The Causes and Consequences of Organizational Change: How to Deal with Unobserved Heterogeneity

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Studies analyzing internal organizational change from an ecological perspective typically deal with two issues. The first issue concerns the likelihood of internal organizational change. Studies in this tradition analyze the determinants of change processes. The second issue that has been extensively researched concerns the effect of change on performance. In these studies organizational performance is usually operationalized as survival or growth. Unfortunately, so far empirical studies on both issues have been producing rather inconsistent or implausible results. In this paper we argue that this is at least in part due to a methodological problem that usually is ignored: unobserved heterogeneity. Unobserved heterogeneity has the potential to bias estimation results severely. We demonstrate this by providing a series of estimation results with several data sets. We compare standard models, as they have been used in the literature, with fixed-effect panel regression models that control for unobserved heterogeneity. Results obtained with fixed-effect models differ dramatically from those found with standard models.

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1. Introduction

A standard criticism of population ecology theory has been that it neglects internal organizational change processes because it emphasizes structural inertia. However, as Hannan and Freeman (1984) explain, the observation that organizations do change internally is not incompatible with assumptions related to the inertia argument. Ecological theory stresses difficulties and dangers that go along with internal organizational change: “In a world of high uncertainty, adaptive efforts ... turn out to be essentially random with respect to future value.” (Hannan and Freeman 1984: 150). Given that organizational change is costly, it follows from the assumption of random value of change that organizations, which change their structures, will suffer higher mortality rates and lower growth rates than those that do not change.

Obviously, this was a bold statement amidst the change-craze propagated by the management literature of the 80ies. Therefore, many scholars tested the simple assertion that change has a negative effect on organizational survival. Results, however, often did not support this proposition, i.e. organizational change did not increase mortality rates (e.g. Singh et al. 1986, Zucker 1987, Haveman 1992).

However, the Hannan/Freeman argument is more complex. The authors distinguish core and peripheral changes. Core changes, they claim, should be especially rare and hazardous. Contrary, changes in an organization’s periphery occur frequently and should not have significant negative effects on an organization’s survival chances or could even have a slightly positive effect (Kelly and Amburgey 1991). Results from some studies gave support to the core/periphery distinction; others did not (e.g. Singh et al. 1986, see also the review in Baum 1996).

In another refinement, ecologists distinguished between the process and the content effects of organizational change (Barnett and Carroll 1995). The short-term effect of an organizational change process is assumed to be always detrimental while the long-term effect can be beneficial. The content effects, i.e. the effects of the different environmental states between which an organization switches when it undertakes a core change, can distort the process effects when they are not controlled for. This is because the different environmental states are connected with different survival and success chances. Distinguishing between process and content effects of organizational change has improved the quality of results of ecological studies significantly. Nonetheless, important inconsistencies remain. Some authors could confirm detrimental short-term effects of organizational change (e.g. Amburgey et al. 1993, Minkoff 1999, Dowell and Swaminathan 2000) while others found the opposite (Delacroix and Swaminathan 1991, Carroll and Teo 1996, Stoeberl et al. 1998).

The standard approach in this kind of studies is to estimate success regressions (mostly with survival as the dependent process) and to introduce a time-varying change dummy and a time-varying change-clock. The change dummy should then decrease success (the negative process effect), and the change-clock should increase it (if the content of change increases the organization-environment fit).

However, a potential problem with this procedure is that change might depend on performance (reverse causality). It is highly plausible that some change events are induced by success (expansion of production facilities, change of legal form, etc.). Other change events are presumably mostly induced by low success (change in leadership, etc.). Then, if one is not able to take into account the factors that cause the varying performance level of firms (because one has not measured these variables), estimates from change models could be heavily biased due to unobserved heterogeneity. For instance, a negative effect of success-induced change might be “overshadowed” by the fact that such change is pursued only by successful firms. Or a positive effect of failure-induced change may be concealed, because these changes are mostly found in failing firms. Therefore, to obtain unbiased estimates of change effects, one has to use models that take unobserved heterogeneity into account. Since this has not be done so far in ecological studies, bias due to unobserved heterogeneity might be the reason for inconsistencies in the results.

A similar problem occurs in ecological studies, which analyze determinants of organizational change. The problem is connected with the estimation of the effect of prior change on further change. Concerning this effect the „repetitive-momentum“ hypothesis is especially prominent, i.e. with increasing number of prior changes there is a rising tendency to change again. It is argued that this is because organizational members acquire increasing competencies and confidence through the repetitive execution of a certain kind of change. Quite some studies confirmed the momentum hypothesis, i.e. they found a positive effect of the number of prior changes on the change rate (e.g. Delacroix and Swaminathan 1991, Amburgey et al. 1993, Ginsberg and Baum 1994, Schulz 1998b, Minkoff 1999, Dobrev et al. 2001).

Interestingly, this reasoning has hardly ever been questioned. However, we would argue that the momentum hypothesis is theoretically not convincing. To us the opposite argument seems more plausible, i.e. a change refines a non-optimal element of the organizational structure, which subsequently should have a lower need of further change. Therefore, we would expect a negative effect of prior changes on the change rate.

At first sight the empirical evidence speaks for the momentum hypothesis. However, we maintain that the momentum effect could again be the result of unobserved heterogeneity. Given that some organizations have a higher inherent propensity to change than others – e.g. because of greater environmental volatility or because a high valuation of change is part of their corporate identity -, they accumulate of course more changes over their "life course". As a consequence, firms with a high propensity to change dominate the risk set at high numbers of prior change. Therefore, the rate of further change might be estimated to depend positively on the number of prior changes, even though the "true" effect is negative. Thus, again, if one does not control for unobserved heterogeneity (in this case the propensity to change) estimation results might be heavily biased.[1]

In Section 2 we will suggest a method for dealing with unobserved heterogeneity in studies analyzing internal organizational change. The rest of the paper contains several demonstrations of how severe this problem is in real data. We use four different data sets that are described in Section 3. In Section 4 we compare the results obtained from standard models and from the model that we propose.

2. The Problem and a Solution

In Figures 1 and 2 we give stylized examples that should demonstrate, why it is important to control for unobserved heterogeneity (i.e. the performance level of a firm) when estimating the effect of change on success. Figure 1 shows two different firms for which we have panel data for 10 time points. Firm A has a higher performance (e.g. profit, growth, etc.) than firm B over the whole observation period. Therefore, firm A has a higher probability to implement a kind of change that is mainly undertaken by successful firms, e.g. the expansion of the product line, etc. We assume that firm A undertakes such a change at t_5. Further, we assume

[1] Several earlier studies mentioned that unobserved heterogeneity might be a problem for studies of organizational change (Barnett and Carroll 1995, Sinha and Noble 1997, Schulz 1998b). However, only Sinha and Noble (1997) tried to solve the problem by using a fixed-effect model – as we will do below.

that the short-term effect of this change is negative; in the long run the previous performance level is attained again.

Now, what would be the estimation result from a standard (OLS) regression, where performance is the dependent and a change dummy the independent variable? The "true" effect of the dummy is negative, i.e. change lowers performance. However, a standard regression yields the opposite result. It can be easily seen that the performance mean of all spells before a change (that means all spells of firm B plus the first 5 spells of firm A) is lower than the performance mean of the spells after the change (the second 5 spells of firm A). Therefore, a standard regression model, which does not account for the different (inherent) performance levels of the two individual firms, estimates a spurious positive effect of change on performance. This result is generalizable to real data. In a sample of organizations estimation results will be biased upwards if change is positively correlated with success. Analogously, if the low performers are more likely to execute a certain type of change, results will be biased downwards.

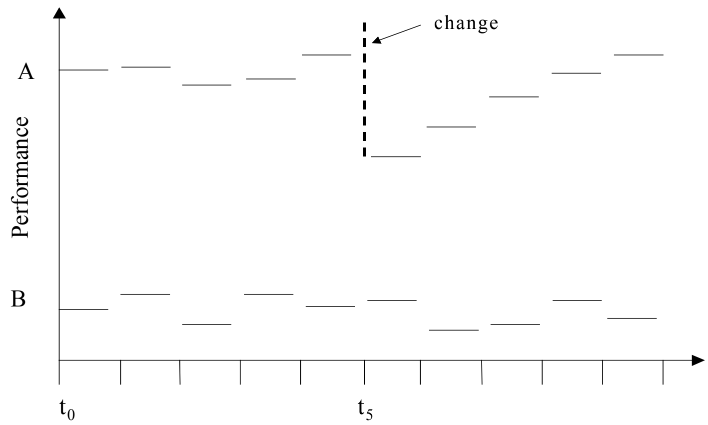

Figure 1: Two firms with different performance levels. Change has a negative short-term effect on performance.

In Figure 2 we assume that change does not affect performance. Compared to Figure 1, a standard model would estimate an even higher positive effect of change since the mean of the spells after the change is higher than it is in Figure 1.

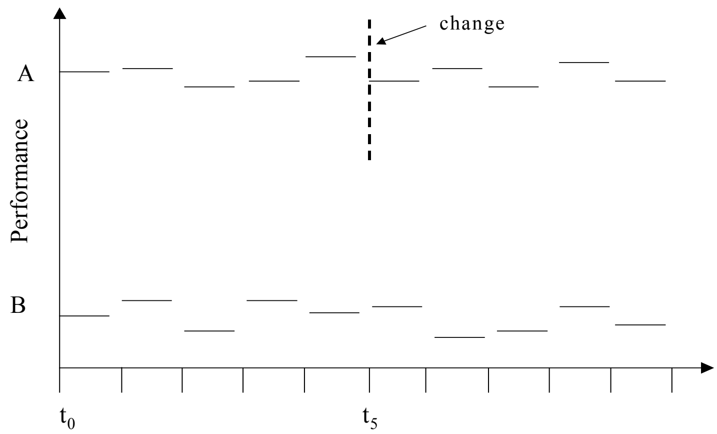

Figure 2: Two firms with different performance levels. Change has no effect on performance.

These examples demonstrate that with standard models there is a potential for severely biased estimates if change is correlated with performance. The problem could be tackled by using simultaneous performance-change models. However, such models rely on strong assumptions. Luckily, with panel data there is a more elegant solution. As one can see from Figures 1 and 2, it would suffice to control for the performance level of the firms. This is exactly what fixed-effect regression models do: The idea of fixed-effect models is to include a dummy for every firm in the panel regression (this is possible because with panel data one has more than one observation per firm). This firm-specific dummy captures all (time-constant) unobserved heterogeneity, i.e. heterogeneity that is not caught by the observed variables. In our example the difference in the average performance levels of firms A and B would be captured by the two dummy variables. This would result in an unbiased estimate of the change effects, i.e. with a fixed-effect regression the change dummy would show a negative (Figure 1) respectively a zero (Figure 2) effect.

With large data sets the estimation of a firm-specific dummy would be somewhat cumbersome. This however, can be avoided. A fixed-effect model can be formulated as an error component model (see Allison 1994):

$$Y_{it} = \beta X_{it} + \alpha_i + \varepsilon_{it}.$$

Y_{it} is performance for firm i in year t. X_{it} is the change dummy, which is zero for firm i before and one after a change occurred. α_i is a firm-specific error term and ε_{it} is a standard error term. α_i can either be estimated by a firm-specific dummy (this estimator is called least squares dummy variable estimator) or more elegantly by subtracting $\overline{Y}_i$ from the Y_{it} values. This cancels α_i and standard OLS can be applied (difference estimator). No matter which estimation method one uses this model controls for all time-constant (fixed) heterogeneity between firms (no time-constant variables can be included in the model). Therefore, this model is able to provide an unbiased estimate of the change effect. The only assumption that must hold (besides standard regression assumptions for sure) is that α_i does not change over time.

This is our proposed solution to the problem of unobserved heterogeneity in change models. But this solution is only applicable, if one analyzes metric performance variables (like profit, growth, etc.). Usually, however, in ecological studies researchers analyze failure times when evaluating effects of organizational change on the performance of organizations. Survival chances of an organization can also be regarded as a manifestation of organizational performance. Typically, event history regressions on the mortality rate with a time-varying change dummy are estimated. In this context our arguments for a potential bias apply accordingly. For instance, if the probability of a change correlates positively with survival probability and change has a negative effect, a standard model will yield a spurious positive effect (i.e. mortality rates after a change are estimated to be lower). Unfortunately, there exists no fixed-effect solution for single-episode (mortality can only happen once) data. In this situation a random-effect model could be applied, but experience with these models shows that results depend heavily on (untestable) distributional assumptions (Yamaguchi 1985). A more promising solution for single-episode data could be the use of simultaneous event history regression models (Lillard 1993, Lillard and Panis 2000). However, these models also make use of untestable distributional assumptions. We will investigate the usefulness of these models in future work. In this paper we will only explore fixed-effect models.

We now consider the problem of unobserved heterogeneity when estimating the effect of previous changes on the likelihood of further change. Figure 3 shows again a stylized example of four firms with different inherent change propensities, which are assumed to be constant over time. In addition we assume that the number of prior changes does not affect the change propensity. The „true“ effect of prior change on further change is therefore assumed to be zero in our example. Firm D has the lowest (zero) change propensity, firm A the highest. Accordingly, we would expect different numbers of change for these four firms in a given observation period. We assume that firm D experienced no change, firm C one, firm B two, and firm A three. These are multi-episode date, since firms can have more than one event. A standard model – as used in the literature – would be a multi-episode change rate regression with the number of prior changes as covariate. What would be the result with our stylized data? The risk set at zero changes includes all four firms. Therefore, the change rate in the beginning is estimated to be the average of all four firms. After the first change has happened, however, the risk set is reduced: firm D is no longer included. Thus, the change rate is higher, because it is the mean of firms C, B, and A. And so on. The result would be that with increasing number of changes the rate of further change increases! This, however, is solely due to the fact that the composition of the risk set changes with the number of prior changes: at higher numbers of change only the high-risk firms are left. Thus, again, standard regressions yield biased estimates.

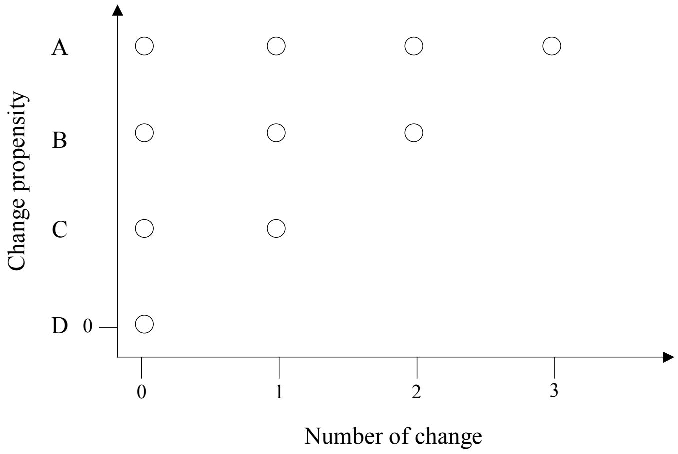

Figure 3: Four firms with different change propensities and their cumulative change in a given observation period

This bias occurs, if one is not able to control for the inherent change propensity. Again, a fixed-effect regression can solve the problem, because it controls for the unobserved change propensity. In our stylized example a fixed-effect regression would yield the correct (i.e. zero) effect. One could use fixed-effect event history methods in continuous time (Yamaguchi 1985). It is easier, however, to switch to discrete time methods. Then it is possible to use the fixed-effect logit regression model:

The left side of this formula denotes the probability that firm i shows a change at time t. X_{it} is

$$P\left(Y_{it}=1\right)=\frac{\exp\left(\beta X_{it}+\alpha_i\right)}{1+\exp\left(\beta X_{it}+\alpha_i\right)}.$$

the number of changes the firm has experienced up to time t. α_i is the firm specific error component. It controls for the firm specific change propensity. Chamberlain (1984) showed that it is possible to get consistent estimates of the ß-coefficient without actually estimating α_i (conditional likelihood approach, again one has to assume that α_i is fixed).

3. Four empirical studies

In order to investigate whether these arguments are of relevance in "real" settings, we want to apply fixed-effect estimation procedures to four different data sets: A data set which covers the life course of German start-up firms in different industries over a period of six years, a data set on Vienna newspapers between 1918 and 1938, and two data sets which contain the rule histories of a German bank and a large international manufacturing company. With the first data set we investigate to what extent organizational change affects success. With the other three data sets we investigate the momentum-hypothesis.

3.1 The Munich Founder Study

The data we use are part of the "Munich Founder Study" (for details, see Brüderl et al. 1992, 1996). From a total of about 28,000 business registrations in 1985/86 in Munich and Upper Bavaria, we drew a random sample of about 6,000 business addresses. Because the population was confined to businesses administered by the Chamber of Commerce, crafts, agro-businesses, physicians, architects, and lawyers are not part of the sample. Of the 6,000 sample addresses 600 could not be updated. In addition, not all business registrations were real businesses; about 20% were only established to obtain tax advantages. Such firms were not interviewed. Finally, interviews with 1,849 business founders could be realized in spring 1990. The question program was broad and required an average interview time of nearly one hour. The first part of the interview concerned start-up characteristics of the firm and its development over time, the second part dealt with the individual attributes and activities of the founder.

Of the 1,849 founders successfully interviewed 146 had to be eliminated, because the founders declared that their firm started before 1985 or after 1986 (N=92), showed no economic activity during the observation period (N=47), or reported zero employees at some time point and therefore no growth rate could be computed (N=7). This leaves 1,703 firms.

We will use these data to investigate to what extent change events affect the annual employment growth rate. The founders were asked to report the average number of employees for every year the firm was in existence. Therefore, we have at most six employment figures (for firms that were founded in 1985 and still in existence in 1990). The number of employees includes the founder. Part time employees were added only as a fraction of one. Note that also the founder possibly works not full time in the new firm. Therefore, employment figures might be below one. The retrospective employment time series is the basis for our analyses (for 25 employment time series some values had to be interpolated to complete them). The number of employees for the first year (S_{i1}) ranges from 0.1 to 205 with a mean of 2.2. 96% of the newly founded firms have less than 10 employees, which demonstrates that the firms in this sample are mostly very small. From these employment figures we calculate annual employment growth rates: $g_{it} = \ln(S_{it}) - \ln(S_{it-1})$. Therefore, we loose firms with only one employment observation. Further, the fixed-effect panel model requires at least two growth observations for each firm. Thus, we can use only firms with at least three employment observations. This leaves 1,380 firms with 5,701 growth observations (these numbers are somewhat lower in the models due to missing values on the change variables). Growth rates in this sample range from -2.3 (employment decreased from 1 to 0.1) to 3.9 (employment increased from 1 to 50) with a mean of 0.044. 4% of the growth rates are negative, 82% zero, and only 14% positive.

The questionnaire included questions on whether there were changes, and if yes, in what year they happened. We can distinguish five change events (proportion of the sample that experienced the change event): space enlargement (17%), location change (15%), equity increase (14%), change of legal form (5%), and CEO-replacement (6%). Only the first incidence of change of a given type is recorded in the data. Theoretically, the first four change events can be classified as peripheral change. According to the Hannan/Freeman argument we would therefore expect a zero effect on growth. CEO-replacement is usually classified as core change, which should accordingly show a negative effect on growth.

We estimate models with the following specification:

$$g_{it} = \beta x_{it-1} + \delta Age_{it-1} + \alpha_i + \varepsilon_{it}.$$

X_{it} is a change dummy that is zero the years before a change, one thereafter. We do not include a change-clock, because our observation period is too short. We control for age, because the literature has shown that growth declines with age (Evans 1987, Dunne et al. 1989). The coefficients give the growth change when the covariate increases by one unit.

3.2 Rule systems of a German bank and an international manufacturing company

In the following two data sets not firms but rules are the units of analysis. Data were collected from only one organization and not from various organizations. Individual rules can be regarded as the most basic structural elements of organizations and every organization is built on sets of rules and routines, which develop around rules (Kieser and Kubicek 1992, Kieser et al. 2001). Changing these elements is equivalent to internal organizational change on the very basic level of an organization.

Although, investigating rule histories in order to analyze organizational change seems to be quite a rare strategy, there have been an increasing number of publications, which deal with changes of formal organizational rules in recent years (e.g. Miner 1991, Zhou 1993, Schulz 1998a, 1998b, March et al. 2000). The first data set consists of the personnel rules of a German bank (Beck 2001) and the second consists of the quality management rules of a large international manufacturing company (Beck 2002).

The rule system of the German bank still exists, however in a totally revised form. It was introduced at the end of 1970. Several “rule administrators” from different departments and hierarchical levels are responsible for introducing and revising rules. Almost any member of the organization can make proposals for changing existing or creating new rules. The initiator of a rule change has to get in contact with the rule administrator who is responsible for the respective rule area. Having consulted with managers who work in the area for which the respective rule applies the rule administrator decides, whether the proposed changes should be implemented into the rule system. Often the rule administrators themselves initiate rules changes. Thus, the process of rule changes is fairly decentralized, but all departments are informed about rule changes on a weekly basis. This is done by sending those pages of the rulebook that contain new or changed rules to the departments.

In November 1989, the rulebook underwent a significant redesign, whose main purpose was to reduce its volume and to increase its manageability. Rules were merged, reordered or formulated in a more concise manner or abolished altogether. Because this redesign marked an obvious discontinuity that triggered unusual change activities long before the “official” redesign, our observation period ends with the end of 1988. We could gather data on the history of 246 bank rules that were changed 655 times.

The quality management rule system of the manufacturing company was introduced in January 1992. We could observe its development until August 2000. The data set consists of 79 rules, which have been changed 99 times.

3.3 Vienna Newspapers

Our fourth data set contains information on Viennese newspapers that appeared between 1918 and 1938. Our primary data source is the archives at the Österreichische Akademie der Wissenschaften in Vienna. Archivists there examined all known newspaper issues accessible to them in various archives and private collections. For each newspaper, the archivists recorded the exact dates of first and last appearance, and distinguished between ending events according to whether the newspaper was suppressed by the government or whether it simply ceased publication (see Melischek and Seethaler 1992). Each newspaper's political party affiliation, exact title, subtitle, layout, price, periodicity, ownership structure, editor, and some other information, which we don't use in this article, was also noted. The data were collected on a daily basis and each change in one of the aforementioned categories was noted together with the exact date.

The archivists also examined secondary sources, including newspaper address books, newspaper catalogues (Annoncenkataloge), various newspaper yearbooks, and newspaper bibliographies and from these recorded the ideological tendencies and circulations of the newspapers on an annual basis. The archivists found thirteen different sources, each recording self-reports of each newspaper's circulation and ideology – information originally collected during the time of the study by annual questionnaires. The full data set covered 140 organizations over 702 organization-years (Barnett and Woywode 2001).

We analyze two change events: change of the weekly number of issues (frequency change) and change of the newspaper printer. The number of organizations for which information on frequency change was available is higher than the number of organizations for which information on printer change was available. Thus, the data set on frequency change contained information of 125 organizations with 61 change events; the data set on printer change contained 114 organizations with 90 change events.

Both rule data sets and the Vienna newspaper data sets were split into monthly sub-episodes. When a change happened in a sub-episode, it was coded 1, otherwise 0. Episode splitting resulted in a data set of 23,877 sub-episodes of the bank rule data, 6,954 sub-episodes of the manufacturer rule data, 5,518 sub-episodes of the frequency change data, and 6,427 sub-episodes of the printer change data. Since some organizations in the latter data set could be observed for a longer time span than in the frequency change data set the number of sub-episodes is higher. These are multi-episode event history data in discrete time, which we analyze with logit models without and with fixed-effects. The fixed-effect logit requires at least two observations per case. Therefore, units that never experienced a change must be excluded. Since this exclusion might affect the results, we estimate the models without fixed-effects with both the complete and the reduced data sets.[2]

Our model specification is as follows:

$$P\left(Y_{it} = 1 \right) = \frac{\exp\left(\beta x_{it-1} + \gamma d_{it} + \delta Age_{it} + \alpha_i \right)}{1 + \exp\left(\beta x_{it-1} + \gamma d_{it} + \delta Age_{it} + \alpha_i \right)}.$$

[2] Our stylized example from Figure 3 tells us about the direction of this change. A standard logit where firm D is excluded from the data still would provide a positive effect of prior change. But the positive effect will be lower, because the risk set is the same for zero and one changes.

The left side of this formula denotes the probability that rule/firm i shows a change at age t. X_{it} is the number of changes the rule/firm has experienced up to age t. d_{it} denotes "time since last change", i.e. the time-clock. We use a logarithmic transformation of prior changes because some studies (e.g. Schulz 1998b) reveal that the positive effect of prior changes is not linear. According to Amburgey et al. (1993) and a couple of other studies we include "time since last change" and "age of rule/firm" as independent variables. Both covariates are considered to exert negative influences on the probability of change since inertia should increase with them (see Delacroix and Swaminathan 1991, Ginsberg and Baum 1994). In the frequency change model of Vienna newspapers we also include "number of issues per week" as an independent variable. The coefficients give the logit increase in the probability of a further change when the covariate increases by one unit.

4. Results

4.1 The Effect of Organizational Change on Growth Rates

Using the data set on German start up firms we investigate the effect of organizational change on performance, i.e. employment growth rates. We estimate standard OLS regressions and fixed-effect regressions (using the STATA-procedure xtreg). Results are displayed in Table 1. Age effects are negative – as expected – in all models. From the OLS regression one would infer that the first four change events significantly increase the growth rate, i.e. these changes increase performance! One would conclude that the Hannan/Freeman argument is completely wrong (disregarding that these are peripheral changes). But these estimates are heavily biased as the fixed-effect results show. When we control the unobserved firm-specific performance level, we see that three changes have a negative (though not significant) effect on growth. Legal form change has a zero effect. These dramatic changes in the effects are completely understandable, if one looks back at Figure 2. All four change events are success induced, as results of regressions of the growth rate on the respective change rate show (see Brüderl et al. 1996: 263). With higher growth rates the probability that these changes occur increases. Therefore, as we argued with Figure 2 the OLS estimates are biased upwards. They become positive though the "true" effects are zero. From the fixed-effect models we can conclude that the four peripheral change events (location change, space enlargement, equity increase, legal form change) have no (significant) effect on performance, as one would expect.

Insert Table 1 about here

The situation differs with the core change event (CEO-replacement). Here the OLS estimate is zero, the fixed-effect estimate is strongly negative. Only the fixed-effect estimate shows that a CEO-replacement decreases performance (at least in the short-term) as the Hannan/Freeman argument predicts. A regression of the growth rate on the CEO-replacement rate shows (see Brüderl et al. 1996: 263) that CEO-replacement is induced by low performance. Thus, the OLS result is not in line with what we would expect from Figure 1 (a downward bias). This shows that the direction of the bias in real data sets is unclear. This even strengthens our argument that one should use the fixed-effect model.[3]

Similar biases seem to operate when one uses the more conventional performance measure "survival". Mortality rate regressions show (see Brüderl et al. 1996: 265) that all five change events increase performance, i.e. decrease the mortality rate. From these results one would

[3] A stylized example for this situation could be: We have three firms (low, medium, and high performer). The low performer has a change at t_8, the high performer at t_6. Both changes have a negative effect on performance. Then the standard model yields a spurious zero effect, the fixed-effect model the correct negative effect.

conclude that change increases performance! However, similar arguments as with the growth models apply here. These results are highly likely to be biased, because standard rate regressions do not control for unobserved heterogeneity.

These results allow us to draw a more general conclusion regarding the “true” effects of core versus periphery changes. Core changes like CEO-replacement, which imply frequently strong disruption of existing organizational processes, affect performance indeed negatively. Instead, periphery changes like location change, space enlargement, equity increase, or legal form change seem to be caused by prior success but don’t have significant performance consequences. Apparently, unobserved heterogeneity has caused the bias of the results obtained with standard models. Using the fixed-effect model we control for unobserved heterogeneity and obtain significantly different results.

4.2 The Effect of Prior Change on Further Change

We will now investigate the momentum-hypothesis. Estimation results are presented in Table 2 (using the STATA-procedure clogit). First, we want to look at the effect of prior changes in the standard logit models – i.e. the models that resemble traditional specifications used in the literature. All four models display a positive influence of prior changes, which is significant for bank rule changes and frequency changes. When estimating logit models with the reduced data sets the positive effects of prior changes decrease – as expected. In fact, the coefficient in the manufacturer rule change model even turns significantly negative. Thus, the positive influence of prior changes in the regular logit models is at least partly due to the presence of totally inert units.

Insert Table 2 about here

When fixed-effects are included the coefficients for prior change become significantly negative in all models. Hence, what we see here is a dramatic change in the coefficients that puts into question results from many studies that have found a positive effect regarding the repetitive momentum-hypothesis. In fact, these results give leverage to a refinement hypothesis: The more often a rule or an organization has experienced a change of a certain kind, the lower the need for further change because the fit of the organizational structures has improved with every change.

The standard logit models display a significantly negative effect of time since last change (the change-clock) in the newspaper models but a positive effect of this variable in the rule change models. When excluding the units, which never change, the effect decreases in all four models. In three of the four fixed-effect models time since last change has a significant negative effect, supporting the inertia thesis. Only in the fixed-effect model of manufacturer rules we find a zero effect. However, the inertia thesis, which is also connected to the age effect, is put into question when looking at the results from the fixed-effect regressions. In all four models the age effect is positive and significant at the 5%-level. Finally, we find that an increasing number of newspaper issues increase the likelihood of changing the number of issues in the next period.

5. Discussion

Concerning the effect of change on performance, we found that peripheral change has a zero effect and core change lowers performance. This is in accordance with the Hannan/Freeman argument. However, this result was only obtained with a fixed-effect approach. A standard modeling approach yielded very different results. This is due to the fact that the standard approach does not take into account the endogeneity of change: some change events are induced by success, others by failure. In fact, we maintain that a standard approach mixes the causes and consequences of change. We hope that we were able to demonstrate that fixed-effect models can solve this problem.

The models with change as a dependent variable revealed that a traditional strategy of modeling organizational change leads to a replication of previous findings regarding prior changes: a positive effect on the rate of further change. On the basis of such results researchers have inferred a repetitive-momentum of organizational change. However, when we analyze the probability of organizational change using a fixed-effect model we find that the effect of previous change on the change rate is the opposite. This result questions findings from many previous studies, which have not controlled for unobserved heterogeneity. It suggests that cumulative organizational change leads to a refinement of organizations. Organizations learn how to design organizational elements better while repeatedly changing them. With every change, reactions and experiences of organizational members as well as knowledge of the reactions of the organizational environment are incorporated. Thus, the internal and external fit of organizational elements improves with an increasing number of changes. In the long run, this makes organizational elements more stable. This does not contradict a dynamic view of organizations since new organizational elements can be incorporated which subsequently are also subject to refinement.

Certainly, the fixed-effect approach cannot cure all problems, when analyzing change. It is only applicable when one has multi-episode data. For the single-episode case one could use simultaneous rate models (Lillard and Panis 2000). Whether this works, we will investigate in future work. Finally, when observation periods are long (as is typical with population studies) the fixed-effect assumption becomes questionable. For this situation Greve (1999) has proposed an alternative estimation approach. He calculated a selection model where he estimated the probability of organizational change and included this probability of change in models of performance. This strategy works only if one has enough information on the units of analysis to specify an adequate selection equation. It may also be a promising estimation strategy if the performance levels of the individual organizations are varying significantly over time. We think that our fixed-effect estimation approach is useful in situations, when observation periods are short and performance levels of organizations are rather stable.

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Table 1: Effects of organizational change events on annual employment growth

	Location change		Space enlargement		Equity increase		Legal form change		CEO-replacement	
	OLS	Fixed effect	OLS	Fixed effect	OLS	Fixed effect	OLS	Fixed effect	OLS	Fixed effect
Change event	0.051*	-0.024	0.102*	-0.019	0.083*	-0.010	0.056*	0.000	-0.002	-0.107*
(dummy)	(5.07)	(1.04)	(11.04)	(1.01)	(8.26)	(0.45)	(4.00)	(0.00)	(0.13)	(3.46)
Firm age in	-0.025*	-0.027*	-0.029*	-0.027*	-0.026*	-0.027*	-0.024*	-0.028*	-0.023*	-0.026*
years	(8.83)	(9.22)	(10.44)	(9.10)	(9.15)	(9.14)	(8.51)	(9.84)	(8.16)	(9.07)
Constant	0.141		0.147		0.141		0.142		0.143	
R^2	0.02	0.31	0.03	0.32	0.02	0.31	0.01	0.31	0.01	0.31
# of spells	5,655		5,628		5,589		5,646		5,641	
# of firms	1,369		1,362		1,353		1,366		1,365	
# of changes	???		???		???		???		???	

* $p < 0.05$; |T-values| in parentheses.
Source: Munich Founder Study, 1990; own computations.

Table 2: Effects of prior change on further organizational change

	Bank			Manufacturer			Vienna Newspapers					
	Rule change			Rule change			Frequency change			Printer change		
	Logit	Logit with reduced data set	Fixed effect logit	Logit	Logit with reduced data set	Fixed effect logit	Logit	Logit with reduced data set	Fixed effect logit	Logit	Logit with reduced data set	Fixed effect logit
Ln (# of previous changes)	0.808* (18.49)	0.635* (13.86)	-4.709* (5.03)	0.067 (0.73)	-0.300* (2.95)	-1.192* (7.67)	0.457* (4.27)	0.081 (0.74)	-1.077* (6.15)	0.136 (1.37)	-0.042 (0.43)	-0.845* (6.04)
Time since last change in years	0.091* (3.23)	-0.022 (0.72)	-0.080* (2.08)	0.210* (2.32)	0.086 (0.92)	0.051 (0.43)	-0.334* (2.12)	-0.355* (2.21)	-0.630* (3.87)	-0.240* (3.30)	-0.244* (3.33)	-0.315* (4.17)
Age in years	-0.131* (8.45)	-0.113* (7.24)	0.070* (3.09)	-0.164* (3.27)	0.002 (0.04)	0.369* (4.39)	0.105* (3.07)	-0.102* (2.98)	0.398* (4.59)	-0.025 (0.69)	-0.031 (0.85)	0.298* (5.17)
# of issues per week							0.069 (1.33)	0.021 (0.41)	0.259* (2.94)			
Constant	-1.665	-1.645		-3.656	-4.122		-3.650	-3.078		-3.588	-3.458	
# of spells	23,877	17,771		6,954	4,853		5,518	2,877		6,427	5,420	
# of rules/firms	246	143		79	47		125	38		114	45	
# of changes	655	655		99	99		61	61		90	90	

* $p < 0.05$; |T-values| in parentheses.
Source: Bank rule data 1970-1988, Manufacturer rule data 1992-2000, Vienna newspapers 1918-1938; own computations.