		-0.0149		-0.0128	
sluDevNoHs	0.0166		0.0171		0.0169		0.0181	
sluComm	0.0030		0.0012		0.0015		0.0047	
sluSubdiv	0.0036		-0.0040		-0.0031		0.0061	
sluPreserved	0.0098		0.0125		0.0124		0.0094	
sluOpen	-2.9817	*	-2.9971		-3.0150		-2.7270	*
sluRoad	-0.1323	**	-0.1420	**	-0.1453	**	-0.1256	**
sluProtected	-0.0369		-0.0416		-0.0412		-0.0358	
sluExempt	-0.0087		-0.0060		-0.0068		-0.0070	
class1	-0.0898	**	-0.1002	**	-0.1010	**	-0.0857	**
class2	-0.1286	**	-0.1395	**	-0.1405	**	-0.1235	**
class3	-0.1209	**	-0.1322	**	-0.1332	**	-0.1167	**
class4	-0.0857	**	-0.0947	**	-0.0957	**	-0.0814	**
Steep	-0.0323	*	-0.0336		-0.0340		-0.0302	*
Forested	0.0203		0.0225		0.0219		0.0221	
agriculture	0.0417		0.0455		0.0449		0.0427	
hasHouse	-2.9557	**	-3.2433	**	-3.2554	**	-2.8499	**
Preservation Constant	-0.5404		-0.3136		-0.1851		-1.1328	
Heterogeneity Parameters								
Heterogeneity parameter 1	1.0961	**	1.4016	**	1.3994	**	0.9603	**
Heterogeneity parameter 2			0.9865	**	0.7879	**		
Correlation			0.5318		0.6638			

* - significant at 10%, ** - significant at 5%

5.4 Conclusion

Modeling land conversion decisions is fraught with difficulties ranging from intensive data requirements to the appropriate choice of model structure. In many cases models to describe conversion decisions utilize aggregate data across time or space and place inappropriate restrictions on econometric specification.

The analysis in this dissertation chapter extends the current literature in at least two directions. First, it utilizes a spatially and temporally explicit micro-level data set to demonstrate how to solve several of the modeling/econometric problems that have plagued analysis of these types of behavior for some time. To this author's knowledge this is the first paper to utilize a multi-state model in a land use context and the first to consider the impacts of unobserved heterogeneity in the conversion timing decision.

Models which remove parametric restrictions and incorporate individual heterogeneity are proposed and estimated. These complex models closely resemble the choice set a landowner is faced with in the county under study and may generalize to counties with similar programs. Estimation of the dependent competing risks model produces coefficient estimates similar to less complicated models with nonparametric baseline specifications or multiplicative individual unobserved heterogeneity, but the framework for the competing risks model can serve as a building block to forecast conversion decisions. At a minimum, the dependent competing risks model validates heretofore assumed restrictions on the single risk models such as non-random censoring and the absence of length-biased sampling caused by individual unobserved heterogeneity.

Second, it incorporates real options theory into the development decision modeling framework and finds that at least some of the theoretical predictions hold up in empirical work. Although there is no consensus in the literature about the appropriate form of a land conversion model, this work supports an argument that rejects the net present value approach as an adequate representation of land conversion. As predicted by real options theory, price volatility is found to slow conversion rates in the competing risks models as well as single risk semi–parametric baseline models with and without heterogeneity. In addition, the existence of an alternative real option (preservation) is found also to slow conversion.

The primary goal of this work was to determine if an easement option impacts conversion decisions, and, if so, to quantify the temporal impact of the option on landowners that do not preserve. With each model, from single risk to competing risk, negative and significant impacts from the easement option are found. The magnitude of the impact on the conversion rate ranges from a 45% to a 50% reduction in the conversion hazard. The less parametric and more realistic models produce the largest estimated impacts.

This analysis finds empirical support for the predictions of the theoretical real options literature in terms of the effect of high price volatility and that of multiple options. However, one should be somewhat cautious in making interpretative statements about some of the control variables in these models because, as in many land use analyses, there is much correlation among many of these covariates. For example, the distance measures are likely correlated with the surrounding land use measures as well as zoning variables such as number of lots. Additionally, as outlined by Irwin (1998) there is ample opportunity for endogeneity in the spatial landscape. It is for these reasons that the next chapters of this dissertation will utilize a technique specifically designed to account for endogeneity to examine a second question: whether preservation has an influence on the development decisions of neighboring parcels.

6. Measuring the Preservation "Spillover Effect"

Many of the control variables utilized in the hazard models in Chapter 5 are generated as a result of policy decisions made by the county in the past, and one may be tempted to interpret their respective coefficients as estimated policy impacts. However one should be cautious because, as in most land use datasets, there is likely correlation among covariates and the potential for selection problems. This chapter will examine a policy relevant variable from the hazard model that deals with the development response to neighboring preservation activity. On the surface, one would expect the coefficient on the surrounding preserved land measure to provide clues concerning the development response to neighboring preservation. But it cannot ensure causation and thus does not address the important policy question: "does preservation attract development?" In the subsequent sections will explain the potential bias in this interpretation, suggest and implement an alternative estimation strategy which deals with the potential biases, and discuss implications and results. For convenience, the impact of preservation activity on neighboring parcels is referred to as the *spillover effect* in the remainder of this analysis.

Chapter 4 introduced the notion of interaction effects in the description of the surrounding land use variables. The spillover effect is one such interaction effect. Interaction effects arise because surrounding land use has an effect on the value of a parcel in a given land use, and thus surrounding land use will also have an effect on the likelihood of a parcel being developed. In the presence of these interaction effects, policies such as the preservation policy that alter land use decisions can alter the likelihood of development of parcels other than those directly affected by the policy. Based on this reasoning surrounding land use measures are included in the hazard models, and these potential interaction effects seem to be confirmed by the significance of the coefficients from those models.   But these coefficients should not be interpreted in the context of a policy evaluation where the interaction effect is the pathway which the policy is expected to influence the outcome. The pattern of surrounding land uses may arise from interaction effects or from other spatially correlated and unobserved exogenous landscape features. It is worth noting that these variables are included in the hazard model as predetermined variables because they are lagged measures, but the interpretation of these variables as purely interaction effects should be made with caution. This issue is especially relevant when the surrounding land use is a direct result of a policy, as is the case with the land use in preservation. If it is true that preservation deters development, as suggested by the negative coefficients on *sluPreserved* in the hazard model, then this preservation program appears to have little downside. But if the opposite is true then the program may have the unintended consequence of attracting development and encouraging further fragmentation of the landscape.

There are at least two sources of bias present in the hazard model relating to the coefficient on neighboring preservation (*sluPreserved*) as this coefficient relates to a direct policy interpretation.   The first potential form of bias is best described as a selection issue. Empirically speaking this may be best explained using terminology associated with a controlled experiment. In a controlled experiment subjects are assigned to the treatment and the control groups randomly. Thus the impact of the treatment can be estimated by comparing the difference in a measured outcome variable between the two groups. In this case the treatment is neighboring a preserved parcel and the outcome of interest is the subsequent observed conversion activity. When we attempt to deduce treatment impacts using actual outcomes, however, it is easy to see that there exists an assignment problem. Parcels are obviously not randomly assigned to treatment and control groups and thus in the treatment context the act of neighboring a preserved parcel is not an exogenous attribute of a parcel. In fact, parcels most likely to be treated have observable attributes that distinguish them from those parcels that are not likely to receive the treatment. These same attributes are included as covariates in the hazard model of conversion and are shown to be significant in the analysis of the conversion decision. So, the hazard models fail to account for this selection issue in estimation of the coefficient on surrounding land preservation. To evaluate a policy specific outcome, such as the spillover effect, it is best to compare "apples to apples". That is, one needs to compare only similar parcels from the treatment and control groups to one another and thus account for this selection on observables.

A second form of bias exists in the hazard models because the surrounding land use measure (*sluPreserved*) includes all preservation activity dating back to 1980. Therefore, to adequately measure the impact of surrounding preservation activity one would need to model conversion decisions starting in 1980. The fact that we cannot do this because of data limitations leads to a starting point bias. As an example, consider a parcel that is preserved in 1983 and has two developable neighbors, one of which is more suitable for development and does develop in 1988.

The preservation status of the first parcel will be present in the data set, but because the study period begins in 1990, the development activity will be missed. Because of the inherent sampling problem associated with the starting point bias, the more developable the neighboring parcel of this preservation, the more likely it will already have been developed by the onset of the study period. The data set used in this dissertation will only report that the sole developable neighbor of the preserved parcel does not develop and will miss the 1988 conversion. As a result the sample will contain a disproportionate number of parcels which neighbor preservation and have attributes that tend to decrease the chances they will convert, perhaps ever, creating an additional selection issue. By itself, this feature of the data may lead to the negative coefficients observed in the hazard models for *sluPreserved*.

In order to evaluate the spillover effect of preservation activity the non-random assignment problem is addressed using a class of estimators called propensity score estimators and starting point bias is addressed by reformulating the data to evaluate only preservation and conversion activity during the study period. But first the source and direction of the spillover effect are discussed in detail.

6.1 Spillover Effects

The aim of this chapter is to test whether a spillover effect exists between preserved parcels and neighboring development decisions. Specifically: does having a preserved neighbor alter a parcel's likelihood of subsequently being developed? There are several reasons why a spillover effect may exist between developable parcels and preserved neighbors, none of which can be ruled out *a priori*, and none of which can be individually identified. On the one hand, a farmer with a neighbor who has preserved his land may find it more profitable to remain in farming for all the reasons that ‘right to farm’ laws have been instituted. Nuisance complaints from equipment noise, manure odors, and chemical releases are less likely to occur, and support industries are more likely to survive in the area. On the other hand, agricultural enterprises that depend on marketing to the public may find it profitable to have residential neighbors. Landscape horticultural enterprises and pick-your-own farm operations could possibly benefit from developments close by.

The effect of a preserved neighbor on the profitability of development is equally uncertain. The proximity of services and shopping is often considered an advantageous feature for residential development and a greater mass of residential development is likely to encourage commercial services in the area. Thus the more preserved land in the area the less the critical mass of households and the lower the levels of services, *ceteris paribus*. On the other hand, neighboring land that is permanently preserved in open space could provide positive spillovers in terms of rural amenities (e.g. scenic views) and low levels of congestion and traffic noise. As the likelihood of development depends in part on a) the willingness to pay of households for housing in a given location and b) the profitability of the existing undeveloped uses (e.g. farming), the relative sizes of the potential spillover effects described above may be important in determining where development takes place.

Testing a hypothesis about these components of the spillover effect is surprisingly difficult. Regression-type analyses of land use interactions suffer from the sorts of identification problems that Manski (1993, 1995) has found so prevalent in socio-economic behavior. Irwin (1998) and Irwin and Bockstael (2002) illustrate the problems that arise in trying to identify spillover effects between neighboring land uses. They suggest that because many of the factors that make development more or less profitable are spatially correlated, the empirical finding of more development adjacent to existing development is not evidence of a positive interaction effect. Such an outcome could easily arise simply because both parcels are characterized by similar levels of the factors that affect development profitability, such as commuting distance to employment centers, road frontage, suitability of soils for development, etc. In testing whether preservation affects neighboring parcels' likelihood of development, these sorts of identification problems will be encountered as well.

In summary, the spillover effects of preservation on development are likely to be both positive and negative. For example, spatial correlation in factors affecting development suggests a potential negative relationship between development decisions and neighboring preservation. However, positive externalities generated by open space in the presence of nearby residential uses suggest a potential positive impact of neighboring preservation on development decisions. The estimator proposed in the next section attempts to measure the sum total of these spillover effects and only if strong positive spillover effects exist between preservation and development would one expect to find a positive empirical effect of preservation on development decisions.

6.2 Propensity Score Matching

The next few subsections will outline the implementation of a propensity score estimator and highlight the advantages and drawbacks of the approach. In this framework, this analysis wishes to test for, and measure, the treatment effect where the observation of interest is a developable parcel, the *treatment* is the preservation of a neighbor, and the *outcome* of interest is whether the developable parcel is developed or not over a specified time period following the preservation action. This is a non-random selection process because, as has been argued, developable parcels that have preserved neighbors are likely to have, on average, different characteristics than parcels without such neighbors, and these different characteristics may alter the likelihood of development.

Conventional analyses, such as the hazard model reported in the previous chapters, might attempt to control for these characteristics by entering them, together with the treatment variable, into a model that seeks to explain the outcome. But criticisms of this type of approach are now standard, and include concern over reliance on linear or simple functional forms and over failure of the common support (cases where treated observations are substantially different from untreated observations). Alternatives for improving the rigor of the statistical test include procedures that estimate treatment effects by matching treated and untreated observations on conditioning variables and excluding observations that are measurably different from any treated observation, i.e. not on the common support.

As an example of how the common support issue might arise in this data, consider the case where a parcel has attributes that make it highly valued as a subdivision and then has a neighbor that enters the preservation program. If a parcel with similar attributes but no bordering preservation does not exist in the data set, then the counterfactual does not exist and non-parametric identification is not possible. This is referred to as failure of the common support. Evaluation of the treatment effect in this case is only possible in the regression context because the functional form of the regression equation will estimate a counterfactual in these regions of sparse data.

In essence the regression function imposes a parametric relationship between the covariates in the model and the outcome of interest in order to construct this counterfactual. Similar to the work of Black and Smith (2004) there is no theoretical argument for the functional form of the outcome equations in this land conversion context, and thus it is inappropriate to rely on a linear or any other specific functional form to predict this counterfactual. The propensity score approach allows the researcher to identify these anomalies and use only parcels with counterparts in the control group to estimate the treatment effect. To reiterate, propensity score methods do not solve this issue, they allow it to be addressed by excluding those observations which fail the common support.

6.2.1 Propensity Score Matching – The Basics

Propensity score-matching estimators were first suggested by Rosenbaum and Rubin (1983). Applications of propensity score-matching are now quite prevalent in the literature, especially in labor economics where the evaluation of job training programs represents a significant challenge (such as Heckman, Ichimura, and Todd 1997; Dehejia and Wahba 2002; Lechner 2002; Smith and Todd 2005a). Following common notation, Y_1 is the outcome under treatment and Y_0 is the outcome with no treatment. For any parcel, only one of these outcomes can be observed. $D = 1$ indicates that the parcel is in the set of parcels that has been treated, and $D=0$ indicates it is in the untreated set. Rosenbaum and Rubin (1983) identify a measurable quantity of interest, defined by the following equation:

(23) $$ATT = E(Y_1 - Y_0 \mid D = 1) = E(Y_1 \mid D = 1) - E(Y_0 \mid D = 1)$$

where ATT is 'the average treatment on the treated'. This equals the expected value of the difference between the treated outcome and the non-treated outcome, for the particular group of parcels that happened to get treated. For this analysis this is the effect on the likelihood of development of having a newly preserved neighbor, averaged over all parcels that were treated. The first term on the right hand side of (23) is easily obtained; it is the percentage of treated parcels that develop. The second term on the right hand side represents the counterfactual – the outcome a treated parcel would have received had it not been treated. Since a parcel can be in only one state, treated or control, the matching procedure boils down to an estimate of $E(Y_0|D=1)$, which is unobservable.

Matching estimators pair each treated observation with 1 or more observationally similar non-treated observations, using the conditioning variables, Z, to identify the similarity. This procedure is justified if it can be argued that conditional on these Z's, outcomes are independent of the selection process. That is, if those parcels found in the set D=1 were actually not treated, the expected value of their outcomes, once conditioned on the Z's, would not differ from the expected value of outcomes in the current group of untreated observations. More precisely, conditional mean independence is required, such that:

$$E(Y_0 \mid Z, D=1) = E(Y_0 \mid Z, D=0) \tag{24}$$

Rosenbaum and Rubin's (1983) method for resolving the non-random assignment problem is based on this idea. They argue that if treatment is determined by some set of covariates, Z, one can establish a control group that is similar in Z relative to the treatment group. They formally state this as:

$$E(Y_1 - Y_0 \mid Z, D=1) = E(Y_1 \mid Z, D=1) - E(Y_0 \mid Z, D=0). \tag{25}$$

Direct implementation of equation (25) would be difficult for a large number of conditioning variables.[46] Yet ensuring that equation (24) holds requires a rich set of these variables which should include all variables that influence the probability of treatment and outcome of interest. This led to the seminal contribution of Rosenbaum and Rubin's 1983 paper. They proved that instead of conditioning on all K elements of the Z vector individually, one can equivalently condition on a one-dimensional function of that vector. They show that if outcome Y_0 is independent of selection when conditioned on the Z's, then it is also independent of selection when conditioned on the propensity score which is defined as the probability of selection conditioned on the Z's or more formally:

$$P(Z) = \Pr(D = 1 \mid Z). \tag{26}$$

[46] This is often called the "curse of dimensionality".

Rosenbaum and Rubin also require that there be no single Z or combination of Z variables that *guarantees* treatment. Put another way, for any set of Z, the probability of treatment must be strictly less than 1, i.e. $Pr(D = 1 | Z) < 1$ for all Z. This condition must be true for each treated observation to have the potential of an analogue among the untreated. Thus, the impact of being treated is only valid for observations within the common support where the distribution of propensity scores for treated and control observations overlap.

Equation (23) can now be rewritten as,

$$(27) \quad ATT = E(Y_1 - Y_0 \mid P(Z), D = 1) = E(Y_1 \mid P(Z), D = 1) - E(Y_0 \mid P(Z), D = 0).$$

In practice, equation (26) is estimated as a binary probit or logit, with the treatment dummy as the dependent variable. Explanatory variables include factors that are expected to affect the probability of treatment and those that are expected to affect outcomes directly and may be correlated with treatment. This works well in the land conversion context because, as discussed in the previous chapters, the variables that are expected to influence preservation activity are often the same variables expected to impact the conversion decision.

With these propensity scores in hand, several ways exist to construct the counterfactual or the last term in equation (27), including kernel estimates, k-nearest neighbor, and caliper based techniques. Based on results from a Monte Carlo study by Frölich, kernel estimates will be employed. Kernel estimates use a weighted average of all or a subset of control observations to construct the counterfactual for each treated observation. Each treated observation, i, is paired or matched with some group of comparable *j* non-treated observations using their respective *P(Z)*. In order to match observations, a weight, $W(i,j)$, is constructed from the kernel function, $K(\cdot)$. The kernel function used is the Epanechnikov kernel because it combines desirable properties from the tricube (i.e. dropping influence from the distribution's tails) and the normal kernels (i.e. weighting close observations, in *P(Z),* more heavily and smoothly diminishing the weight with distance) (Smith and Todd 2005a).[47] This allows the matching of the outcome of the set of treated parcels to the 'kernel weighted' outcome of an appropriate set of the $D = 0$ control group. This will construct the counterfactual and estimate the average treatment effect of the treated, $A\widehat{T}T$.

Heckman, Ichimura, and Todd (1997) and Smith and Todd (2005a) provide the following formal exposition:

$$A\widehat{T}T = \sum_{i \in \{D=1\}} \frac{1}{N_1} \left[y_{1i} - \sum_{j \in \{D=0\}} W(i,j) y_{0j} \right] \tag{28}$$

where,

$$W(i,j) = \frac{G_{ij}}{\sum_{k \in \{D=0\}} G_{ik}}, \tag{29}$$

and $G_{ij} = K(z_{ij}, h)$, $z_{ij} = P(Z_j) - P(Z_i)$, $h = bandwidth$, and

$$\sum_{j \in \{D=0\}} W(i,j) = 1.$$

N_1 is the number of treated observations and *h* is the bandwidth of the kernel. The kernel choice, as suggested by DiNardo and Tobias (2001), has less impact on the

[47] The Epanechnikov kernel is given by $K(z) = 1 - (z/h)^2$ *if* $|z| <= h$.

estimated weight, $W(i,j)$, than does the choice of bandwidth (h). More bias and less variance are associated with higher values of h, and less bias and more variance are associated with lower values of h. Again following (Frölich 2004), the optimal bandwidth is found through the "leave-one-out" method of cross validation. To perform "leave-one-out' cross validation, one observation at a time is left out of the analysis. With a specific bandwidth and kernel the value of the dropped observation is predicted. This is done for each observation in the sample, in turn, and the prediction errors are collected. Formally, if y_{0j} is the outcome of the j^{th} observation then the prediction error is $y_{0j} - \hat{y}_{0j}$ where $\hat{y}_{0j}$ is the kernel based estimate of the outcome. Finally, the mean squared error is calculated as $\frac{1}{N_0}\sum_{j\in\{D=0\}}\left(y_{0j} - \hat{y}_{0j}\right)^2$ for each bandwidth on the grid search, where N_0 is the number of observations in the control group.[48] The "optimal" bandwidth is the one that produces the minimum mean squared error.

The strength of propensity score matching is that it exposes regions in which the support of **Z** does not overlap for treated and untreated observations. For example, there may be no untreated observations with propensity scores in the range of high values of $P(Z_i)$. When this is the case, the matching procedure is defensible only over the region of the common support. Treated observations outside the common support are dropped from the analysis, and the parameter $\widehat{ATT}$ is an estimate of the treatment effect on the treated only over the range of the common support.

[48] For kernel matching the bandwidth grid is 0.01 x 1.2^{g-1} for g = 1,...,29.

7. Application of Propensity Score Matching

An important policy impact of any land preservation program is the impact on surrounding parcels, what has been described as the spillover effect in this paper. Policies that alter land uses produce spillover effects if they positively or negatively impact the probability of conversion of neighboring parcels. In this context the Howard County farmland preservation program, which enrolls parcels in perpetuity, may have the further effect of discouraging development in areas neighboring preservation or, conversely, the unintended consequence of attracting development into the very areas the county wishes to reduce development pressures. Econometrically, the spillover effect is difficult to measure with regression techniques because treated parcels, those that neighbor a new preservation, are not randomly assigned and thus the treatment is not exogenous but is correlated with other measurable parcel attributes. Propensity score estimators address this assignment, or selection, problem by predicting the probability of treatment, conditional on observables, then matching parcels across treatment and control groups by their propensity scores to form an estimate of the treatment effect.

Using experimental and non-experimental comparison data, Heckman, Ichimura and Todd (1997) and Heckman, Ichimura, Smith and Todd (1998) concluded that criteria for matching estimators to have a low bias include:

- The conditioning variables should represent a rich set of factors related to both selection and outcome;

- The treated and untreated observations should be drawn from the same underlying population;
- The dependent outcome variable should be measured in the same way over treatment and control groups.

These conditions arise from the fact that the majority of matching tests have been applied to labor market data where treated and untreated samples are often drawn from completely different data sources, neither of which have very rich sets of individual characteristics. This land use problem poses no obvious biases from these sources. Treated and untreated observations are all members of the set of developable parcels in Howard County. Outcomes are measured for both groups using the same data acquisition and processing procedures, and extensive data (at least relative to many labor market studies) are available for each parcel for use as conditioning variables. Arguments based on economic theory and institutional knowledge inform the choice of conditioning variables in the land use problem.

It should be noted that the propensity score approach is not a silver bullet and comes with its own drawbacks for this land use problem. Posing the hypothesis about spillover effects in a treatment effects setting is not straightforward because of the dynamic nature of the development and preservation processes and the static nature of the propensity score matching tests. The propensity score approach, by design, estimates the probability of treatment based on the state of the landscape prior to treatment. The outcome, the observed conversion decision, must then be measured over a subsequent period of years. The strength of the hazard model in incorporating the dynamic nature of the development environment is missing in the propensity score approach. Due to the static nature of the propensity score estimator all time-varying variables as of the beginning of the study period and to exclude the options variables for variance and drift are excluded from the analysis.

Additionally, the data set is limited to only those parcels in the western part of the county because the probability of neighboring a preserved parcel is virtually zero for the parcels not in the west. All parcels in this subset of the data are located outside the sewer boundary so the variable for sewer planned is dropped from the analysis. Also eliminated is the incidence of adequate public facilities ordinances because this variable changes from year to year. Covariates with this much variation over time cannot be accommodated in the static propensity score framework. An additional variable is added to the analysis - a dummy variable to indicate if the parcel has a neighbor that qualifies for an easement (*sEasement*). The surrounding land use radii are expanded to 400 meters in order to detect development pressure for multiple years of conversion decisions. In the hazard model the 100 meter buffer was used because it was updateable each year and the conversion decision was analyzed each year not aggregated over many years as is necessary in this model. A complete list of data and summary statistics for the utilized in the propensity score analysis are given in Table 7.1.

Table 7.1 Summary Statistics –Propensity Score Analysis

Variable	Obs.	Mean	Min	Max
Easement	605	0.3471	0	1
sEasement	605	0.3851	0	1
distDC	605	50.8241	29.5114	69
distBA	605	39.1997	23.6942	73

sluDevWithHs	605	29.2921	0.3885	80
sluDevNoHs	605	23.2532	0	88
sluComm	605	1.3988	0	33
sluSubdiv	605	7.1199	0	39
sluPreserved	605	5.7642	0	67
sluOther	605	2.6145	0	60
sluOpen	605	0.3343	0	35
sluRoad	605	4.7551	0	41
sluProtected	605	5.0918	0	68
sluExempt	605	1.0852	0	49
numLots	605	11.0661	3	95
reqOpenSpace	605	0.7058	0	1
class1	605	3.3490	0	67
class2	605	53.8708	0	100
class3	605	25.0245	0	85
class4	605	13.5739	0	86
agriculture	605	56.7937	0	100
acres	605	49.469	4.294	430
hasHouse	605	0.4893	0	1
steep	605	9.7439	0	99
forested	605	32.8115	0	100
notRoadSuit	605	36.1646	0	100
notSepSuit	605	43.4757	0	100

Two experimental evaluations are implemented. In each the treatment is defined as the preservation of a neighbor during the years 1990-1992.[49] This particular treatment definition was selected in part because a large group of parcels were preserved in 1990-1992. The two alternative outcomes are a) whether the parcel was developed during a short run period (from 1992 through 1997) and b) whether the parcel was developed during in a longer term period (from 1992 through 2001), both following a neighboring preservation action. The observation set includes any parcel

[49] Data on all preservation decisions back to the program's inception in the early 1980's are available, allowing inclusion of 1990 as well as 1991 preservation decisions in this treatment. Data on development decisions are available only from 1991 onwards, however.

in the western part of the county that was developable as of 1991. Subsequent development decisions during the duration of the study period can then be examined.

The first step of the procedure is to calculate the propensity score by estimating the probability that a parcel is treated as a function of factors that affect the likelihood of treatment and factors that affect the outcome (i.e. development). The factors that are hypothesized to affect the likelihood of treatment are almost identical to those that have already argued should affect the development decision. However, a treated parcel is one that has a preserved neighbor, not one that is preserved itself. Nonetheless, parcel attributes are identified that are expected to affect preservation decisions to determine how they may be reinterpreted as factors affecting treatment. Recall that Howard County sets out specific criteria for ranking parcels for easement purchases. The county assigns a higher score to the application,

- the more agricultural or forested land in the vicinity of the parcel;
- the more active the agricultural operations on the parcel;
- the more protected land in the area;
- the larger the parcel size;
- the fewer acres subject to erosion or drainage problems on the parcel;
- the greater the proportion of LCC class 1, class 2, and class 3 soils on the parcel;
- and the greater the road frontage;

Although these criteria are intended to apply to the parcel being considered for preservation, many are characteristics that apply to that parcel's *surrounding* land use or characteristics that are likely to be spatially correlated factors and therefore similar among neighboring parcels. Therefore, there is every reason to expect a selection problem. To be more specific, in attempting to measure the effect on parcel A's development decision from having a neighbor, B, preserve, one needs to take into account the fact that features of the landscape that make B more likely to preserve will be a land use description of the neighborhood of both A and B, as well as physical and environmental characteristics of B that are likely not to vary much across the neighborhood of A and B. And, what is more, these very features are factors likely to affect the probability of development, as well. The variables that should help explain the probability of treatment clearly overlap almost exactly with the set that affects the development outcome. Fortunately, propensity score matching does not require separating out the effects of various explanatory variables on the outcome and the likelihood of being treated, but only that the analysis controls for them in testing for the treatment effect. Because of this, matching methods for estimating treatment effects seem particularly well-suited to deal with this otherwise confusing and confounding problem.

7.1 Results

The results of the initial specifications of the probit estimation are given in Table 7.2.[50] Interpretation is complicated by the fact that the variables are measured for the developable parcel and are included to explain whether that parcel is treated in the sense that a neighbor preserves. The probit does not directly estimate the probability of preservation but instead the probability of treatment, but these will be

---

[50] The full balanced specification included acres squared and the variable for surrounding land use in a developable state without a house squared.

similar for variables that are correlated in space. Fortunately, interpretation of net effects is not necessary in constructing propensity scores and correlation among covariates causes no particular problems.

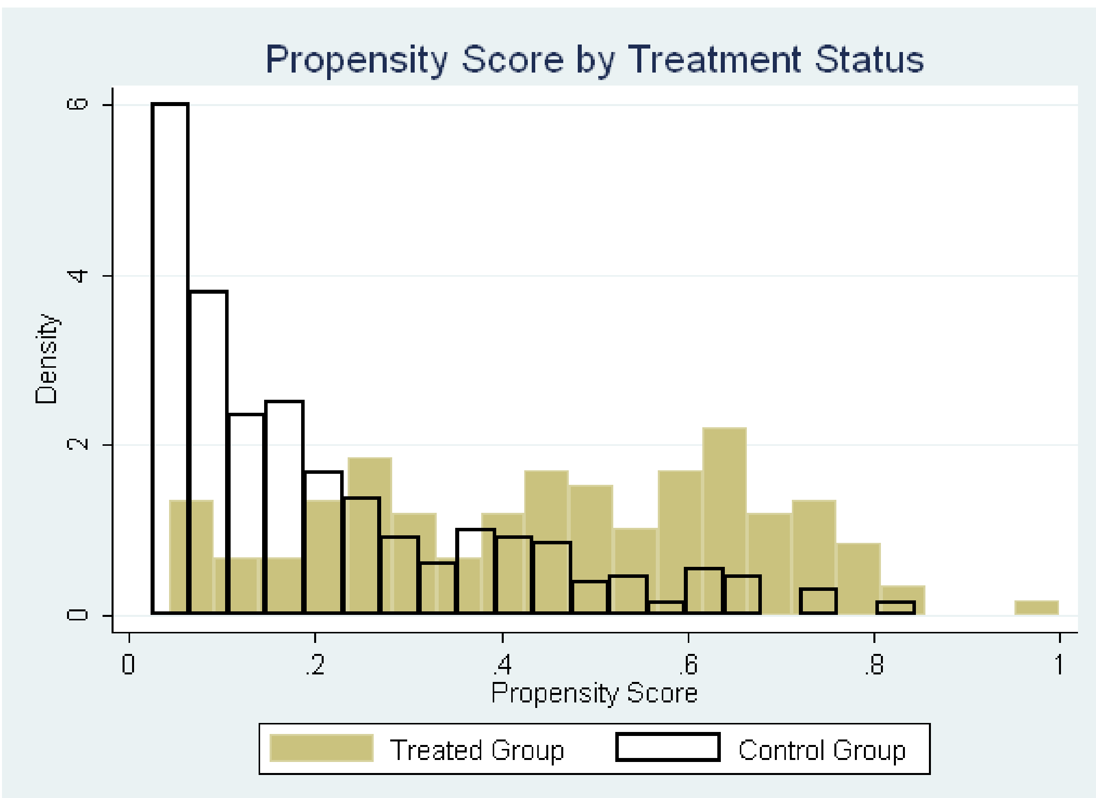
Table 7.2 Propensity score estimates

Variables	Coefficients	
Options variables		
Easement	-0.2208	
sEasement	0.6613	**
Development Returns		
distDC	0.0200	**
distBA	0.0082	
sluDevWithHs	0.0177	**
sluDevNoHs	0.0511	**
sluComm	-0.0167	
sluSubdiv	-0.0004	
sluPreserved	0.0057	
sluOpen	-0.0087	
sluRoad	-0.3657	*
sluProtected	-0.0258	
sluExempt	-0.0299	**
numLots	0.0109	**
reqOpenSpace	-0.1532	
Opportunity costs		
class1	-0.0159	
class2	-0.0064	
class3	0.0033	
class4	-0.0090	
agriculture	0.0109	**
acres	0.4477	
hasHouse	0.0371	
Conversion costs		
steep	-0.0094	*
forested	0.0093	
notRoadSuit	0.0109	
notSepticSuit	-0.0075	
Constant	-4.2256	**
N = 605	Psuedo R2 = 0.308	

* - significant at 10%, ** - significant at 5%

Once having obtained the propensity scores, the common support condition can be examined. Figure 7.1 provides the histogram of propensity scores for the treated and non-treated groups for both quasi-experiments. The x-axis measures the value of the predicted propensity score, calculated from the results of the probit analysis. The y-axis measures the percent of each sample (treated and non-treated) found in each predicted propensity score interval of approximately *5 percentage points*. Intervals for which there are treated observations but no non-treated observations available for controls are intervals over which the common support fails.

Figure 7.1: Histogram of propensity scores by treatment group.



Before calculating the average treatment on the treated, $\widehat{ATT}$, the outcome must be shown to be mean independent of the treatment, conditional on the propensity score. Given the conditional independence assumption set out in (24) above, this requires ensuring that the covariates in ***Z*** meet this condition, which is equivalent to achieving 'balance' between treatments and their controls. In layman's terms, balancing ensures that covariates in ***Z*** cannot be used to predict membership in the treatment or control group, i.e. the ideal situation of a random assignment has been recreated. Several balancing tests exist in the literature. The test used – commonly called regression based balancing – is suggested by Smith and Todd (2005a) and explained in more detail in Smith and Todd (2005b). The intuition behind this test is that after conditioning on *P(Z)*, any further conditioning on the ***Z*** vector should not provide new information on *D,* the treatment assignment. In other words, the balancing tests evaluate whether covariates in ***Z*** are informative of treatment assignment after conditioning on the propensity score.[51] If differences remain, then this suggests the propensity score model is mis-specified. Following Dehejia and Wahba (2002), cross products and squares of covariates are added to the specification until balancing is achieved.

Two features of balancing are important to note. First, it may not be possible to achieve balancing in some problems. Second, the balancing test does not provide a means of selecting conditioning variables; it only assures that any predictive power of treatment classification in the selected variables is squeezed out of the set of variables that is available. After balancing the covariates in *Z*, the propensity score calculated from *Z* is no longer informative as to which parcels are in the treatment or control

[51] Operationally, each covariate in regressed on the propensity score, the treatment dummy, the propensity score squared and cubed, and the propensity score, squared and cubed, interacted with the treatment dummy. The F test of all variables containing the treatment dummy equal to zero provides the test statistic.

group. In essence the procedure has recreated a random experiment. The final specification passes the balancing test suggested by Smith and Todd, when applied to the observations in the common support.[52]

The treatment is defined as having a neighbor that preserves in 1990-92, and the short run outcome measure is conversion after treatment and before 1997.[53] The eligible set includes only those developable parcels not treated before 1990 and includes 605 observations of which 12 are dropped because of a common support violation and 113 remain in the treated set. The remaining developable parcels are in the control set. The second quasi-experiment evaluates the outcome over a longer time horizon, from 1991 – 2001. In this experiment, fewer parcels drop from the common support (only one), because the optimal bandwidth is slightly wider for the longer outcome window. Table 7.3 reports the number of treated and untreated observations for each quasi-experiment and the number of observations that fall on the common support.

Table 7.3 Common Support Results

Conversion Time Lag – 5 years		
	On Support	Off Support
Treated	113	12
Controls	480	0
Conversion Time Lag – 10 years		
	On Support	Off Support
Treated	124	1
Controls	480	0

The results of the matching tests for both quasi-experiments are found in Table 7.4. The first column reports information about actual outcomes – specifically the percent of treated and untreated parcels that ultimately develop within each specified time

52 Output for the balancing tests is quite lengthy and thus available by request.
53 Of course, a parcel which converts then is treated is considered in the control group.

frame. For example, for the short run timeframe, 12.80% of the sample of parcels with neighbors that preserve in 1990-92 developed in the period from 1992 to 1997. In contrast, 13.75% of those parcels that were not treated developed between 1992 and 1997. For the longer outcome timeframe, 19.20% of the sample of parcels treated ultimately developed by 2001 and 22.29% of the untreated developed.

Table 7.4 Average Treatment Effects

Conversion Time Lag – 5 year		
	Unmatched	Kernel Estimate
Treated	12.80	13.27
Controls	- 13.75	- 4.84
Difference	-0.95	$\widehat{ATT}$ **8.43****
Conversion Time Lag – 10 years		
	Unmatched	Kernel Estimate
Treated	19.20	19.35
Controls	- 22.29	- 10.78
Difference	-3.09	$\widehat{ATT}$ **8.57****

Note: Significance levels based on 1,000 bootstrapped repetitions are: **: 5%, *: 10%.
The bandwidths are 0.01095 for 5 year lag and 0.0471 for the 10 year lag.
Kernel estimate is based on the Epanechnikov kernel.

The column marked 'Kernel Estimate' reports matching results for the Epanechnikov kernel matching algorithm. The first number reports the proportion of treated parcels that ultimately develop for the parcels that remain after eliminating those treated observations that violate the common support condition. The second item in this column reports the mean counterfactual. This is a re-weighted average value of the binary outcome, where the weights are defined as in equation (29).

The difference between the percent of actual treated and untreated parcels that subsequently convert is small, in both the short and long run experiments. These differences are not statistically different at even the 10% level and suggest that treated parcels show no greater propensity to develop than do control parcels. However, when the matching procedure is employed which involves limiting the treated set to those on the common support and comparing these treated parcels only with similar control parcels, the difference is much larger (8.43 percent and 8.57 percent for the short and long term experiment, respectively). This suggests that parcels that neighbor preservation were more than three times as likely as their control counterparts to develop within the short run period of five years following neighboring preservation and almost twice as likely during the ten year period. The decline in the difference in treatment effect with the longer time horizon suggests that any effect due to neighboring preservation declines with time.[54] To determine significance of the $\widehat{ATT}$ estimates, bootstrapped standard errors are calculated with 1,000 repetitions. In both the short and long term cases, the $\widehat{ATT}$ is significant at the 5% level.

Table 7.5 illustrates the ability of the matching estimator to mimic a controlled experiment. First compare columns 2 and columns 4 which are the raw means of the covariates by treatment status. There are large differences in the acreage (*acres*), amount of land in agriculture (*agriculture*), percentage of lands with steep slopes (*steep*), surrounding lands in farmsteads (*sluDevWithHs, sluDevNoHs*), as well as easement eligibility of the own parcel and neighbors' parcels (*sEasement*). Columns 6 and 8, under the heading "After Matching", display the same variables limited to the common support and using the weighted control observations. Notice the similarity now between acreage (55 acres for the treated versus 56 acres for

[54] More observations are on the common support in the long run outcome measure of 10 years so the samples are not identical.

controls), percentage of land in agriculture (66% treated versus 68% controls), amount of surrounding lands in farmsteads (31% versus 31%), own parcel easement eligibility (49% versus 45% for controls), and neighbors' eligibility (64% versus 63% controls). In summary, this table illustrates the power of the matching estimation procedure to convert a non-random assignment problem into one that approaches a randomized method. It compares treatment and weighted control observations that are very similar in observable covariates addressing the selection problems inherent in enrollment and the reliance on the functional form of a regression approach to evaluate a treatment outcome.

Table 7.5: Summary Statistics by Treatment Status (for the 5 year conversion time lag)

	Before Matching		After Matching	
	Treated Parcels	Untreated Parcels	Treated Parcels	Untreated Parcels
			Common Support	Weighted Common Support
Variable	Mean	Mean	Mean	Mean
Easement	0.5360	0.2979	0.4956	0.4573
sEasement	0.6800	0.3083	0.6460	0.6324
distDC	54.8163	49.7845	54.5850	54.3459
distBA	41.1578	38.6898	41.0783	40.2384
sluDevWithHs	32.7515	28.3911	31.6471	31.0966
sluDevNoHs	31.0519	21.2223	31.3326	31.7939
sluComm	0.6221	1.6010	0.6871	0.7129
sluSubdiv	5.6651	7.4987	5.7150	6.1485
sluPreserved	8.3900	5.0804	8.4643	8.6221
sluOther	1.3591	2.9414	1.4568	1.5516
sluOpen	0.0408	0.4108	0.0451	0.0546
sluRoad	3.3440	5.1226	3.5002	3.7935
sluProtected	2.2470	5.8327	2.3354	2.0190
sluExempt	0.2625	1.2995	0.2903	0.2989
numLots	13.3360	10.4750	12.2832	12.3539
reqOpenSpace	0.8000	0.6813	0.7788	0.7816
class1	3.4122	3.3325	3.3704	2.9476
class2	51.8468	54.3978	51.4276	50.7769

class3	26.8231	24.5561	26.1902	27.6638
class4	13.8549	13.5007	14.6862	12.9518
agriculture	67.4830	54.0100	66.8907	68.4552
Acres	60.597	46.571	55.794	56.206
hasHouse	0.4640	0.4958	0.4690	0.4395
Steep	4.5002	11.1094	4.6943	4.8946
forested	28.1272	34.0313	28.3235	27.1504
notRoadSuit	35.4083	36.3616	36.1388	36.3186
notSepSuit	38.4003	44.7975	39.0636	38.9852
Observations	125	480	113	452
Weighted Number of Observations				113

Note: 1 – Weighted using the kernel weights from the Epanechnikov kernel.

7.2 Conclusion

Testing the effects of policies and programs on land use decisions is not an easy task. This is especially true when the effect being tested is some type of spatial interaction. Because the landscape is characterized by so much spatial correlation, it is empirically difficult to distinguish between true interactions between outcomes, and outcomes that are correlated because they are affected by correlated exogenous variables. In other words, it is difficult to provide evidence of causation as opposed to correlation.

Following matching methods developed in the labor literature, propensity score matching is used to test for a treatment effect. As pointed out, it is not possible to design a quasi-controlled experiment that tests the same hypothesis as is tested in the hazard model because the hazard simulates a dynamic process while the matching tests are essentially static. In the matching framework, two outcomes are considered, one that tests the effect of the preservation actions on subsequent development in the short run and a second that allows the effect to be measured in the long run. The propensity score matching method reveals significant effects from treatment. Parcels with preserved neighbors are significantly more likely to develop subsequent to their neighbor's preservation than those without such a treatment. However, the difference in the calculated treatment effect in the short and long run experiments is very small suggesting that the spillover effect occurs quickly and remained stable into the long run. The size of the effect is estimated to be about 8.5% - that is developable parcels in the western part of Howard County are about 8% more likely to developable if a neighbor preserved than a counterpart with no preserving neighbor.

This chapter provides evidence for the contention that preserved open space is likely to induce more neighboring development, holding other things equal. Thus preservation programs, if not designed carefully, may actually encourage landscape fragmentation by setting in motion a path dependent process that encourages a checkerboard pattern of preservation and development. Knowing of the existence of this spillover effect may help the public sector design land use policies with a higher probability of achieving their stated goals.

8. What Does It All Mean?

This dissertation quantifies two outcomes of a voluntary easement program designed to preserve farmland in perpetuity. One outcome, an induced delay in conversion timing, is unexpected and desirable for a county attempting to control the pace of development. The second is an unintended consequence, i.e. a negative impact, because it suggests that preserved lands attract development activity. This unintended consequence may be unavoidable but should be quantified if one wishes to evaluate the true impact of a land preservation program or design a policy to minimize this effect.

A primary goal of this dissertation was to determine if an easement option impacts conversion decisions, and, if so, to quantify the temporal impact of the option on landowners that do not preserve. The results from complex competing risks models which closely mimic the actual decision process suggest that the easement program significantly delays the conversion of eligible parcels. This delay is estimated at 7 years for parcels that qualify for the program without assistance from adjacent parcels and the conversion rate is reduced by over 45% for all easement eligible parcels.

For policy makers the induced delay is important, especially in counties just beginning a period of rapid growth. These results suggest that having an alternative option to land conversion not only has the direct impact of preserving parcels, but also significantly delays the conversion decisions of parcels, eligible to preserve, that may ultimately choose not to participate in the program. The resulting delay is important because eligible parcels are larger than average and are capable of producing many new housing units. This new housing, in turn, puts pressure on schools, roads, utilities, and other infrastructure. These pressures are often ultimately alleviated by county public spending on infrastructure, but the delay in conversion might allow the county needed time to cope with financial pressures and may, additionally, soften speculative housing demand driven by ‘boom’ cycles in housing construction. Also, the preservation program may alleviate the need for command and control programs to limit conversion rates which are often challenged on legal grounds.

However, not all consequences of an easement program are desirable. While qualified parcels may delay conversion decisions, it appears that parcels that neighbor a preserved parcel are more likely to develop. These permanently preserved parcels create positive spillover effects that make neighboring parcels more attractive to development. Spillover effects could arise from the scenic nature of farmland or simply from the reduced uncertainty surrounding future land uses which are inherent in non-preserved parcels. This spillover effect may exacerbate development into agricultural areas and has the potential to produce the undesirable patterns of growth many counties are aiming to prevent. Thus being aware of this potential impact in the policy design phase may prove crucial to promoting a desirable pattern of growth.

The number of localities contemplating policies to preserve land is growing each year, so the question of generalization of these results is an important one. However, the uniqueness of the area under study may complicate broad generalization of these findings. The study area, Howard County, Maryland, is unique for several reasons. First, it is a wealthy exurban county and is relatively autonomous with regard to land use policy because the reliance on state funds, for schools and infrastructure, is less than an average county. Second, the county is under intense growth pressure but at the same time has a long history as an agricultural county. There is considerable will within the existing population to fund preservation of this historical land use. Thus, the key findings of the dissertation may generalize to a limited number of areas at this point in time, but as easement programs and farmland preservation programs gain in popularity these results will prove useful in many more localities. As more counties contemplate and implement incentive based land use regulation the need to design policy, forecast outcomes, and evaluate impacts of these regulations will grow in importance and the results derived in this dissertation should prove useful at each point in this continuum. This work should inform policy makers on design issues of future policies and outline methods for researchers to quantify impacts which account for many of the inherent problems that are prevalent in land use policy analysis.

Appendix A: The easement payout worksheet.

Figure A.1: Payout formula

Agricultural Land Preservation Board
EASEMENT PRICE FORMULA
WORKSHEET

Based on Resolution No. 34-1994

1. **Base** - all eligible farms receive 100 base points ______100

2. **Parcel Size** ______ ≥100 acres with agreement not to subdivide ____ acres/2 = ______
   ______ <100 acres or no agreement = 0 points
   Maximum 150 pts

3. **Soils** Class I __________ acres x 2.0 = ______
   Class II __________ acres x 1.5 = ______
   Class III __________ acres x 1.0 = ______
   Combined Maximum 200 pts

4. **Road Frontage** - total feet of public road frontage (up to 2500 feet) ______/50 = ______
   Maximum 50 pts

5. **Zoning** - in the RC zoning district
   ______ Yes = 75 points ______ No = 0 points ______

6. **Lots Eliminated** - agreement to not take allowed lots
   No. of lots ______ x 5 points = ______

7. **Tenant Dwellings** -
   Existing tenant houses at rate greater than 1 per 50 acres
   No. of additional tenant dwellings ______ x -10 = ______

   Agreement to not exceed 1 per 50 acres for future tenant houses
   ______ Yes = 10 points ______ No = 0 points ______

8. **LESA** - score
   ____ ≥200 = 25 points ____ ≥175 to <200 = 10 points ____ <175 = 0 points ______

   SUBTOTAL ______

9. **Importance to agricultural community**
   Advisory Board may add or subtract up to 50 points ______

(Maximum 550 Points) TOTAL PRICE POINTS ______

Points

550 x $12 = $ 6600 / acre X ______ acres = ______ TOTAL PRICE

+ 50 (Ag Board Points)

x $12 = $ ______ x ______ acres = $ ______ TOTAL PRICE

______ Name ______ Acres ______ Tax Map ______ Parcel ______ Date

formula.worksheet.

Appendix B: Data

Table B.0.1 Data Sources

Source	Label
1990 Census files(maps or data)	**Census**
Howard County GIS	**HC GIS**
Howard County Tax Assessment	**HC Tax**
Howard County General Plan (1990)	**HC GP**
Maryland Department of Transportation	**MDT**
Maryland Department of Planning	**MDP**
Natural Soils Maps (Natural Resources Conservation Services)	**NRCS**
Soil Survey Geographic Database (NRCS & National Cartography and Geospatial Center, NCGC)	**SSURGO**

Table B.2 Variables and Sources

Variable	Source
Easement	HC GIS, HC Tax
Variance measure	HC GIS, HC Tax
Drift measure	HC GIS, HC Tax
distBA	MDT
distDC	MDT
sluDevWithHs	HC GIS, HC Tax
sluDevNoHs	HC GIS, HC Tax
sluComm	HC GIS, HC Tax
sluSubdiv	HC GIS, HC Tax
sluPreserved	HC GIS, HC Tax
sluOpen	HC GIS, HC Tax
sluRoad	HC GIS, HC Tax
sluProtected	HC GIS, HC Tax
sluExempt	HC GIS, HC Tax
devRate	HC Tax, Census
popDen	HC Tax, Census
numLots	HC Tax, MDP, HC GP
reqOpenSpace	HC Tax, HC GP
class1	NRCS
class2	NRCS
class3	NRCS

class4	NRCS
Agriculture	MDP
Acres	HC GIS, HC Tax
hasHouse	HC GIS, HC Tax
Steep	NRCS
Forested	MDP
notRoadSuit	SSURGO
notSepticSuit	SSURGO
sewerPlanned	MDP
intRate	US Federal Reserve
APFO	Howard County Council Legislative Record

Bibliography

Abbring, J. and G. van den Berg. 2003. “The identifiability of the mixed proportional hazards competing risks model,” *Journal of the Royal Statistical Society* 65(3):701-710.

Allison, P. 1997. *Survival Analysis Using the SAS® System: A Practical Guide*, Cary, NC: SAS Institute Inc. 292 pp.

American Farmland Trust. 2005. *Fact Sheet Status of Local PACE Programs*, American Farmland Trust Farmland Information Center, Northampton, MA

Black, D. and J. Smith. 2004. “How robust is the evidence on the effects of college quality? Evidence from matching,” *Journal of Econometrics* 121: 99–124.

Bockstael, N. 1996. “Modeling Economics and Ecology: The Importance of a Spatial Perspective”, *American Journal of Agricultural Economics*, 785, 1168-1180.

Box-Steffensmeier, J. and B. Jones. 1997. “Time is of the Essence: Event History Models in Political Science,” *American Journal of Political Science* 41:1414-1461.

Brownstone, David and A.De Vany. 1991. “Zoning, Returns to Scale, and the Value of Undeveloped Land,” *The Review of Economics and Statistics* 73(4):699-704.

Bulan, L., C. Mayer, and T. Somerville. 2002. “Irreversible Investment, Real Options, and Competition: Evidence from Real Estate Development,” *Working Paper*.

Capozza, D. R., and R. Helsley. 1990. “The Stochastic City,” *Journal of Urban Economics* 28:187-203.

Capozza, D. and Y. Li. 1994 “The Intensity and Timing of Investment: The Case of Land.” *American Economic Review* 84: 889-904.

Carrion-Flores, Carmen and E. Irwin. 2004. “Determinants of Residential Land-Use Conversion and Sprawl at the Rural-Urban Fringe,” *American Journal of Agricultural Economics* 86(4): 889-904.

Collett, D. 1994. *Modelling Survival Data in Medical Research*, London: Chapman & Hall.

Cox, D.R. 1959. “The Analysis of Exponentially Distributed Life-Times with Two Types of Failure,” *Journal of the Royal Statistical Society* 34:187-220.

Cox, D.R. 1962. *Renewal Theory.* London: Metheun.

DiNardo, J. and J.L. Tobias. 2001. "Nonparametric Density and Regression Estimation." *Journal of Economic Perspectives* 15: 11-28.

Dehejia, R. and S. Wahba. 2002. “Propensity Score Matching Methods for Nonexperimental Causal Studies”, *Review of Economics and Statistics*, 84(1): 151-161.

Deng, Y., J. Quigley, and Robert van Order. 2000. “Mortgage Terminations, Heterogeneity and the Exercise of Mortgage Options,” *Econometrica* 68(2):275-307.

Dixit, Avinash and Robert Pindyck. 1994. *Investment Under Uncertainty*. Princeton University Press, Princeton, NJ.

Duke, J. 2004. “Participation in Agricultural Land Preservation Programs: Parcel Quality and a Complex Policy Environment.” *Agricultural and Resource Economics Review* 33: 34-49.

Fallick, B. and K. Ryu. 2007. “The Recall and New Job Search of Laid-off Workers: A Bivariate Proportional Hazard Model with Unobserved Heterogeneity,” *The Review of Economics and Statistics,* 89(2):313-323.

Flinn, C. and J. Heckman. 1982. “New Methods for Analyzing Structural Models of Labor Force Dynamics,” *Journal of Econometrics* 18:115-168.

Frolich, M. 2004. “Finite-Sample Properties of Propensity-Score Matching and Weighting Estimators,” *Review of Economics and Statistics* 86(1):77-90.

Geltner, D., T. Riddiough, S. Stojanovic. 1996. “Insights on the Effect of Land Use Choice: The Perpetual Option on the Best of Two Underlying Assets.” *Journal of Urban Economics* 39: 20-50.

Geoghegan, J., L. Lynch, and S. Bucholtz. 2003. “Capitalization of Open Spaces,”*Agricultural and Resource Economics Review* 32(1):33-45.

Gordon, S. 2002. “Stochastic Dependence in Competing Risks,” *American Journal of Political Science* 46(1):200-217.

Gritz, M. 1993. “The Impact of Training on the Frequency and Duration of Employment,” *Journal of Econometrics* 57(1-3)21-51.

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

Han, A. and J. Hausman. 1990. “Flexible Parametric Estimation of Duration and Competing Risk Models,” *Journal of Applied Econometrics* 5(1):1-28.

Heckman, J. and B. Singer. 1982. “The Identification Problem in Econometric Models for Duration Data,” invited paper in *Advances in Econometrics*, Proceedings of the Fourth World Congress of the Econometric Society, ed. By W. Hildenbrand. Cambridge University Press.

Heckman, J. and B. Singer. 1984. “A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data,” *Econometrica* 52:271-320.

Heckman, J., H. Ichimura, J. Smith, and P. Todd. 1998. “Characterizing Selection Bias Using Experimental Data,” NBER Working Paper: 6699.

Heckman, J., H. Ichimura, and P. Todd. 1997. “Matching as an Econometric Evaluation Estimator: Evidence from Evaluation a Job Training Programme,” *Review of Economic Studies* 64(4):605-54.

Heckman, J., H. Ichimura, and P. Todd. 1998. “Matching as an Econometric Evaluation Estimator,” *Review of Economic Studies* 65(2):261-94.

Heckman, J. and B. Honore. 1989. “The Identifiability of the Competing Risks Model,” *Biometrika* 76(2):325-330.

Honore, B. 1990. “Simple Estimation of a Duration Model with Unobserved Heterogeneity,” *Econometrica* 58(2):453-73.

Hite, D., B. Sohngen, and J. Templeton. 2003. “Zoning, Development Timing, and Agricultural Land Use at the Suburban Fringe: A Competing Risks Approach.” *Agricultural and Resource Economics Review* v32, n1 (April): 145-57.

Irwin, Elena. 1998. *Do Spatial Externalities Matter: The Role of Endogenous Externalities in the Evolution of Land Use Pattern*, Ph.D. Dissertation, Department of Agricultural and Resource Economics, University of Maryland.

Irwin, Elena and Nancy Bockstael. 2002. "Interacting Agents, Spatial Externalities and the Evolution of Land Use Change," *Journal of Economic Geography* 2(1): 31-54.

Kalbfleisch, J. D. and R. L. Prentice. 1980. *The Statistical Analysis of Failure Time Data*. New York: John Wiley & Sons.

Klein, J. and M. Moeschberger. 1997. *Survival Analysis: Techniques for Censored and Truncated Data*, New York: Springer-Verlag.

Kline, J. and R. Alig. 1999. "Does Land Use Planning Slow the Conversion of Forest and Farm Lands?" *Growth and Change* 30(1):3-22.

Lancaster, T. 1990. *The Econometric Analysis of Transition Data*. Cambridge: Cambridge University Press.

Lechner, M. 2002. "Program Heterogeneity and Propensity Score Matching: An Application to the Evaluation of Active Labor Market Policies," *The Review of Economics and Statistics* 84(2): 205-220.

Lillard, L. 1993. "Simultaneous Equations for Hazards," *Journal of Econometrics* 56:189-217.

Lillard, L. and W.A. Panis. 2003. *aML Multilevel Multiprocess Statistical Software, Version 2.0*. EconWare, Los Angeles, California.

Lynch, Lori, and S. J. Lovell, "Combining Spatial and Survey Data to Explain Participation in Agricultural Land Preservation Programs," *Land Economics* 79(2) (May 2003):259-276.

Lynch, Lori and X. Liu, "Impact of Designated Preservation Areas on Rate of Preservation and Rate of Conversion," American Journal of Agricultural Economics, December 2007.

Lynch, L. and J. Carpenter. 2003. "Is There Evidence of a Critical Mass in the Mid-Atlantic Agriculture Sector between 1949 and 1997?" *Agricultural and Resouce Economics Review* v32, n1 (April): 116-28

Manski, Charles. 1993. "Identification of Endogenous Social Effects: The Reflection Problem", *Review of Economic Studies,* 60: 531-42.

Manski, Charles. 1995. *Identification Problems in the Social Sciences,* Cambridge, MA: Harvard University Press.

Maranthe, Rahul and S. Ryan. 2005. "On the Validity of the Geometric Brownian Motion Assumption," *The Engineering Economist* 50: 159-92.

Maryland Department of Planning. 2008. <http://www.mdp.state.md.us/zip_downloads_accept.htm>.

Mayer C. and C. Somerville. 2000. "Land Use Regulations and New Construction," *Regional Science and Urban Economics* 30:639-62.

McCall, B. 1996. "Unemployment Insurance Rules, Joblessness, and Part-Time Work," *Econometrica* 64(3):647-682.

McMillen, D. 1989. "An Empirical Model of Urban Fringe Land Use", *Land Economics* 652: 138-145.

McMillen Daniel and J. McDonald. 2002. "Land Values in a Newly Zoned City," *The Review of Economics and Statistics* 84(1):62-72.

Meyer, B. 1990. "Unemployment Insurance and Unemployment Spells," *Econometrica* 58(4):757-82.

Nickell, S. 1979. "Estimating the Probability of Leaving Unemployment," *Econometrica* 47:1249-1264.

Nickerson, C. 2000. "Characterizing Participation in Farmland Preservation Programs." Ph.D. Dissertation, Department of Agricultural and Resource Economics, University of MD.

Nickerson, C. and C. Barnard. 2004. "The Revealed Importance of Federal Farmland Protection Objectives." Paper presented at the NAREA Meeting, Halifax, Nova Scotia, June 20-23.

Nickerson, C. and L. Lynch. 2001. "The Effect of Farmland Preservation Programs on Farmland Prices," *American Journal of Agricultural Economics* 83(2):341-51.

Parks, Peter. 1995. "Explaining 'Irrational' Land Use: Risk Aversion and Marginal Agricultural Land," *Journal of Environmental Economics and Management* 28:34-47.

Plantinga, A., R. Lubowski and R. Stavins. 2002. "The Effects of Potential Land Development on Agricultural Land Prices," Journal of Urban Economics 52: 561-81.

Ridder, G. and T. M. Woutersen. 2003. "The Singularity of the Information Matrix of the Mixed Proportional Hazard Model," *Econometrica* 71(5): 1579-1589.

Roe, B., E.G. Irwin, and H.A. Morrow-Jones. 2004. "The Effects of Farmland, Farmland Preservation, and Other Neighborhood Amenities on Housing Values and Residential Growth, Land Economics 80(1):55-75.

Rosenbaum, P. and D. Rubin. 1985. “Constructing a Control Group Using Multivariate Matched sampling Methods that Incorporate the Propensity Score.” *American Statistician* 39: 33-38.

Smith, Jeffrey and P. Todd. 2005a. “Does Matching Overcome LaLonde’s Critique of Nonexperimental Estimators?” *Journal of Econometrics*, 125: 305-353.

Smith, Jeffrey and P. Todd. 2005b. “Rejoinder,” *Journal of Econometrics*, 125: 365-

Soil Survey Staff, Natural Resources Conservation Service, United States Department of Agriculture. Soil Survey Geographic (SSURGO) Database for *Howard County, Maryland* [Online WWW]. Available URL: "http://soildatamart.nrcs.usda.gov" [Accessed *11/2004*].

Stavins, Robert and A.Jaffe. 1990. “Unintended Impacts of Public Investments on Private Decisions: The Depletion of Forested Wetlands,” *The American Economic Review* 80(3):337-352.

Steele, Fiona, and S. Curtis. 2003. “Appropriate Methods for Analyzing the Effect of Method Choice on Contraceptive Discontinuation,” *Demography* 40(1):1-22.

Towe, C., C. Nickerson, and N. Bockstael. 2007. “An Empirical Examination of the Timing of Land Conversions in the Presence of Farmland Preservation Programs,” *American Journal of Agricultural Economics* Forthcoming.

Tsiatis, A. 1975. “A Nonidentifiability Aspect of the Problem of Competing Risks,” *Proceedings of the National Academy of Science* 20-22.

Trust for Public Land. 2005. *Commonly Requested Conservation Finance Figures*. Online at http://www.tpl.org/content_documents/LandVote_database_summaries.pdf.

Van Den Berg, G. 2005. “Competing Risks Models,” IFAU – Institute for Labour Market Policy Evaluation - Working paper.

Van Den Berg, G., M. Lindeboom, and G. Ridder. 1994. “Attrition in Longitudinal Panel Data and the Empirical Analysis of Dynamic Labour Market Behaviour,” *Journal of Applied Econometrics* 9(4):421-435.

Weinke, A., K. Arbeev, I. Locatelli, and A. Yashin. 2005. “A Comparison of Different Bivariate Correlated Frailty Models and Estimation Strategies,” *Mathematical Biosciences*. 198:1-13.