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

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Abstract. *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.*

JEL classification: *J*23, *J*64, *J*68, *H*53.

Keywords: Empirical matching function; stock-flow matching; Hartz reform.

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 into 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 the 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 1 January 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 1 January 2004, implementing 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 1 January 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 evaluate the effectiveness of the reforms scientifically. For a detailed description of the reforms, their background 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 a consequence of the Hartz I reform (see Bonin and Schneider, 2006; Jacobi and Kluve, 2006), whereas other reform packages like the 'mini jobs' show no significant employment effects (see Caliendo and Wrolich, 2008).

Instead of evaluating any of the specific components of the Hartz reforms, this paper adopts an entirely different, complementary approach and

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 the macroeconomic aspects of the Hartz reform, but focuses on the effectiveness of expenditures, in particular, active labor market programs; see Fertig *et al*. (2005).

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 this, we apply the workhorse of modern 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 on improving the matching between unemployed and worker-searching firms and the goal to reduce unemployment 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 *et al*. (2006) and 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 *et al*. (2006) use regional data on the basis of employment agency districts for the years 1998–2000 to evaluate the effectiveness of expenditures for different particular policy measures on gross and net labor market flows.

Since our analysis is concerned with estimating the reform effects on the speed of the matching process, an important issue is the definition of the relevant labor markets. We follow Fahr and Sunde (2004) in using occupations as defining labor markets, because in the German context occupation is generally the relevant criterion for search activities by the unemployed and firms. Usually, firms post vacancies for certain qualifications in terms of occupation or education (which are closely related, given the German dual-track education system), and workers primarily look for jobs in their occupation. Given the German dual-track education system, with a strong emphasis on occupational education through the apprenticeship system, occupations are best suited to capture differences in qualificatory demands, differences in skill requirements, particular search channels and search intensity, screening problems and matching speed – essential issues that affect the frictions that characterize the matching process and thus underlie the idea of matching functions. Previous evidence also suggests that occupational mobility is very low. An occupational definition of labor markets therefore appears better suited for the purpose of this paper than a disaggregation by industries or regions, which typically employ all sorts of occupations, albeit with different proportions. Finally, we think that

investigating the effects on the level of occupations is most interesting from the perspective of labor market policy, given that the Hartz reforms also introduced qualification measures, but were less concerned with bolstering regional mobility. To investigate the robustness of our results with respect to alternative definitions of relevant labor markets, we also conduct the analysis using a regional concept of labor markets.

In our analysis, we use panel data for 40 occupational groups over the period March 2000 until December 2004 with a monthly frequency. This allows us to present a first evaluation of the effects of the Hartz laws both on the aggregate level as well as on the level of occupational groups. Moreover, our analysis is the first estimation of the matching technology for Germany using data with a monthly frequency. Empirical matching functions have been estimated for Germany before (see e.g. Entorf, 1998; Fahr and Sunde, 2004, 2005, 2006a, 2006b; Gross, 1997). However, all these estimates for Germany have used data on a quarterly or even an 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 a 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 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 an observation period for job creation but also of the inflows into these pools during the observation period (see Coles and Petrongolo, 2002; Coles and Smith, 1998; Gregg and Petrongolo, 2005; Petrongolo and Pissarides, 2001). 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 a consequence of the Hartz reforms, are fairly robust to the underlying parameterization of the matching function.

According to our estimates, the implementation of the Hartz I and II reforms on 1 January 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 the strongest right at the beginning of the implementation period. Again, manufacturing occupations appear to have benefited most from the reform

that implied changes in the intermediation process through employment agencies. We also find positive effects of the Hartz reforms when using data disaggregated by region instead of occupations, with the relatively strongest effects in East German regions. Overall, the results point to 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 centerpiece 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 and can be micro-founded (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 that therefore has been referred to as the stock–stock approach, has been repeatedly criticized (see e.g. Coles and Smith, 1998; Gregg and Petrongolo, 2005; Petrongolo and Pissarides, 2001). The reason is that the stock–stock approach implicitly assumes an undirected, random underlying search process leading to matches between homogeneous unemployed and homogeneous vacancies. However, if 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

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

where U_t is the stock of unemployed at time t, u_t the inflow of new unemployed between t and $t+1$ and, likewise, V_t the stock of vacancies at t and v_t the inflow of vacancies between t and $t+1$, respectively, and ε_{it} 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 by 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 a very detailed empirical concept of new hires. Our baseline measure comprises 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 the basis of individual register data used by Fahr and Sunde (2004, 2005) are not available for the period after 2002 at the time of writing. 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 coarser measure of unemployment outflows as the primary dependent variable (see e.g. Broersma and van Ours, 1999; Petrongolo and Pissarides, 2001). It is worth noting that our measure excludes unemployment inflows into training measures or public job-creation schemes.

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

$$U_{it} = U_{it-1} + u_{it-1} - m_{it-1} \quad (2)$$

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,

$$\varepsilon_{it} = \rho \varepsilon_{it-1} + \zeta_{it} \quad (3)$$

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 consequence, 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 of 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 changes in the environment other 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.[2]

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 before 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.[3] To provide a more detailed account of the reform effects, 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 technological parameters of the matching function: the α's and β's. We will report the results of appropriate tests below. However, as is discussed in more detail in the following, the possibility of identifying 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.

2. Addition of further occupation-specific, time-varying controls, such as the share of long-term unemployed, would be technically possible, but is prevented by the unavailability of respective data on the required levels of disaggregation.
3. 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.

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.[4] 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.[5] Vacancy data refer to vacancies registered at employment offices, and are therefore of a higher quality and precision than most vacancy data available in other countries that are based, for example, on help-wanted indices.[6] Our dependent variables are the respective outflows out of unemployment into employment, disaggregated by occupational group. The data for our main analysis are aggregated on the level of 40 occupational groups. The occupation classification we use is provided on the 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.[7] In our regressions, we control for four broad groups of loosely related occupations. For part of the analysis, we also use data disaggregated by federal states (*Bundesland*) and control for regional differences between four major regions in Germany. To control for business cycle effects, we use an index of business expectations provided by the Ifo Institute in Munich.[8] In order to

4. This implies that we assign, for example, the month 'December 2005' to the data collected between 18 November and 15 December 2005; see also BA (2004).
5. Data can be downloaded from the homepage of the federal employment agency: http://www.arbeitsamt.de
6. Registration of vacancies is not mandatory in Germany, but typically around 60% of all job accessions are accounted for by registered vacancies in Germany. One should note that the results will be largely unaffected by the utilization of registered vacancies as long as the use of alternative search channels did not change systematically with the reform implementation. See also Fahr and Sunde (2005) and Sunde (2007) for a more detailed investigation of search channels.
7. See also the discussions and results presented in Fahr and Sunde (2004, 2005) and Sunde (2007).
8. 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.

eliminate any spurious seasonal variation that might affect our results, we adjust each series by regressing it on a full set of month-of-year 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 a consequence, we observe breaks in the respective data series used for the analysis.[9]

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 a 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 placement statistics for the time 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 efforts 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 and outflows into self-employment) that are most affected by errors in the statistical procedure before May 2003.[10]

9. The information reported in this section follows two documents available at the homepage of the Federal Employment Agency (http://www.arbeitsagentur.de): http://www.pub.arbeitsamt.de/hst/services/statistik/000100/html/interpretation/Uebergreifendes/03.Allg.St.Hinw.pdf (1 September 2006) and http://www.pub.arbeitsamt.de/hst/services/statistik/000100/html/interpretation/Ausgleichsprozesse/05.Vermittl.Statistik.pdf (1 September 2006).
10. This information has been provided by personal conversation with Erich Janka at the statistic service department of the Federal Employment Agency in August 2006.

As a consequence, we take outflows into employment as the relevant outflows in our empirical analysis, regardless of whether these were generated as a consequence of an unemployed's own search efforts or 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 affects 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 decline by around 3–6% in 2004 after the implementation of the new technology, compared to 2003. In order to provide consistent statistics for the time before the technological upgrade, the Federal Employment Agency recomputed the labor market statistics for the unemployed with the new system back to 1998, however. The vacancy 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 at 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. Consequently, there was an increase in the number of unemployed that was not due to economic changes but only caused by the redefinition of the unemployment status. Thus, the intervention not only leads 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 focus attention on 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, does not appear to be feasible on the basis of the publicly available data, because of the simultaneous structural breaks in dependent and explanatory variables.

In our preferred specification for identifying the 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.[11] As the control period, we use observations before the particular reform under consideration and, as the 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, each comprising ten occupations: occupations in the agricultural and manufacturing sector (group 1), craft occupations (group 2), white-collar and high-skill occupations (group 3), and service sector and low-skill occupations (group 4). Table 10 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.[12]

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 one, the estimates are similar to those usually found with high-frequency data in the international literature for unemployment outflows as the dependent variable.[13] In contrast, the

11. 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.
12. 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 20–75% higher for broad occupation groups 1 and 2 compared to groups 3 and 4.
13. Previous studies using data at a lower frequency found consistently lower coefficients, presumably due to time aggregation; see, for example, Entorf (1998) and Fahr and Sunde (2004), as well as Broersma and van Ours (1999) for a survey.

Table 1 The effects of Hartz I/II reforms on the speed of matching by broad occupations

	Logged outflow from unemployment into employment: $\ln m_{it}$			
	No broad occupation interactions		Broad occupation interactions	
Dependent variable	(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 index (2000 = 1, R3)	0.006***	0.005**	0.006***	0.005**
	[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 (1 = after 2003.1)	0.048***	0.090**	0.112***	0.153***
	[0.017]	[0.038]	[0.024]	[0.041]
Reform × occupation 2 interaction			−0.056**	−0.055**
			[0.028]	[0.028]
Reform × occupation 3 interaction			−0.108***	−0.106***
			[0.028]	[0.028]
Reform × occupation 4 interaction			−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	1,840	1,840	1,840	1,840
Number of occupation groups	40	40	40	40
R^2 (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

Notes: Standard errors are in brackets. The 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.
***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

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 that usually reported in the literature (see Broersma and van Ours, 1999, Table 1).[14] The inflows into vacancies exhibit significant positive effects on the measure of matches, whereas the effect of the inflows into unemployment affects 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 dataset. These results also provide some empirical indication of 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.[15] A better business climate seems to affect the speed of the matching process in column (1), but not in specification (2). In terms of the 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 5–10%, 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.[16]

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

14. The results obtained by regressing unemployment outflows on stocks of vacancies and unemployed have been criticized repeatedly on the grounds of compatibility of stocks and flows, and competition among different pools of searchers and vacancies (see Anderson and Burgess, 2000; Broersma and van Ours, 1999; Mumford and Smith, 1999; Sunde, 2007). 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 we can do at this stage.
15. See Gregg and Petrongolo (2005) and Petrongolo and Pissarides (2006) for discussions of the issues of scale and time aggregation.
16. 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 on request.

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.[17]

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 benefited relatively the most from the reform implementation. Crafts (broad occupation group 2) was affected moderately less, but high- and low-skill occupations (groups 3 and 4) have benefited significantly less from the reform than manufacturing occupations. The lower panel summarizes the results for the different occupations separately, by adding up the main effects and the 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 suggest that the first wave of Hartz reforms did affect the matching process. However, these tests also provide some evidence that the Hartz reforms affected the structure of the matching technology. This points to potentially even more far-reaching implications of the Hartz reforms for labor markets than indicated by changes in the speed of matching. While these changes could potentially be an interesting topic for future research, in this paper, we restrict our 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. In this context, it is also worth noting that the results are obtained with a very flexible dynamic specification with fixed effects for all months in the sample. In this sense, the specification used for detecting reform effects is very conservative.

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 the results for estimation results that anticipate or delay the implementation artificially by up to three months. In other words, the specifications displayed in columns (1), (2) and (3)

17. A test of joint relevance of the four Hartz coefficients reveals values of $F(4, 1790) = 6.66$ and $F(4, 1748) = 6.15$, with significance at the 1% level for columns (3) and (4), respectively.

Table 2 The effects of Hartz I/II reforms on the speed of matching – alternative implementation periods

	Logged outflow from unemployment into employment: $\ln m_{it}$						
Dependent variable:							
Alternative implementation:	− 3 months after 2002.10	− 2 months after 2002.11	− 1 month after 2002.12	Correct month after 2003.1	+ 1 month after 2003.2	+ 2 months after 2003.3	+ 3 months after 2003.4
Reform indicator = 1	(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	0.030	0.077	− 0.011	0.001	− 0.079	− 0.019	− 0.006
measurement change	[0.054]	[0.052]	[0.050]	[0.051]	[0.050]	[0.051]	[0.048]
(Pseudo) Hartz reform	0.061	0.014	0.102***	0.090**	0.170***	0.110***	0.097***
indicator	[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	1,840	1,840	1,840	1,840	1,840	1,840	1,840
Number of occupation groups	40	40	40	40	40	40	40
R^2 (within)	0.48	0.48	0.48	0.48	0.48	0.48	0.48

Notes: Standard errors are in brackets. The 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.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

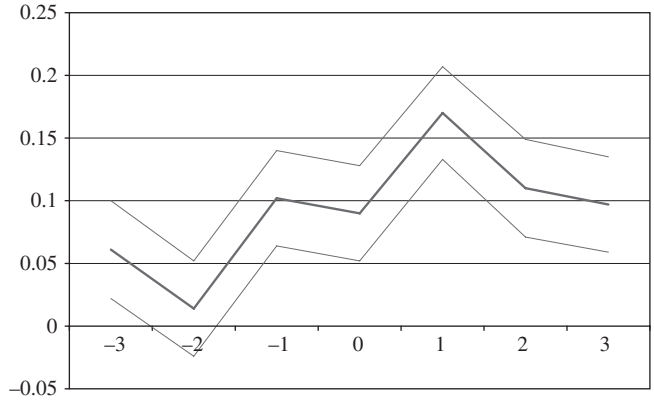

Figure 1 The treatment effect for Hartz I/II reforms for alternative implementation periods

Notes: The figure shows 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 16 January to 15 February 2003). The figure is based on results presented in Table 2.

treat November 2002, December 2002 or January 2003, respectively, as the 'pseudo-Hartz' reform months. Column (4) displays the results taking February 2003 as the implementation month as in column (4) of Table 1. Columns (5), (6) and (7) of Table 2 present the results for March 2003, April 2003 and May 2003, respectively, as alternative implementation months.[18]

The results obtained with implementation periods earlier 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 again reveals 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 22 September 2002 could be interpreted as an alternative treatment that might have potentially affected labor market dynamics through, for example, 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 the implementation

18. Note that this includes the actual implementation of the 'Mini-Jobs' on 1 April 2003.

period; see column (1) in Table 2. This evidence lends additional support to our findings for the Hartz 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 in terms of the validity of these results could be the use of retrospectively recomputed vacancy data that were constructed as a consequence of the change in measurement in 2003/04. 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 a 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 the 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 benefited 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 of positive expectations about the future business conditions. The robustness results point to 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 starting in February 2003 effectively refers to data collected between 16 January 2002 and 15 February 2003. A lag of one month as indicated by the results in Table 4 therefore indicates that the reform fully set 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 a

Table 3 The effects of Hartz I/II reforms on the speed of matching – alternative implementation periods in restricted sample

	Logged outflow from unemployment into employment: $\ln m_{it}$						
Dependent variable:							
Alternative implementation:	− 3 months after 2002.10	− 2 months after 2002.11	− 1 month after 2002.12	Correct month after 2003.1	+ 1 month after 2003.2	+ 2 months after 2003.3	+ 3 months after 2003.4
Reform indicator = 1	(1)	(2)	(3)	(4)	(5)	(6)	(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^2 (within)	0.36	0.36	0.36	0.36	0.36	0.36	0.36

Notes: Standard errors are in brackets. The 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.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

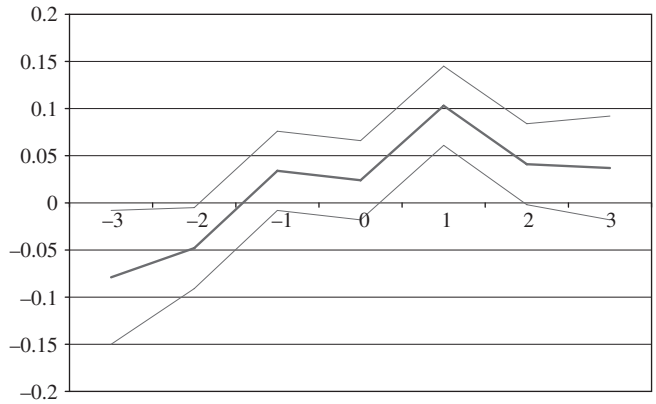

Figure 2 The treatment effect for Hartz I/II reforms for alternative implementation periods in restricted sample

Notes: The figure shows 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 16 January to 15 February 2003). The figure is based on results presented in Table 3.

first indication that, at the macro level, the first wave of Hartz reforms indeed had an effect on the process of matching and job creation.

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 1 January 2003, a second set of reforms became effective on 1 January 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 the potentially distinct effects of this second set of reforms. In this section, we perform an analysis comparable 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 March 2003 until December 2004.[19] 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) show 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,

19. This observation window is chosen to avoid overlap with the pre-Hartz phase. Also note that data reported in March 2003 contain stocks at or inflows since mid-February 2003.

Table 4 The effects of Hartz I/II reforms on the speed of matching by broad occupations – alternative implementation periods

	Logged outflow from unemployment into employment: $\ln m_{it}$						
Dependent variable: Alternative implementation: Reform indicator = 1	− 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	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	0.031	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	0.108**	0.064	0.161***	0.153***	0.220***	0.122***	0.084**
reform indicator	[0.042]	[0.041]	[0.041]	[0.041]	[0.041]	[0.042]	[0.042]
Reform × occupation 2	− 0.045*	− 0.049*	− 0.059**	− 0.055**	− 0.032	0.015	0.051
interaction	[0.026]	[0.026]	[0.027]	[0.028]	[0.029]	[0.030]	[0.031]
Reform × occupation 3	− 0.061**	− 0.070***	− 0.081***	− 0.106***	− 0.097***	− 0.031	0.015
interaction	[0.026]	[0.026]	[0.027]	[0.028]	[0.029]	[0.030]	[0.032]
Reform × occupation 4	− 0.079***	− 0.084***	− 0.103***	− 0.101***	− 0.082***	− 0.038	− 0.013
interaction	[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	1,840	1,840	1,840	1,840	1,840	1,840	1,840
Number of occupation groups	40	40	40	40	40	40	40
R^2 (within)	0.48	0.48	0.49	0.49	0.49	0.48	0.48
R^2 (between)	0.92	0.92	0.92	0.92	0.92	0.92	0.92

Notes: Standard errors are in brackets. The 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.
***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 5 The effects of the Hartz III reform on the speed of matching by broad occupations

	Logged outflow from unemployment into employment: $\ln m_{it}$			
	No broad occupation interactions		Broad occupation interactions	
Dependent variable	(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]
Reform × occupation 2 interaction			−0.064**	−0.065**
			[0.033]	[0.032]
Reform × occupation 3 interaction			−0.060*	−0.059*
			[0.033]	[0.032]
Reform × occupation 4 interaction			−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^2 (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

Notes: Standard errors are in brackets. The 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.
***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

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 of the finding mentioned above that the Hartz reforms apparently also affected the technological structure of the matching process. In particular, the results point to 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 the 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).[20] According to this specification, the second reform wave accelerated the unemployment outflows into employment by more than 10% on average for manufacturing occupations. As with the first reform wave Hartz I/II analyzed before, interactions with broad occupations show that manufacturing occupations benefited 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 that 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

20. Test statistics for joint significance tests are $F(8, 870) = 2.07$ ($p \leq 0.084$) and $F(4, 851) = 2.94$ ($p \leq 0.02$) in columns (3) and (4), respectively.

Table 6 The effects of Hartz III reforms on the speed of matching – alternative implementation periods

	Logged outflow from unemployment into employment: $\ln m_{it}$						
Dependent variable:							
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	−0.019	−0.014	−0.033	−0.033	−0.033	−0.033	−0.033
change	[0.044]	[0.043]	[0.042]	[0.042]	[0.042]	[0.042]	[0.042]
(Pseudo) Hartz reform	0.021	−0.088**	0.168***	0.065*	0.113***	0.023	0.013
indicator	[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^2 (within)	0.57	0.57	0.57	0.57	0.57	0.57	0.57

Notes: Standard errors are in brackets. The 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.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 7 The effects of Hartz III reforms on the speed of matching by broad occupations – alternative implementation periods

	Logged outflow from unemployment into employment: $\ln m_{it}$						
Dependent variable: Alternative implementation: Reform indicator = 1	− 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]
Reform × occupation 2 interaction	−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]
Reform × occupation 3 interaction	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]
Reform × occupation 4 interaction	−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^2 (within)	0.57	0.57	0.57	0.57	0.57	0.57	0.57

Notes: Standard errors are in brackets. The 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.
***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

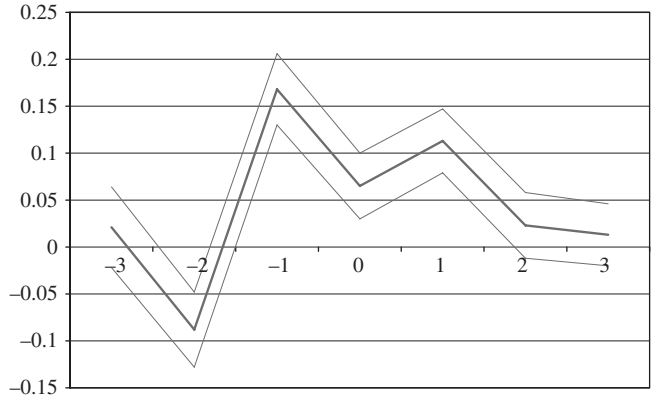

Figure 3 The treatment effect for Hartz III reforms for alternative implementation periods

Notes: The figure shows the coefficient of the Hartz III 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 16 January to 15 February 2004). The figure is based on results presented in Table 6.

illustrates the results from Table 6 graphically. The results indicate a clear hump-shaped 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, on 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 the 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 to 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 1 January 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.

5.3. Robustness: regional disaggregation

In this section, we investigate the robustness of the previous findings by performing the same estimations using data disaggregated by region rather than occupation. This also provides some additional insights about the macroeconomic effects of the Hartz reforms. In order to capture regional

differences at a policy-relevant level while avoiding problems for the consistent estimation of matching functions that could arise from systematic commuting flows and spatial interdependencies of labor markets, we consider labor markets at the level of German federal states (*Bundesländer*).[21] This allows us to estimate stock-flow matching functions using a specification that is comparable to the previous analysis without having to model spatial dependencies explicitly. Previous research has shown that matching processes at the level of a finer definition of regions exhibit patterns with positive and negative spatial dependencies across regions (clusters and hot-spots); see Fahr and Sunde (2006a, 2006b). These spatial dependencies are typically confined within federal states, or at least the broad regions of West Germany, Southern Germany, Eastern Germany and Northern Germany, that we consider in our analysis.[22] Hence, the identification of the parameter of interest in the analysis below should not be affected by these regional aspects.

Table 8 presents estimation results for the effects of the first wave of Hartz reforms, Hartz I/II, on the matching process when using regions as relevant labor markets. The specifications are comparable to those presented for occupational matching functions in Table 1, without and with month fixed effects and interaction terms for broad regions, respectively. The findings concerning the coefficients of the matching technology corroborate previous findings. The matching elasticities with respect to unemployment and vacancy stocks and vacancy inflows are qualitatively and quantitatively virtually identical to those obtained with occupational data. Only the negative effect of unemployment inflows on the outflows from unemployment to employment disappears when using regional data. As before, we find some evidence for increasing returns to scale in the matching function. More positive expectations about the business climate accelerate the matching process. But unlike with the occupation data, we find a significant effect of the change in the measurement on the matching flows.

Turning to the effects of the Hartz reforms, the results presented in columns (1) and (2) reveal little evidence for a significant change in the speed of the matching process as a consequence of the reform changes. Only when considering interactions of the reform indicator with broad regions it appears that the reforms had significantly positive effects.[23] However, these effects seem to have been concentrated in regions in East Germany. Notably, unemployment rates are the highest in East Germany, suggesting that the reforms might have had a significant impact particularly in those regions with the greatest need for an improvement in the matching speed.

Table 9 presents the results for the effects of the Hartz III reforms. Again, we find almost identical coefficients for the coefficients of the matching

21. We thank a referee for suggesting this extension.
22. Table 11 provides an overview of how federal states and regions are defined in our data.
23. Tests reveal joint significance of the reform indicators in the four regions with $F(4, 530) = 3.03$ ($p < 0.018$) and $F(4, 488) = 3.48$ ($p < 0.01$) in columns (3) and (4), respectively.

Table 8 The effects of Hartz I/II reforms on the speed of matching by region

	Logged outflow from unemployment into employment: $\ln m_{it}$			
	No region interactions		Region interactions	
Dependent variable	(1)	(2)	(3)	(4)
$\ln U_{it}$	1.019***	0.963***	1.142***	1.085***
	[0.082]	[0.093]	[0.092]	[0.108]
$\ln V_{it}$	0.118***	0.079***	0.072**	0.024
	[0.028]	[0.030]	[0.031]	[0.033]
$\ln u_{it}$	0.015	−0.059	0.026	−0.046
	[0.049]	[0.053]	[0.051]	[0.056]
$\ln v_{it}$	0.316***	0.395***	0.342***	0.433***
	[0.019]	[0.033]	[0.021]	[0.037]
IFO business expectations index (2000 = 1, R3)	0.003***	−0.001	0.003***	−0.002
	[0.001]	[0.002]	[0.001]	[0.002]
Measurement change indicator	0.056***	0.113***	0.057***	0.130***
	[0.016]	[0.039]	[0.017]	[0.042]
Hartz reform indicator (1 = after 2003.1)	0.019	0.050	−0.022	0.020
	[0.016]	[0.034]	[0.028]	[0.043]
Reform × South interaction			−0.041	−0.038
			[0.031]	[0.030]
Reform × East interaction			0.054**	0.056**
			[0.027]	[0.027]
Reform × North interaction			0.024	0.021
			[0.030]	[0.029]
Constant	−1.070***	−0.573**	−1.143***	−0.607**
	[0.139]	[0.227]	[0.148]	[0.240]
State fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	No	Yes	No	Yes
Observations	598	598	552	552
Number of states	13	13	13	13
R^2 (within)	0.96	0.97	0.96	0.97
Implied overall effects				
Effect of Hartz I/II on region West			−0.022	0.020
Effect of Hartz I/II on region South			−0.063	−0.018
Effect of Hartz I/II on region East			0.032	0.076
Effect of Hartz I/II on region North			0.002	0.041

Notes: Standard errors are in brackets. The 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. The reference region is West.
***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 9 The effects of Hartz III reform on the speed of matching by region

	Logged outflow from unemployment into employment: $\ln m_{it}$			
	No region interactions		Region interactions	
Dependent variable	(1)	(2)	(3)	(4)
$\ln U_{it}$	2.029***	2.003***	2.243***	2.223***
	[0.225]	[0.225]	[0.238]	[0.236]
$\ln V_{it}$	0.170***	0.192***	0.189***	0.215***
	[0.037]	[0.037]	[0.039]	[0.039]
$\ln u_{it}$	0.004	−0.112	0.003	−0.111
	[0.087]	[0.091]	[0.090]	[0.094]
$\ln v_{it}$	0.114***	0.127***	0.135***	0.159***
	[0.029]	[0.031]	[0.031]	[0.033]
IFO business expectations	0.015***	0.011***	0.016***	0.012***
index (2000 = 1, R3)	[0.003]	[0.004]	[0.003]	[0.004]
Control for measurement change	0.01	0.02	0.016	0.023
	[0.025]	[0.041]	[0.026]	[0.043]
Hartz III reform indicator	0.001	−0.014	−0.070**	−0.093**
(1 = after 2004.1)	[0.015]	[0.030]	[0.028]	[0.038]
Reform × South interaction			0.078**	0.075**
			[0.036]	[0.035]
Reform × East interaction			0.113***	0.120***
			[0.031]	[0.030]
Reform × North interaction			0.070*	0.071**
			[0.037]	[0.035]
Constant	−3.122***	−2.674***	−3.466***	−3.066***
	[0.429]	[0.515]	[0.448]	[0.534]
State fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	No	Yes	No	Yes
Observations	299	299	276	276
Number of states	13	13	13	13
R^2 (within)	0.96	0.97	0.96	0.97
Implied overall effects				
Effect of Hartz III on region West			−0.07	−0.093
Effect of Hartz III on region South			0.008	−0.018
Effect of Hartz III on region East			0.043	0.027
Effect of Hartz III on region North			0	−0.022

Notes: Standard errors are in brackets. The 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. The reference region is West. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 10 Occupational groups

Occupation	Occupational code (official classification)	Broad occupation[a]
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
Healthcare-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

[a] 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.

Table 11 *Bundesländer* and regions

	Bundesland	Region[a]
1	North-Rhine Westphalia	1
2	Schleswig-Holstein/Hamburg	4
3	Niedersachsen/Bremen	4
4	Mecklenburg-Vorpommern	3
5	Hessia	1
6	Rheineland-Palatine/Saar	1
7	Baden-Württemberg	2
8	Bavaria	2
9	Brandenburg	3
10	Saxonia	3
11	Saxonia-Anhalt	3
12	Thuringia	3
13	Berlin	3

[a] *Bundesländer* are merged into the following regions: 1, West; 2, South; 3, East; 4, North.

technology, i.e. the matching elasticities with respect to unemployment and vacancy stocks, as well as vacancy inflows, as when using data disaggregated by occupation. There is no effect of unemployment inflows on matches, however, when using regional data. Concerning the effects of the implementation of the Hartz III reforms on the speed of the matching process, the results again reveal no significant aggregate effect as indicated by the estimates in columns (1) and (2). When considering regional differences in the reform effects as in columns (3) and (4), however, we find that the speed of the matching process declined in West Germany, the reference group, after the implementation of the Hartz III reforms. There is no significant overall effect in the South or the North, but a significantly positive effect in the East.[24]

On summing up the results of our robustness analysis using regional data, we obtain very similar estimates for the parameters of the matching technology, suggesting that the dimension of disaggregation does not strongly affect our findings. However, it also seems that the Hartz reforms were particularly effective in speeding up the matching process in East German regions. This is consistent with the fact that unemployment rates are the highest in East Germany compared to all other German regions according to our definition. When interpreting these results in light of the earlier findings on the level of occupations, one needs to keep in mind, however, that the occupation composition differs widely across regions.

24. Again, the reform effects are jointly significant with $F(4, 254) = 3.27$ ($p < 0.013$) and $F(4, 235) = 4.12$ ($p < 0.01$) in columns (3) and (4), respectively.

6. CONCLUSION

This paper makes an 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 benefited less from the reforms in terms of the acceleration of the matching process. Also, the reforms appear to have had a stronger impact on regions in East Germany.

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, 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. Unfortunately, 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 of this reform wave.

Of course, the results obtained within a matching framework, which is based on the notion of frictional unemployment, have to be interpreted accordingly. The findings presented in this paper are primarily concerned with labor market dynamics from a search and matching perspective. While this perspective is in line with the concerns motivating the Hartz reforms, it has little to say about the effects of the reform in light of structural unemployment. Moreover, as the identification of reform effects heavily relies on the quality of the data used, one has to be cautious when drawing conclusions for the effectiveness of particular policy instruments from the results presented in this paper. Nevertheless, by applying the matching approach to the evaluation of policy reforms on the macroeconomic level, we believe that this paper presents a potentially very useful tool, and therefore constitutes an important first step to complement micro-based evaluation studies of the Hartz reforms.

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