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Did the Hartz Reforms Speed-Up Job Creation?
A Macro-Evaluation Using Empirical Matching Functions

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ABSTRACT

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Starting in January 2003, Germany implemented the first two so-called Hartz reforms, followed by the third and fourth packages of Hartz reforms in January 2004 and January 2005, respectively. The aim of these reforms was to accelerate labor market flows and reduce unemployment duration. Without attempting to evaluate the specific components of these Hartz reforms, this paper provides a first attempt to evaluate the overall effectiveness of the first two reform waves, Hartz I/II and III, in speeding up the matching process between unemployed and vacant jobs. The analysis is conceptually rooted in the flow-based view underlying the reforms, estimating the structural features of the matching process. The results indicate that the reforms indeed had an impact in making the labor market more dynamic and accelerating the matching process.

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1 Introduction

This paper provides a first attempt to evaluate the overall effectiveness of the largest labor market reform in Germany in the post-war period in terms of speeding up the matching process between unemployed and vacant jobs. In spring 2002, the German federal government under chancellor Gerhard Schröder issued a request for a commission consisting of politicians and business professionals to come forward with suggestions for policy reforms that would lead to full employment. In its report, the commission led by Peter Hartz, at the time personnel manager at Volkswagen, emphasized unemployment as the paramount problem for society. To overcome this problem the commission proposed a program of 13 modules, see Hartz *et al.* (2002). All of the modules proposed by the Hartz-commission share the view that policy intervention should provide both assistance and incentives for successful integration in the labor market (the principle "assist and demand", "*Fördern und Fordern*"). The Hartz-suggestions mark a paradigm shift in German labor market policy in the sense that they are based on a dynamic, flow-based view of the labor market. The aim of the reforms was to accelerate labor market flows and reduce unemployment duration, and thereby reduce the number of people detached from the labor market.

In the meantime, some of these modules have been implemented in form of four "Laws for a modern provision of services on the labor market" (*Gesetze für moderne Dienstleistungen am Arbeitsmarkt*), the so-called "Hartz-Laws" (Hartz I-IV). The first two of these laws became effective on January 1, 2003. Hartz I was mainly concerned with implementing occupational training programs, subsistence payments on behalf of the employment agency, and the facilitation of new forms of employment for elderly or temporary employment. Hartz II introduced the so-called mini- and midi-jobs, low-paid or part-time employment that are (partly) exempt from taxation and social security contributions or have different rules applying than regular jobs, special programs for self-employment, and the implementation of so-called Job-Centers, agencies to improve the matching between unemployed and firms with vacancies. Hartz III followed on January 1, 2004, implement-

ing a reorganization of the federal employment agency and its local employment offices. The most debated package of laws was Hartz IV, which became effective on January 1, 2005 and modified the rules for entitlement to unemployment assistance and social assistance as well as the administrative responsibilities. This reform also changed the rules for eligibility to unemployment benefits, and consequently the definition of the status of being unemployed. While the public debate about the pros and cons of the Hartz reforms is still ongoing, first attempts are being made to scientifically evaluate the effectiveness of the reforms. For a detailed description of the background of the reforms as well as first evaluation results see Jacobi and Kluve (2006).[1] The results indicate by and large that the effectiveness of occupational training programs seems to have improved as consequence of the Hartz I reform (see Bonin and Schneider, 2006, and Jacobi and Kluve, 2006), whereas other reform packages like the "mini jobs" show no significant employment effects (see Caliendo and Wrolich, 2006).

Instead of evaluating any of the specific components of the Hartz reforms, this paper takes an entirely different, complementary approach and provides a first look at the overall effectiveness of the reforms. The analysis is motivated by the primary aim of the reforms, the acceleration of the speed of matching between unemployed and vacant jobs, and therefore analyzes the reforms from a macroeconomic perspective. To do that, we apply the workhorse of modern macroeconomic labor economics, the matching function. In particular, we estimate the structure of the matching technology and the changes that occurred in the aftermath of the implementation of the Hartz reforms. The matching function describes a functional relationship between the inflow into new jobs and its determinants, in particular the available stocks of job searchers and vacancies and the inflows to these stocks, in a parsimonious but nevertheless empirically relevant way, see the survey by Petrongolo and Pissarides (2001). Given the Hartz-commission's emphasis of improving the matching between unemployed and worker-searching firms and the goal to reduce un-

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[1] An evaluation of the Hartz reforms commissioned by the Federal Labor Ministry is coordinated by Bruno Kaltenborn, see also `http://www.wipol.de/hartz/evaluierung.htm` for the preliminary reports. One report for the evaluation commission addresses macroeconomic aspects of the Hartz reform, but focuses on the effectiveness of expenditures in particular active labor market programs, see Fertig *et al.* (2005).

employment duration, the matching framework with its focus on frictional unemployment seems the natural starting point for a macroeconomic evaluation.

To our knowledge, there are only two studies that evaluate the macroeconomic effectiveness of active labor market policies in Germany, namely the studies by Fertig, Schmidt, and Schneider (2006) and by Hujer *et al.* (2006). However, in contrast to our approach, none of these studies applies a methodology based on the matching framework. Moreover, these studies refer to the effectiveness of active labor market policy *before* the implementation of the Hartz reforms using regional data. For example, Fertig, Schmidt, and Schneider (2006) use regional data on basis of employment agency districts for the years 1998 to 2000 to evaluate the effectiveness of expenditures for different particular policy measures on gross and net labor market flows.

In our analysis we use panel data for 40 occupational groups over the period March 2000 until December 2004 with a monthly frequency. While the data used in this study allow for a first evaluation of the effects of the Hartz laws both on the aggregate level as well as on the level of occupational groups, our analysis is the first estimation of the matching technology for Germany using data with monthly frequency. Empirical matching functions have been estimated for Germany before, see e.g. Gross (1997), Entorf (1998), Fahr and Sunde (2004, 2005, 2006a, 2006b). However, all these estimates for Germany have used data on a quarterly or even annual basis, and the most recent estimates date back to the late 1990s. The high data frequency used in this study helps to circumvent some technical problems with the estimation of matching functions encountered by the previous contributions. In particular, data with high frequency avoid time aggregation problems that lead to downward-biased coefficient estimates.

From a more technical point of view, this paper makes several contributions to the empirical matching literature. Besides being the first paper that uses German data on a monthly basis to estimate a empirical matching functions, we present the first estimates of the matching function following the stock-flow approach for German data. The stock-flow approach emphasizes the relevance not only of the pools of unemployed and vacancies

at the beginning of a observation period for job creation, but also of the inflows into these pools during the observation period (see Coles and Smith, 1998, Petrongolo and Pissarides, 2001, Coles and Petrongolo, 2002, and Gregg and Petrongolo, 2005). While delivering different estimates of the elasticities of the matching process with respect to unemployed and vacancies, we find that the results of central interest, namely the changes in the speed of matching as consequence of the Hartz reforms, are fairly robust to the underlying parametrization of the matching function.

According to our estimates, the implementation of the Hartz I and II reforms on January 1, 2003, had significant positive effects on the speed of the matching process between unemployed and vacancies on German labor markets. This finding is robust to corrections for autocorrelated error structures, or time aggregation, and cannot be explained by business cycle effects. The findings also show that the positive effect was slightly delayed after the implementation and favored manufacturing and crafts occupations. The Hartz III reforms appear to have had an even stronger effect on the speed of matching. Contrary to the previous reform wave, however, this effect is strongest right at the beginning of the implementation period. Again, manufacturing occupations appear to have benefitted most from the reform that implied changes in the intermediation process through employment agencies. Overall, the results point at a strong and significant macroeconomic impact of the first two waves of Hartz reforms.

The remainder of the paper proceeds as follows. Section 2 presents the econometric framework, the specifications we estimate and the identification assumptions. In section 3 we describe our data sources and the sample, and in section 4 we discuss data limitations that affect our identification strategy. The central results of our study are presented in section 5. Section 6 concludes.

2   Econometric Framework and Specification

The matching function is the center piece of most macroeconomic models of frictional unemployment. It reflects the notion that matches between unemployed workers looking for

a job and firms looking for adequate applicants to fill their vacancies do not arise instantaneously, but involve a lengthy and costly process of search on both sides. Rather than capturing the structure of this process or the information or trading frictions underlying the matching problem, the matching function represents a reduced form of the matching process. The conventional approach, going back to Blanchard and Diamond (1989), models the matching function as a Cobb-Douglas with the stocks of unemployed and vacancies as the inputs. This reduced form can be micro-founded, however (see e.g. Petrongolo and Pissarides, 2001). This specification, in which the flow of matches is regressed on both stocks of unemployed and vacancies, and which therefore has been referred to as the stock-stock approach, has been repeatedly criticized (see e.g. Coles and Smith, 1998, Petrongolo and Pissarides, 2001, and Gregg and Petrongolo, 2005). The reason is that the stock-stock approach implicitly assumes an undirected, random underlying search process leading to matches between homogeneous unemployed and homogenous vacancies. However, workers and jobs are differentiated, it seems more natural to assume that unemployed and firms with a vacancy first sample the pool of available potential trading partners. The mutually beneficial exchange of goods or services is conducted if a suitable match is encountered. If this is not the case, however, there is no need to sample the entire stock again, but rather only the inflow of new potential trading partners into this stock. This is the idea behind the so-called stock-flow approach to matching. According to this approach unemployed or vacancies that have not found a suitable match right after entering the stock and starting to search should only match with new inflows of vacancies or unemployed, respectively. Coles and Smith (1998) were the first to derive and estimate a reduced form specification of the matching function that takes this idea into account.

We follow their approach and estimate a stock-flow model of the flow of new hires m_{it} in a given occupation i between date t and $t+1$ as

$$\ln m_{it} = A_{it} + \alpha_1 \ln U_{it} + \beta_1 \ln V_{it} + \alpha_2 \ln u_{it} + \beta_2 \ln v_{it} + \varepsilon_{it} \,, \tag{1}$$

where U_t is the stock of unemployed at time t, u_t is the inflow of new unemployed between t and $t+1$, and likewise V_t is the stock of vacancies at t and v_t is the inflow of vacancies

between t and $t+1$, respectively, and where ε_{it} is an error term discussed below. The term A_t reflects the efficiency of the matching process, and is affected both by the speed of the matching process as well as the probability that a match leads to an employment relationship (see Gorter and van Ours, 1994, for a more detailed analysis). In the empirical specification, we use different empirical concepts of new hires. Our baseline measure is unemployment outflows into employment, but we also utilize finer concepts of unemployment outflows into employment that originate through the initiative of the unemployed, as well as through placement by the employment office. This choice is mainly dictated for lack of better data. More appropriate measures of matches such as employment inflows generated on basis of individual register data used by Fahr and Sunde (2004, 2005) are currently not available for the period after 2002. Nevertheless, we believe that the data reflect the goals of the Hartz reforms, namely reducing unemployment by spurring outflows from unemployment, quite well. Moreover, most of the international literature on empirical matching functions uses a courser measure of unemployment outflows as primary dependent variable (see e.g. Broersma and van Ours, 1999, and Petrongolo and Pissarides, 2001).

A feature that has to be taken into account to obtain consistent results with the stock-flow model (1) is a mechanical relationship leading to serial correlation in the disturbance term ε_{it}. As was noted by Gregg and Petrongolo (2005), inflows into and outflows from unemployment are linked through the identity

$$U_{it} = U_{it-1} + u_{it-1} - m_{it-1} \,. \tag{2}$$

An analogous condition holds for vacancies. Substituting m_{it-1} by the respective expression given by (1), it becomes clear that the explanatory variables in either model are correlated with past disturbances. If one assumes that the error component ε_{it} follows an AR(1) process,

$$\varepsilon_{it} = \rho \varepsilon_{it-1} + \zeta_{it} \,, \tag{3}$$

where $\rho < 1$ and $\zeta \sim N(0, \sigma_\zeta^2)$, then the explanatory variables in a model that disregards this autocorrelation can be expected to be correlated with the error term. As a conse-

quence, any estimation results obtained with specifications that neglect this issue would be inconsistent. In order to examine the relevance of this potential flaw for our results, we estimate a version of the stock-flow model that allows for serially correlated AR(1) disturbances.

The Hartz reforms may have affected the job creation process in different ways. A first indication whether the Hartz-reforms had any desired effect in terms of facilitating and improving the matching between unemployed and firms, can be obtained from an investigation of variation in the matching efficiency parameter A in either of the models in response to the implementation of the Hartz laws. In the simplest specification, which is also the most frequently used one in the literature, the matching efficiency term is specified as being affected by factors that reflect the overall business climate and business expectations, factors that determine differences across subsets of the labor market, like occupations, and time effects that reflect other changes in the environment than the business cycle. In our analysis, we specify A_{it} as $A_{it} = a + Z_t + \eta_i + \delta_0 H_t$ where Z_t controls for business cycle effects using an indicator that is discussed in the data section. By adding a full set of 40 occupation dummies η_i we take occupation specific differences in the speed of matching into account.

The impact of Hartz reforms is then estimated by adding a treatment dummy that indicates whether the particular package of Hartz reforms one is interested in was in place or not, i.e. a Hartz-Dummy H_t with $H_t = 0$ for all months prior to the implementation month and $H_t = 1$ for all months following the implementation. In a more flexible specification, we additionally control for time effects by including a full set of month fixed effects and investigate the time pattern of the month fixed effects.[2] To get additional identification, we also use variation in the treatment effects across occupations that might vary in terms of the intensity in which they use the services of employment agencies.

A more general test for the effects of the Hartz reforms is to check for structural breaks not only in the parameter reflecting the efficiency of the matching process, but also in the

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[2] In additional robustness checks, we examine whether the series of fixed effects exhibits a structural break in the month in which the particular Hartz reforms of interest were implemented.

technological parameters of the matching function, the α's and β's. We will report results of appropriate tests below. However, as is discussed in more detail in the following, the possibility to identify any effects of the policy changes is to a large extent dictated by data quality and data constraints that have to do with measurement and the availability of data for treatment and control. We therefore defer the discussion of identification issues to section 4 after the data description.

3   Data and Sample

The estimation of empirical matching functions in the stock-flow framework requires data on the pools of unemployed individuals and vacancies on a specific level of aggregation, as well as the corresponding inflows to these pools. The data used for the analysis are in principle available for the period December 1999 until January 2006 and are provided by the Federal Employment Agency (*Bundesagentur für Arbeit*, BA). As the particular series are collected between approximately the 16th of a given month and the 15th of the following month, we assign the flows to the latter month, i.e. to the month in which the respective number was publicly announced.[3] We use series on the stocks of unemployed by occupational group, the stocks of vacant positions by occupational group, and the respective inflows to unemployment and vacancies by occupational groups.[4] Our dependent variables are the respective outflows out of unemployment into employment by occupational group. All data are aggregated on the level of 40 occupational groups. The occupation classification we use is provided on basis of individuals' current or previous employment. We choose to use this rather coarse definition of occupations in order to minimize flows between occupations, and to maintain comparability with the results of previous estimations of matching functions using German data.[5] In our regressions we control for four broad groups of loosely related occupations. To control for business cycle

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[3] This implies that we assign e.g. the month "December 2005" to the data collected between November 18 and December 15, 2005, see also BA (2004).

[4] Data can be downloaded from the homepage of the federal employment agency: `www.arbeitsamt.de`.

[5] See also the discussions and results presented in Fahr and Sunde (2005) and Sunde (2006).

effects, we use an index of business expectations provided by the ifo-Institute in Munich.[6] In order to eliminate any spurious seasonal variation that might affect our results, we adjust each series by regressing it on a full set of month dummies and preserving the residuals.

4   Data Limitations and Consequences for Identification

Three major events fundamentally affect the coherence of our data over the observation period under consideration. Two events concern the measurement of explanatory variables in the estimated models, and another event affects the measurement of the dependent variable. As consequence, we observe breaks in the respective data series used for the analysis.[7]

The most serious data problem for the current analysis is a fundamental break in May 2003 in the statistics concerning the dependent variable, the outflow of unemployment into employment. Until April 2003 the Federal Employment Agency ascertained the placements by comparing changes in two independent statistics. Following the so called job centered approach in the labor market statistics a placement was counted once a registered vacancy was removed from the vacancy statistics and a job searcher notified a removal from the job searcher statistics within the same time period. This procedure was heavily criticized by the German Federal Court of Auditors in spring 2002. In reaction to this critique the Federal Employment Agency has begun to count placements according to the job placement statistics since May 2003 only if a job searcher enters a new employment as consequence of direct help by the employment office. Because detailed statistics are not available for the time before May 2003 it is not possible to adjust the placements statistics for the time

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[6] Time series are available at `http://www.cesifo-group.de`. We use the index R3 of business expectations, since job creation should be affected by expectations rather than actual business conditions. Robustness checks with alternative indices R1 and R2 reveal similar results, however.

[7] The information reported in this section follows two documents available at the homepage of the Federal Employment Agency (`www.arbeitsagentur.de`): `http://www.pub.arbeitsamt.de/hst/services/statistik/000100/html/interpretation/Uebergreifendes/03.Allg.St.Hinw.pdf` (1st September 2006) and `http://www.pub.arbeitsamt.de/hst/services/statistik/000100/html/interpretation/Ausgleichsprozesse/05.Vermittl.Statistik.pdf` (1st September 2006).

before May 2003 in order to obtain consistent data series. While series reporting outflows into employment by placements by the employment agency and by own search effort are available for the whole time period, it is unclear whether the measurement error leads to a systematic overestimation of placements and underestimation of employment relations brought about by search activities of the unemployed, or vice versa. It is also not possible to identify the destinations of outflows from unemployment (e.g. outflows into employment, outflows into self-employment) which are most affected by errors in the statistical procedure before May 2003.[8]

As a consequence we take outflows into employment as the relevant outflows in our empirical analysis, regardless of whether these were generated as consequence of an unemployed's own search efforts of through placement by the employment office. Because the explanatory variables are not affected by the measurement error in the dependent variable we account for the measurement error by a dummy variable indicating the period with the more precise measurement of the placements by the employment agency. While a comparison of outflows into employment distinguishing those matches generated by the initiative of the unemployed searcher from those generated by the initiative of the employment agency would be interesting we forbear from this analysis because it is unclear how the change in the measurement of the placement statistics affect the relative composition of the aforementioned flows.

Two breaks affect the series of explanatory variables in our analysis. In January 2004 the Federal Employment Agency changed the information technology used to generate the statistics for unemployed, job searchers and registered vacancies. Because the new technology is better suited to detect faulty insertions of unemployment incidences or vacancies, the respective series drop around 3 to 6 in percent in 2004 after the implementation of the new technology, compared to 2003. In order to provide consistent statistics for the time prior to the technological upgrade, the Federal Employment Agency recomputed the labor market statistics for the unemployed with the new system back to 1998, however.

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[8] This information has been provided by personal conversation with Erich Janka at the statistic service department of the Federal Employment Agency in August 2006.

The vacancies statistics are re-analyzed back to 2001. Thus, the renewal of information technology at the Federal Employment Agency affects the informational content of the data available for the analysis in the present paper only for the vacancy statistics in the very beginning of the observation window.

Finally, the implementation of the Hartz IV reform in January 2005 came along with a change of the definition of the unemployment status. The consequence was an increase in the number of unemployed which is not due to economic changes but only caused by the re-definition of the unemployment status. Thus, the intervention does not only lead to economic changes in the matching process, but also leads to a structural break in the measurement of some of the variables of central interest.

Because of these identification problems, we do not attempt to evaluate the Hartz IV reform package and concentrate attention to the Hartz I/II and Hartz III waves. A clean macro-evaluation of the effects of one of the largest labor market interventions in German history, the Hartz IV reform, appears not to be feasible on basis of the publicly available data. In our preferred specification for identifying effects of the Hartz I/II reforms, we use data from March 2000 until December 2003. In robustness checks, we also disregard the observations affected by the mismeasurement of placements before 2002. Instead, we concentrate on the time period January 2002 until December 2003 in our analysis, and control for the break in the measurement of transitions from unemployment to employment by using an indicator variable.[9] As control period, we use observations prior to the particular reform under consideration, as treatment period we use the months after implementation. The sample period for the Hartz III evaluation is March 2003 until December 2004.

To investigate the effectiveness of the Hartz reforms further, we also exploit variation across occupations that might differ in the intensity in which they use employment offices for generating new matches. We do this by interacting the respective reform indicator with indicators for broad occupation groups. In defining these broad occupation groups, we follow Fahr and Sunde (2005) and group our data into four broad occupational categories,

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[9] Note that there is no reason to assume that the change in measurement of unemployment outflows affected the measurement of unemployment and vacancy stocks and inflows. Moreover, there is no indication that the change in the data definition had asymmetric effects on different occupations.

each comprising ten occupations: occupations in the agricultural and manufacturing sector (group 1); crafts occupations (group 2); white collar and high skill occupations (group 3); and service sector and low skill occupations (group 4). Table 8 in the appendix lists all occupations and the allocation of the occupations into the four broad groups. Transitions from unemployment to employment in broad occupation groups 1 and 2 are relatively more likely to involve activity of an employment agency than occupation groups 3 and 4.[10]

5   The Overall Effectiveness of Hartz Reforms on Unemployment Dynamics

5.1   Main Results: Hartz I and II

Our first set of main results is displayed in Table 1. All specifications allow for autocorrelation in the error components. The null of no autocorrelation in the errors can be rejected at any level. Columns (1) and (2) present the benchmark estimation results for empirical matching functions following the stock-flow approach, without and with month fixed effects, respectively. The first thing to note is that the estimates of the matching elasticities with respect to the stock of unemployed are positive and significant. The estimates are very similar in the two specifications of the matching efficiency term A_{it}. With an unemployment elasticity of around 1.1 the estimates are similar to those usually found with high frequency data in the international literature for unemployment outflows as dependent variable. In contrast, the stock of vacancies has only a weak and quantitatively small effect on unemployment outflows in the stock-flow model, and the point estimate is smaller than usually reported in the literature, (see Broersma and van Ours, 1999, Table 1).[11] The inflows into vacancies exhibit significant positive effects on the measure of

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[10] This statement is based on the number of transitions from unemployment to employment involving employment agency intervention, relative to the number of unemployed in the respective broad occupation. This number is 20 to 75 percent higher for broad occupation groups 1 and 2 compared to groups 3 and 4.

[11] The results obtained by regressing unemployment outflows on stocks of vacancies and unemployed have been criticized repeatedly on grounds of compatibility of stocks and flows, and competition among

matches, whereas the effect of the inflows into unemployment affect matches significantly negatively. This provides strong evidence for the relevance of taking inflows into account, in particular when analyzing high-frequency data such as the monthly data in our data set. These results also provide some empirical indication for increasing returns to scale in the matching process. The hypothesis of constant returns to scale can be rejected at conventional levels. Given our high-frequency data, these findings should not be strongly biased by time aggregation.[12] A better business climate seems to affect the speed of the matching process in column (1), but not in specification (2). In terms of overall fit of the model, the results indicate that most cross-sectional variation is captured by the occupation fixed effects that are added in each estimation. Month fixed effects add moderately to the explanation of within occupation variation.

The first indication for the effect of the Hartz policy reforms can be gained from the indicator variable of the post-Hartz I/II implementation period. The indicator takes the value 1 starting with the first full month of data during the implementation period. In our data, this corresponds to the report month February 2003, i.e. for the data collected between mid-January 2003 and mid-February 2003. According to the estimation results there is a positive and significant effect of the first reform wave on the speed of unemployment outflows into employment. The results are robust to the inclusion of month dummies, and even become stronger. The reforms accelerated the outflows from unemployment to employment by 5 to 10 percent, which corresponds to a reduction in the average duration of unemployment spells of about the same magnitude. Thus, the results indicate that the first wave of reforms was indeed effective.[13]

When using variation across occupations in addition to identify effects of the reforms,

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different pools of searchers and vacancies (see Broersma and van Ours, 1999, Mumford and Smith, 1999, Anderson and Burgess, 2000, and Sunde, 2006). In light of the different focus of this paper, and the fact that our main results do not seem to be driven by a particular specification of the matching function, we think that our main results are likely to be robust to this type of criticism. Without better data, however, there is not much more we can do at this stage.

[12] See Gregg and Petrongolo (2005) and Petrongolo and Pissarides (2006) for discussions of the issues of scale and time aggregation.

[13] Estimations of the more standard stock-stock specification of the matching function that neglects inflows into unemployment and vacancy stocks as explanatory variables deliver qualitatively identical results. These results are available upon request.

we find a very similar picture. The results are displayed in columns (3) and (4) of Table 1. The results for the coefficients of the matching technology are virtually the same as in the specifications without occupation-reform interactions. Most interestingly, the reform indicators are significant and positive, and the coefficients for the main effect are considerably larger than those found in the baseline model.

The interactions with broad occupations suggest that intermediation and placement intensive occupations have gained more from the reforms. This can be seen from the significant negative interactions with the indicators for broad occupations 2, 3 and 4 as compared to the reference occupation 1. Remember that these manufacturing occupations are relatively intensive in using employment agencies for generating job matches. These occupations seem to have benefitted relatively most from the reform implementation. Crafts (broad occupation group 2) was affected moderately less, but high skill and low skill occupations (groups 3 and 4) have benefitted significantly less from the reform than manufacturing occupations. The lower panel summarizes the results for the different occupations separately, by adding up main effects and respective interactions. As can be easily seen, the effect of the reforms was positive overall.

These results may be affected by several technical problems. The specifications estimated so far implicitly assume that the effects of the Hartz reforms mainly affect the speed of matching, i.e. the efficiency of the matching process, rather than the entire structure of the matching technology. Results of Chow tests deliver strong evidence against the hypothesis that the first wave of Hartz reforms did not affect the matching process. However, these tests also provide some evidence that the Hartz reforms affected the structure of the matching technology. This points at even more far reaching implications of the Hartz reforms for labor markets than indicated by changes in the speed of matching. While these changes would potentially consider an interesting topic for future research, in this paper we restrict attention to the changes in matching efficiency that are attributable to the policy change. We view this as a conservative approach that is supported by results of the specification tests. Moreover, we proceed by assuming that the reforms only affect

the speed of matching in order to be able to conduct a parametric analysis of the effects.

All results so far heavily hinge on the identifying assumption that the implementation month of the Hartz I/II reform package indeed represents the correct threshold month for any reform effects. To test the robustness of the previous results, we also estimated all specifications for alternative implementation periods. Table 2 presents results for estimation results that anticipate or delay the implementation artificially by up to 3 months. In other words, the specifications displayed in columns (1), (2) and (3) treat November 2002, December 2002 or January 2003, respectively, as the "pseudo-Hartz" reform months. Column (4) displays the results for taking February 2003 as implementation month as in column (4) of Table 1. Columns (5), (6) and (7) of Table 2 present results for March 2003, April 2003 and May 2003 as alternative implementation months.[14]

The results obtained with earlier implementation periods than the real implementation period are consistent with the results obtained with the "correct" implementation period. Looking at Pseudo-Hartz reforms implemented earlier than the true reform delivers smaller and weaker treatment effects in any specification. In fact, there appears to be an upward trend in the strength of the treatment effect. Interestingly, when considering a treatment indicator that is lagged by one month, we obtain the largest positive treatment effects throughout all specifications, as indicated by column (5) in Table 2. A treatment lagged by two or three months reveals again much weaker results as the second set of estimations in that table shows. Figure 1 summarizes the effects of the first wave of Hartz reforms with alternative artificial implementation dates. It is noteworthy that our identification strategy allows for alternative interpretations of the treatment. For example, the general elections that were held on September 22, 2002, could be interpreted as alternative treatment that potentially might have affected labor market dynamics through, e.g., expectations of firms concerning changes in the legal environment. Interestingly, we find no effect for the specification that treats the period after October 2002 as implementation period, see column (1) in Table 2. This evidence lends additional support to our findings for the Hartz

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[14] Note that this includes the actual implementation of the "Mini-Jobs" on April 1, 2003.

laws that were actually designed to change labor market dynamics, and suggests that our results do not just pick up other events.

A potential problem for the validity of these results could be the use of retrospectively re-computed vacancy data that were constructed as consequence of the change in measurement in 2003/2004. In order to exclude those vacancies that could be affected by imprecise measurement, in particular those in 2001, we repeated the estimation only with data from 2002 and 2003. This gives us one year of comparison observations, and one year of observations with treatment to identify any reform effects. Moreover, this has the advantage of delivering a comparable identification design as that used for the Hartz III reforms below, for which we only have a two year window of data to identify any effects. The results of the estimations with the shorter data series indicate a weaker effect of the Hartz I/II reforms than before. Using the "correct" implementation month as break period, we find no significant effect. When shifting the implementation period, however, we find the strongest effect for the treatment lagged by one month, similar to the results using the longer sample. These results are depicted in Table 3. Figure 2 summarizes the results. This finding points to the robustness of our previous results.

When adding occupational variation to identify treatment effects, we find a similar picture, as shown by the results in Table 4. Again, the main treatment effect is strongest for the specifications with the approximately correct implementation period to identify the effect, columns (3), (4) and (5). The strongest main effect is found for a slightly delayed implementation in column (5). Also the occupation interactions reveal a similar picture as in the benchmark of Table 1. Compared to manufacturing occupations, all other broad occupations benefitted somewhat less from the Hartz reform implementation. The negative interaction effects only disappear for the specifications with an artificially delayed implementation, see columns (6) and (7).

Overall, the estimation results for the coefficients of the matching function are strikingly robust, regardless of the underlying matching model or the length of the control period. The results suggest that the speed of matching was significantly higher in periods

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of positive expectations about the future business conditions. The robustness results point at an implementation lag of the reforms of approximately one to two months. This can be seen when comparing the treatment effects for different implementation periods, see Figure 1. When interpreting the results, one should keep in mind that months in our analysis refer to report months. Treatment in January 2003 effectively refers to data collected between January 16, 2002 and February 15, 2003. A lag of one month as indicated by the results in Table 4 therefore indicates that the reform fully set in in late February 2003. Given the contents of the Hartz I and II reform packages (Mini-/Midi Jobs, exemptions from social security contributions for low paid jobs, subsidies for self-employment and the implementation of Job Centers), this lag in the effectiveness is not too surprising. We therefore interpret our results as first indication that, on the macro level, the first wave of Hartz reforms indeed had an effect on the process of matching and job creation.

Table 1: **The Effects of Hartz I/II Reforms on the Speed of Matching by Broad Occupations**

Dependent variable:	Logged Outflow from Unemployment into Employment: $\ln m_i t$			
	No Broad Occupation Interactions		Broad Occupation Interactions	
	(1)	(2)	(3)	(4)
$\ln U_{it}$	1.038***	1.007***	1.057***	1.026***
	[0.038]	[0.040]	[0.038]	[0.040]
$\ln V_{it}$	-0.016	-0.028	-0.019	-0.032
	[0.025]	[0.026]	[0.025]	[0.026]
$\ln u_{it}$	-0.150***	-0.178***	-0.143***	-0.170***
	[0.024]	[0.024]	[0.024]	[0.024]
$\ln v_{it}$	0.317***	0.375***	0.320***	0.379***
	[0.017]	[0.021]	[0.017]	[0.021]
IFO Business Expectations	0.006***	0.005**	0.006***	0.005**
Index (2000=1, R3)	[0.001]	[0.003]	[0.001]	[0.003]
Measurement Change Indicator	0.001	0.001	0.001	0.002
	[0.020]	[0.051]	[0.020]	[0.051]
Hartz Reform Indicator	0.048***	0.090**	0.112***	0.153***
(1=after 2003.1)	[0.017]	[0.038]	[0.024]	[0.041]
Interaction Reform*Occupation 2			-0.056**	-0.055**
			[0.028]	[0.028]
Interaction Reform*Occupation 3			-0.108***	-0.106***
			[0.028]	[0.028]
Interaction Reform*Occupation 4			-0.102***	-0.101***
			[0.028]	[0.028]
Constant	-0.550***	-0.548**	-0.549***	-0.538**
	[0.105]	[0.241]	[0.104]	[0.240]
Occupation Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	No	Yes	No	Yes
Observations	1840	1840	1840	1840
Number of Occupation Groups	40	40	40	40
R-squared (within)	0.45	0.48	0.46	0.49

Implied Overall Effects:

Effect of Hartz I/II on Occupation 1			0.112	0.153
Effect of Hartz I/II on Occupation 2			0.056	0.098
Effect of Hartz I/II on Occupation 3			0.004	0.047
Effect of Hartz I/II on Occupation 4			0.001	0.052

Note: Standard errors are in brackets. Sample period is March 2000-December 2003. Months are measured by months of reporting. Omitted reference months in all specifications with month dummies are the first month in the pre- and post-treatment period, respectively.

Table 2: **The Effects of Hartz I/II Reforms on the Speed of Matching – Alternative Implementation Periods**

Dependent variable:				Logged Outflow from Unemployment into Employment: $\ln m_{it}$			
Alternative Implementation:	-3 Months	-2 Months	-1 Month	Correct Month	+1 Month	+2 Months	+3 Months
Reform Indicator = 1	after 2002.10	after 2002.11	after 2002.12	after 2003.1	after 2003.2	after 2003.3	after 2003.4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\ln U_{it}$	1.007***	1.007***	1.007***	1.007***	1.007***	1.007***	1.007***
	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]
$\ln V_{it}$	-0.028	-0.028	-0.028	-0.028	-0.028	-0.028	-0.028
	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
$\ln u_{it}$	-0.178***	-0.178***	-0.178***	-0.178***	-0.178***	-0.178***	-0.178***
	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]
$\ln v_{it}$	0.375***	0.375***	0.375***	0.375***	0.375***	0.375***	0.375***
	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]
IFO Business Expectations	0.005**	0.005**	0.005**	0.005**	0.005**	0.005**	0.005**
Index (2000=1, R3)	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Control for Measurement Change	0.030	0.077	-0.011	0.001	-0.079	-0.019	-0.006
	[0.054]	[0.052]	[0.050]	[0.051]	[0.050]	[0.051]	[0.048]
(Pseudo) Hartz Reform Indicator	0.061	0.014	0.102***	0.090**	0.170***	0.110***	0.097***
	[0.039]	[0.038]	[0.038]	[0.038]	[0.037]	[0.039]	[0.038]
Constant	-0.548**	-0.548**	-0.548**	-0.548**	-0.548**	-0.548**	-0.548**
	[0.241]	[0.241]	[0.241]	[0.241]	[0.241]	[0.241]	[0.241]
Occupation Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1840	1840	1840	1840	1840	1840	1840
Number of Occupation Groups	40	40	40	40	40	40	40
R-squared (within)	0.48	0.48	0.48	0.48	0.48	0.48	0.48

Note: Standard errors are in brackets. Sample period is March 2000-December 2003. Months are measured by months of reporting. Omitted reference months in all specifications are the first month in the pre- and post-treatment period, respectively.

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Table 3: **The Effects of Hartz I/II Reforms on the Speed of Matching – Alternative Implementation Periods in Restricted Sample**

Dependent variable: Alternative Implementation: Reform Indicator = 1	Logged Outflow from Unemployment into Employment: $\ln m_{it}$						
	-3 Months after 2002.10 (1)	-2 Months after 2002.11 (2)	-1 Month after 2002.12 (3)	Correct Month after 2003.1 (4)	+1 Month after 2003.2 (5)	+2 Months after 2003.3 (6)	+3 Months after 2003.4 (7)
$\ln U_{it}$	0.972***	0.972***	0.972***	0.972***	0.972***	0.972***	0.972***
	[0.061]	[0.061]	[0.061]	[0.061]	[0.061]	[0.061]	[0.061]
$\ln V_{it}$	-0.039	-0.039	-0.039	-0.039	-0.039	-0.039	-0.039
	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]
$\ln u_{it}$	-0.163***	-0.163***	-0.163***	-0.163***	-0.163***	-0.163***	-0.163***
	[0.035]	[0.035]	[0.035]	[0.035]	[0.035]	[0.035]	[0.035]
$\ln v_{it}$	0.326***	0.326***	0.326***	0.326***	0.326***	0.326***	0.326***
	[0.031]	[0.031]	[0.031]	[0.031]	[0.031]	[0.031]	[0.031]
IFO Business Expectations	0	0.008	0.008	0.008	0.008	0.008	0.004
Index (2000=1, R3)	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.004]
Control for Measurement Change	0.097	0.036	-0.046	-0.036	-0.115	-0.053	0
	[0.064]	[0.075]	[0.072]	[0.073]	[0.071]	[0.073]	[0.000]
(Pseudo) Hartz Reform Indicator	-0.079	-0.048	0.034	0.024	0.103**	0.041	0.037
	[0.071]	[0.043]	[0.042]	[0.042]	[0.042]	[0.043]	[0.055]
Constant	0.072	-0.714	-0.714	-0.714	-0.714	-0.714	-0.396
	[0.527]	[0.436]	[0.436]	[0.436]	[0.436]	[0.436]	[0.394]
Occupation Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	960	960	960	960	960	960	960
Number of Occupation Groups	40	40	40	40	40	40	40
R-squared (within)	0.36	0.36	0.36	0.36	0.36	0.36	0.36

Note: Standard errors are in brackets. Sample period is January 2002-December 2003. Months are measured by months of reporting. Omitted reference months in all specifications are the first month in the pre- and post-treatment period, respectively.

Table 4: **The Effects of Hartz I/II Reforms on the Speed of Matching by Broad Occupations – Alternative Implementation Periods**

Dependent variable: Alternative Implementation: Reform Indicator = 1	Logged Outflow from Unemployment into Employment: ln m_{it}						
	-3 Months after 2002.10 (1)	-2 Months after 2002.11 (2)	-1 Month after 2002.12 (3)	Correct Month after 2003.1 (4)	+1 Month after 2003.2 (5)	+2 Months after 2003.3 (6)	+3 Months after 2003.4 (7)
ln U_{it}	1.024***	1.025***	1.026***	1.026***	1.024***	1.013***	1.005***
	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]	[0.040]
ln V_{it}	-0.027	-0.028	-0.030	-0.032	-0.032	-0.029	-0.027
	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
ln u_{it}	-0.176***	-0.176***	-0.173***	-0.170***	-0.169***	-0.174***	-0.176***
	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]	[0.024]
ln v_{it}	0.378***	0.378***	0.378***	0.379***	0.378***	0.376***	0.375***
	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]
IFO Business Expectations Index (2000=1, R3)	0.005**	0.005**	0.005**	0.005**	0.005**	0.005**	0.005**
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Control for Measurement Change	0.031	0.078	-0.010	0.002	-0.078	-0.019	-0.006
	[0.054]	[0.051]	[0.050]	[0.051]	[0.050]	[0.051]	[0.048]
(Pseudo) Hartz Reform Indicator	0.108**	0.064	0.161***	0.153***	0.220***	0.122***	0.084**
	[0.042]	[0.041]	[0.041]	[0.041]	[0.041]	[0.042]	[0.042]
Interaction Reform*Occupation 2	-0.045*	-0.049*	-0.059**	-0.055**	-0.032	0.015	0.051
	[0.026]	[0.026]	[0.027]	[0.028]	[0.029]	[0.030]	[0.031]
Interaction Reform*Occupation 3	-0.061**	-0.070***	-0.081***	-0.106***	-0.097***	-0.031	0.015
	[0.026]	[0.026]	[0.027]	[0.028]	[0.029]	[0.030]	[0.032]
Interaction Reform*Occupation 4	-0.079***	-0.084***	-0.103***	-0.101***	-0.082***	-0.038	-0.013
	[0.026]	[0.026]	[0.027]	[0.028]	[0.029]	[0.030]	[0.032]
Constant	-0.538**	-0.539**	-0.539**	-0.538**	-0.539**	-0.545**	-0.548**
	[0.240]	[0.240]	[0.240]	[0.240]	[0.240]	[0.241]	[0.241]
Occupation Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1840	1840	1840	1840	1840	1840	1840
Number of Occupation Groups	40	40	40	40	40	40	40
R-squared (within)	0.48	0.48	0.49	0.49	0.49	0.48	0.48
R-squared (between)	0.92	0.92	0.92	0.92	0.92	0.92	0.92

Note: Standard errors are in brackets. Sample period is March 2000-December 2003. Months are measured by months of reporting. Omitted reference months in all specifications are the first month in the pre- and post-treatment period, respectively.

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5.2   The Effects of Hartz III

As discussed in the introduction, after the implementation of the first set of Hartz reforms (Hartz I and II) on January 1, 2003, a second set of reforms became effective on January 1, 2004. These so-called Hartz III reforms implied a reorganization of the federal employment agency and its local employment offices. Their intention was explicitly to improve the matching process by making the placement process through employment offices more effective. The analysis so far has concentrated on the first reform wave and neglected potentially distinct effects of this second set of reforms. In this section we perform a comparable analysis to the one presented above to evaluate the effects of the second wave of Hartz reforms. To do this, we restrict attention to data from January 2003 until December 2004. To facilitate the comparison and highlight the robustness of the results the analysis follows the same steps as before.

The results of these estimations are displayed in Table 5. Columns (1) and (2) reflect results for the baseline stock-flow model without and with time fixed effects, respectively. All specifications allow for serially autocorrelated disturbances. The structural features of the matching process, reflected in the matching elasticities of stocks and inflows of unemployed and vacancies, respectively, are somewhat different from those obtained in our previous analysis. The qualitative pattern is similar, however. What is striking is the much larger elasticity of matches with respect to the unemployment stock. This is another indication for the finding mentioned above that the Hartz reforms apparently also affected the technological structure of the matching process. In particular, the results point at stronger increasing returns to scale than the results in the last section. Another noteworthy finding is the lower relative importance of the vacancy stock in the matching process. Also inflows to unemployment and to the stock of vacancies play a comparably smaller role in the sample considered to evaluate the second wave of Hartz reforms. Business expectations affect unemployment outflows only in the specifications without month fixed effects.

In terms of the effects of the Hartz III reforms, we find weakly positive main effects, indicating that the speed of the matching process accelerated after the implementation

of the second reform wave compared to the year before, after the first reform wave. The strongest effect is found for the stock-flow specification with month fixed effects, where we find a marginally significant positive effect of the second reform wave.

Columns (3) and (4) of Table 5 present results for specifications using occupational variation. The main effect of the reform is consistently significant and positive. The strongest positive effect is found for the preferred specification, the stock-flow model with month fixed effects in column (4). According to this specification, the second reform wave accelerated the unemployment outflows into employment by more than 10 percent on average for manufacturing occupations. As with the first reform wave Hartz I/II analyzed before, interactions with broad occupations show that manufacturing occupations benefitted relatively more from the implementation of the Hartz III reforms. Unlike there, we find negative coefficients for all three remaining occupations, but only a marginally significant effect for white collar and skilled occupations (broad occupation group 3). Also, unlike for the Hartz I/II reforms, white collar occupations show a much smaller negative interaction coefficient. While the results for the specification without month effects even show negative treatment effects for occupations 2, 3 and 4, we find positive effects for our preferred specification with month fixed effects. As for Hartz I/II, these are smaller than the effects for the reference occupation 1, however.

Chow tests of the stability of the matching technology reveal a significant overall effect of the reforms on the matching process. In particular, the negative effect of unemployment inflows on the outflow of unemployed into employment appears to have worsened with the reform implementation.

As before, we investigate the robustness of the results by estimating similar models with different implementation thresholds for the policy indicator. The results of these estimations are depicted in Tables 6 and 7. Figure 3 illustrates the results from Table 6 graphically. The results indicate a clear hump-shape profile of main treatment effects over the alternative implementation periods. In fact, we find negative effects when shifting the implementation artificially three months back in time, as indicated by the results of

column (1). In contrast, when artificially delaying the implementation by two or three months, we find no significant treatment effect. As with the first reform wave, however, the main effect of the reform is not largest for the specification with the 'correct' definition of the treatment month. In contrast to before, however, we find the largest effect for the specification with treatment starting in January 2004. This points at effects of the reform that set in right at the time of implementation in early January 2004 (remember that the January 2004 report month contains data collected between mid-December 2003 and mid-January 2004). This implies that the Hartz III reforms, which implied a reorganization of the federal employment agency and its local employment offices that went into effect on January 1, 2004, were successful in accelerating the matching process immediately. Given that these reorganizations were preceded by preparations and anticipatory training measures of staff of the employment agencies this immediate treatment effect is not overly surprising.

Table 5: **The Effects of the Hartz III Reform on the Speed of Matching by Broad Occupations**

| Dependent variable: | Logged Outflow from Unemployment into Employment: $\ln m_i t$ | | | |
| | No Broad Occupation Interactions | | Broad Occupation Interactions | |
	(1)	(2)	(3)	(4)
$\ln U_{it}$	2.227***	2.268***	2.252***	2.293***
	[0.075]	[0.073]	[0.075]	[0.074]
$\ln V_{it}$	0.037***	0.043***	0.032**	0.038***
	[0.014]	[0.014]	[0.014]	[0.014]
$\ln u_{it}$	-0.118***	-0.117***	-0.119***	-0.118***
	[0.014]	[0.014]	[0.014]	[0.014]
$\ln v_{it}$	0.112***	0.118***	0.113***	0.120***
	[0.017]	[0.017]	[0.017]	[0.017]
IFO Business Expectations	0.015***	0.002	0.015***	0.002
Index (2000=1, R3)	[0.003]	[0.004]	[0.003]	[0.004]
Control for Measurement Change	0.048*	-0.033	0.049*	-0.033
	[0.028]	[0.042]	[0.028]	[0.042]
Hartz III Reform Indicator	-0.009	0.065*	0.043*	0.116***
(1=after 2004.1)	[0.014]	[0.035]	[0.024]	[0.039]
Interaction Reform*Occupation 2			-0.064**	-0.065**
			[0.033]	[0.032]
Interaction Reform*Occupation 3			-0.060*	-0.059*
			[0.033]	[0.032]
Interaction Reform*Occupation 4			-0.089***	-0.089***
			[0.033]	[0.032]
Constant	-1.477***	-0.192	-1.478***	-0.198
	[0.286]	[0.435]	[0.286]	[0.434]
Occupation Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	No	Yes	No	Yes
Observations	920	920	920	920
Number of Occupation Groups	40	40	40	40
R-squared (within)	0.53	0.57	0.54	0.57

Implied Overall Effects:				
Effect of Hartz III on Occupation 1			0.043	0.116
Effect of Hartz III on Occupation 2			-0.021	0.051
Effect of Hartz III on Occupation 3			-0.017	0.057
Effect of Hartz III on Occupation 4			-0.046	0.027

Note: Standard errors are in brackets. Sample period is March 2003-December 2004. Months are measured by months of reporting. Omitted reference months in all specifications with month dummies are the first month in the pre- and post-treatment period, respectively.

Table 6: **The Effects of Hartz III Reforms on the Speed of Matching – Alternative Implementation Periods**

Dependent variable:	Logged Outflow from Unemployment into Employment: ln m_{it}						
Alternative Implementation:	-3 Months	-2 Months	-1 Month	Correct Month	+1 Month	+2 Months	+3 Months
Reform Indicator = 1	after 2003.10	after 2003.11	after 2003.12	after 2004.1	after 2004.2	after 2004.3	after 2004.4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ln U_{it}	2.268***	2.268***	2.268***	2.268***	2.268***	2.268***	2.268***
	[0.073]	[0.073]	[0.073]	[0.073]	[0.073]	[0.073]	[0.073]
ln V_{it}	0.043***	0.043***	0.043***	0.043***	0.043***	0.043***	0.043***
	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]
ln u_{it}	-0.117***	-0.117***	-0.117***	-0.117***	-0.117***	-0.117***	-0.117***
	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]
ln v_{it}	0.118***	0.118***	0.118***	0.118***	0.118***	0.118***	0.118***
	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]
IFO Business Expectations	0.009	0.012**	0.002	0.002	0.002	0.002	0.002
Index (2000=1, R3)	[0.006]	[0.005]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Control for Measurement Change	-0.019	-0.014	-0.033	-0.033	-0.033	-0.033	-0.033
	[0.044]	[0.043]	[0.042]	[0.042]	[0.042]	[0.042]	[0.042]
(Pseudo) Hartz Reform Indicator	0.021	-0.088**	0.168***	0.065*	0.113***	0.023	0.013
	[0.043]	[0.040]	[0.038]	[0.035]	[0.034]	[0.033]	[0.033]
Constant	-0.894	-1.153**	-0.192	-0.192	-0.192	-0.192	-0.192
	[0.587]	[0.484]	[0.435]	[0.435]	[0.435]	[0.435]	[0.435]
Occupation Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	920	920	920	920	920	920	920
Number of Occupation Groups	40	40	40	40	40	40	40
R-squared (within)	0.57	0.57	0.57	0.57	0.57	0.57	0.57

Note: Standard errors are in brackets. Sample period is March 2003-December 2004. Months are measured by months of reporting. Omitted reference months in all specifications are the first month in the pre- and post-treatment period, respectively.

Table 7: **The Effects of Hartz III Reforms on the Speed of Matching by Broad Occupations– Alternative Implementation Periods**

Dependent variable: Alternative Implementation: Reform Indicator = 1	Logged Outflow from Unemployment into Employment: $\ln m_{it}$						
	-3 Months after 2003.10 (1)	-2 Months after 2003.11 (2)	-1 Month after 2003.12 (3)	Correct Month after 2004.1 (4)	+1 Month after 2004.2 (5)	+2 Months after 2004.3 (6)	+3 Months after 2004.4 (7)
$\ln U_{it}$	2.277***	2.280***	2.287***	2.293***	2.293***	2.281***	2.258***
	[0.074]	[0.074]	[0.074]	[0.074]	[0.074]	[0.075]	[0.075]
$\ln V_{it}$	0.043**	0.042***	0.041***	0.038***	0.040***	0.045***	0.046***
	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]
$\ln u_{it}$	-0.117***	-0.117***	-0.117***	-0.118***	-0.118***	-0.119***	-0.119***
	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]	[0.014]
$\ln v_{it}$	0.116***	0.116***	0.116***	0.120***	0.123***	0.121***	0.121***
	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]
IFO Business Expectations Index (2000=1, R3)	0.009	0.012**	0.002	0.002	0.002	0.002	0.002
	[0.006]	[0.005]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Control for Measurement Change	-0.019	-0.014	-0.032	-0.033	-0.033	-0.033	-0.033
	[0.044]	[0.043]	[0.042]	[0.042]	[0.042]	[0.042]	[0.042]
(Pseudo) Hartz Reform Indicator	0.036	-0.063	0.210***	0.116***	0.150***	0.028	-0.021
	[0.047]	[0.045]	[0.043]	[0.039]	[0.039]	[0.039]	[0.039]
Interaction Reform*Occupation 2	-0.022	-0.042	-0.064**	-0.065**	-0.024	0.035	0.092***
	[0.032]	[0.032]	[0.032]	[0.032]	[0.032]	[0.033]	[0.033]
Interaction Reform*Occupation 3	0.006	-0.007	-0.029	-0.059*	-0.052	-0.005	0.054
	[0.033]	[0.032]	[0.032]	[0.032]	[0.032]	[0.033]	[0.034]
Interaction Reform*Occupation 4	-0.044	-0.051	-0.078**	-0.089***	-0.071**	-0.043	-0.008
	[0.033]	[0.032]	[0.032]	[0.032]	[0.032]	[0.033]	[0.034]
Constant	-0.898	-1.157**	-0.196	-0.198	-0.198	-0.195	-0.189
	[0.587]	[0.484]	[0.434]	[0.434]	[0.435]	[0.435]	[0.433]
Occupation Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	920	920	920	920	920	920	920
Number of Occupation Groups	40	40	40	40	40	40	40
R-squared (within)	0.57	0.57	0.57	0.57	0.57	0.57	0.57

Note: Standard errors are in brackets. Sample period is March 2003-December 2004. Months are measured by months of reporting. Omitted reference months in all specifications are the first month in the pre- and post-treatment period, respectively.

6 Conclusion

This paper provides the first attempt to evaluate the macroeconomic effects of the implementation of the most significant reforms on the labor market in Germany, the so-called Hartz reforms. We use an empirical strategy that is based on estimating empirical matching functions to evaluate the determinants of employment inflows. This model reflects closely the intentions of the reforms to stimulate labor market dynamics, which was explicitly based on a flow approach to solve the unemployment problem. We find that the first reform waves, Hartz I/II and Hartz III, indeed had a significant positive impact on the process of job creation. In particular, we find that the reforms accelerated the outflows from unemployment to employment compared to the pre-reform periods.

The results also show that the reform effects are heterogeneous across different segments of the labor market. In particular, we find that compared to manufacturing occupations, other occupations benefitted less from the reforms in terms of the acceleration of the matching process.

The construction of the identification strategy – we separately identify the effects of the two reform waves comparing the pre-implementation to the post-implementation structure of the matching process – implies that both reform waves complement each other. Taken together, the effects of both reforms are even larger than the effects of the single reform waves suggest, simply because the second reform shows a positive effect on the speed of unemployment outflows compared to the period after the first wave. This compounding effect must be kept in mind when interpreting the size of the effects we find in this paper.

Severe problems with the data, in particular changes in data definitions and measurement practices at the same time of the implementation of the third reform wave, the Hartz IV reforms, rule out an analogous evaluation. Nevertheless, this paper constitutes an important first step to complement micro-based evaluation studies of the Hartz reforms.

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Table 8: **Occupational Groups**

Occupation	Occupational Code (official classification)	Broad Occupation*
plant cultivator/stockbreeding/fisher	01-05	1
forester/huntsman	06	1
miner/exhauster of mineral resources	07-09	1
stone processor/producer of building materials	10-11	1
ceramicist/glazier	12-13	1
chemical worker/polymer processor	14-15	1
paper producer	16	1
printer	17	1
woodworker/woodprocessor	18	1
metal worker	19-24	1
locksmith/mechanic	25-30	2
electrician	31	2
assembler/metal-related professions	32	2
textile related professions	33-36	2
leather and fur manufacturer	37	2
nutrition-related professions	39-43	2
construction-related professions	44-47	2
interior designer/furniture supplier/ upholsterer	48-49	2
carpenter/modeler	50	2
painter/varnisher/related professions	51	2
goods tester/consignment professions	52	4
unskilled worker	53	4
machinist/related professions	54	4
engineer/chemist/ physicist/mathematician	60-61	3
technician	62	3
technical specialist	63	3
merchandise manager	68	3
service merchants	69-70	3
transportation-related professions	71-73	4
storekeeper/worker in storage and transport	74	4
organization-/management-/office- related professions	75-78	3
security-service-related professions	79-81	4
publicist/translator/librarian	82	3
artists and related professions	83	3
health care-related professions	84-85	3
social worker/pedagogue/science careers	86-89	3
beauty culture	90	4
guest assistant/steward/barkeeper	91	4
domestic economy/housekeeping	92	4
cleaning-industry-related professions	93	4

* Occupations are merged into the following equally sized broad occupational groups:

1. Occupations in agriculture and manufacturing.
2. Crafts.
3. White collar and high skill occupations.
4. Service sector and low skill occupations.

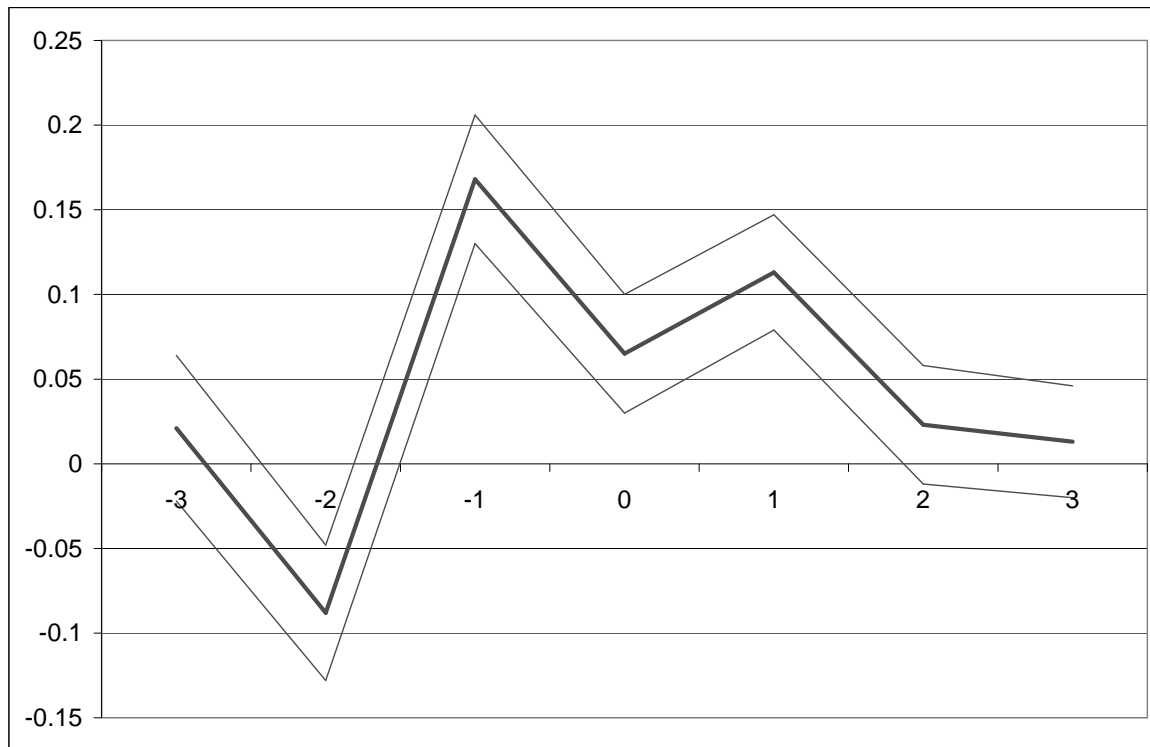Figure 1: The Treatment Effect for Hartz I/II Reforms for Alternative Implementation Periods
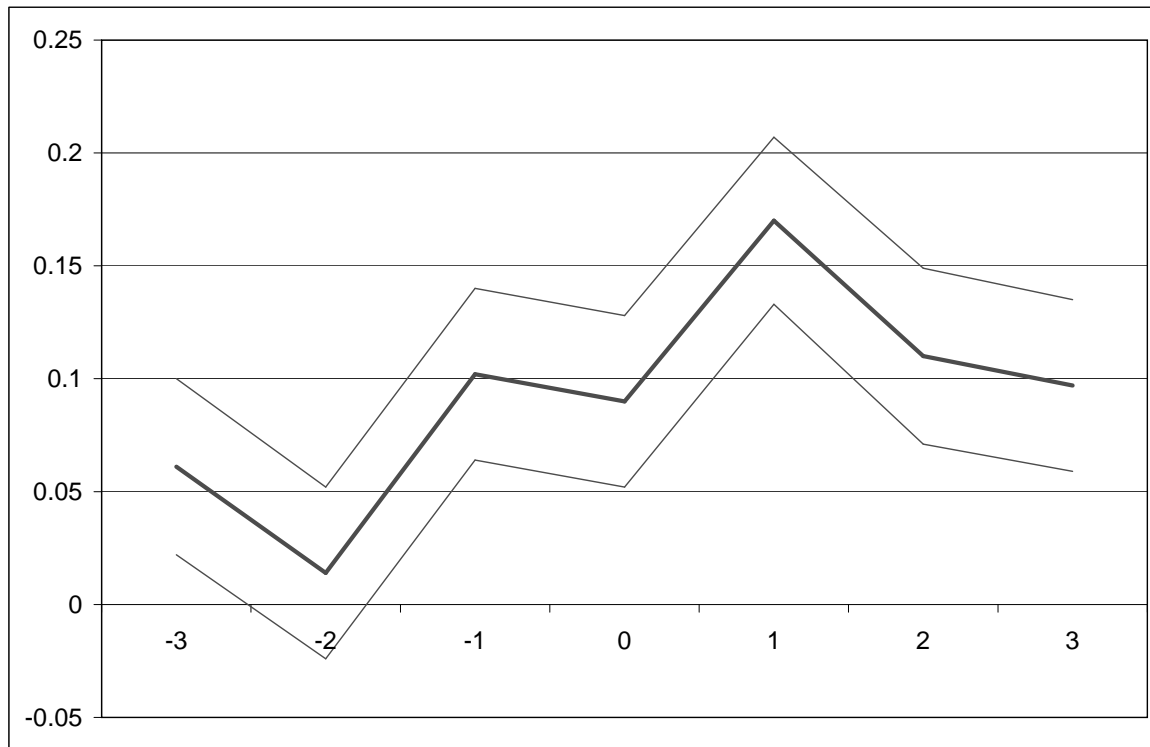
Notes: The figure shows the the coefficient of the Hartz I/II treatment with a band of 1 standard deviation for alternative implementation periods compared to the actual implementation month (0 = February 2003, corresponding to the period January 16 to February 15, 2003). The figure is based on results presented in Table 2.

Figure 2: The Treatment Effect for Hartz I/II Reforms for Alternative Implementation Periods in Restricted Sample
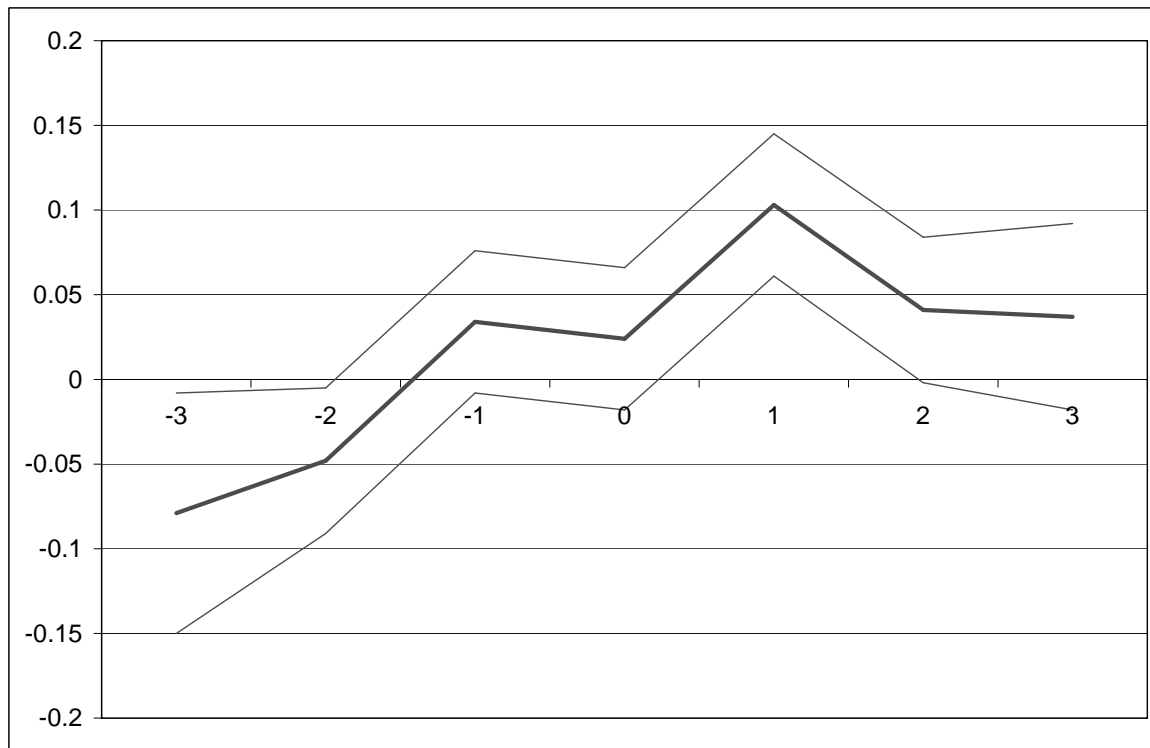
Notes: The figure shows the the coefficient of the Hartz I/II treatment with a band of 1 standard deviation for alternative implementation periods compared to the actual implementation month (0 = February 2003, corresponding to the period January 16 to February 15, 2003). The figure is based on results presented in Table 3.

Figure 3: The Treatment Effect for Hartz III Reforms for Alternative Implementation Periods

Notes: The figure shows the the coefficient of the Hartz I/II treatment with a band of 1 standard deviation for alternative implementation periods compared to the actual implementation month (0 = February 2004, corresponding to the period January 16 to February 15, 2004). The figure is based on results presented in Table 6.