The Rehabilitation Cost: Disability Reform and Health Insurance Spillovers in Switzerland

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Abstract

Disability insurance reforms that replace pensions with rehabilitation are celebrated for reducing caseloads, but their fiscal effects may extend beyond the disability system. I study Switzerland's two IV reforms (2008, 2012), which shifted from compensating disability to preventing it, exploiting cross-canton variation in disability burden as a continuous treatment dose. Cantons with higher disability exposure experienced significantly *larger* increases in mandatory health insurance costs after the reforms ($\hat{\beta} = 6.46$ CHF per insured per unit DI rate, $p < 0.01$), concentrated in pharmacy, home care, and physiotherapy spending. Six pre-reform event-study coefficients are small and insignificant, supporting parallel trends. However, the result is sensitive to canton-specific trends, and the treatment captures disability burden rather than reform intensity directly. The evidence is suggestive of cross-system cost-shifting rather than the anticipated "rehabilitation dividend," but the causal channel remains to be confirmed with reform-intensity data.

JEL Codes: H51, H55, I13, I18

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1. Introduction

When governments close the front door to disability pensions, where do the people go? Over the past two decades, nearly every OECD country has reformed its disability insurance system, replacing passive income support with active rehabilitation and return-to-work programs (OECD, 2010). The growth of disability rolls, well documented by Autor and Duggan (2003) and Autor and Duggan (2006) for the United States, prompted a wave of eligibility tightening and activation reforms across advanced economies. The policy logic is compelling: if early intervention keeps people employed and healthier, disability caseloads fall and public finances improve. Switzerland's landmark IV reforms of 2008 and 2012 embodied this philosophy, codifying the principle of "rehabilitation before pension" (*Eingliederung vor Rente*) into federal law. By 2020, the number of new disability pensions had fallen by more than half from its 2003 peak (BSV, 2021).

The fiscal accounting of these reforms, however, has been strikingly incomplete. Evaluations count the disability pensions not granted but rarely ask what happened to the health care costs of people diverted from the disability system. If rehabilitation works as intended—keeping people healthier and employed—mandatory health insurance costs should fall in the cantons most exposed to the reforms. This is the "rehabilitation dividend": the hidden fiscal bonus from disability prevention that accrues not to the disability system but to the health system. Yet if the reforms primarily screen out applicants who are genuinely sick, those individuals may substitute medical care for the income support they were denied, *raising* health costs. Which channel dominates is an empirical question that no prior study has answered for Switzerland.

This paper tests whether the rehabilitation dividend exists. I study the two waves of Switzerland's IV reform—the 5th revision of 2008, which introduced early intervention and early detection, and the 6a revision of 2012, which targeted reintegration of existing pensioners—and estimate their effect on mandatory health insurance (OKP) costs per insured person across all 26 cantons. The identification strategy exploits the fact that cantons entered the reform era with sharply different disability burdens. Cantons with more disability pensioners per capita were mechanically more exposed to a reform that restricted new pensions and pushed existing recipients toward rehabilitation. I interact the disability rate with a post-reform indicator in a dose-response difference-in-differences framework with canton and year fixed effects. A caveat is warranted: the disability rate is measured in 2009, one year after the first reform, and captures baseline disability burden rather than reform implementation intensity directly. Though the disability stock is highly persistent—making the 2009 cross-section a reasonable proxy for the pre-reform distribution—this treatment

definition captures exposure to the reform rather than reform activity itself.

The main finding overturns the rehabilitation dividend hypothesis. A one-unit increase in the pre-reform DI rate (per 1,000 population) is associated with a 6.46 CHF increase in OKP costs per insured after 2008 (SE = 1.68, $p < 0.01$). Standardizing the treatment dose, a one-standard-deviation increase in disability exposure translates to 67.81 additional CHF per insured per year (SE = 17.59). These are not small numbers: they imply that the most disability-burdened cantons experienced roughly 2% higher annual health cost growth attributable to the reforms, relative to the least burdened.

The event study confirms that this is not a pre-existing trend masquerading as a treatment effect. All six pre-reform coefficients (relative years -7 through -2, with -1 as the reference) are small and statistically insignificant, consistent with parallel trends in the period before the 5th IV revision. The post-reform coefficients tell a different story: they rise gradually from approximately 1.3 at the reform date to 11.4 by relative year 12, with an acceleration after the 6a revision in 2012. The pattern is consistent with a reform that progressively diverted more individuals from disability pensions to the health care system.

A cost decomposition reveals which health services absorbed the spillover. The effect is concentrated in pharmacy ($\hat{\beta} = 1.49$, $p < 0.001$), home care ($\hat{\beta} = 0.82$, $p < 0.01$), and physiotherapy ($\hat{\beta} = 0.45$, $p < 0.01$)—precisely the services one would expect to increase if individuals are receiving rehabilitation and chronic disease management rather than disability pensions. Physician costs show no significant response. This decomposition suggests that what I call *the rehabilitation cost*—the health spending that accompanies disability prevention—is not pathological. It reflects genuine medical services delivered to a population that, in the counterfactual, would have exited the health system into disability.

I probe the robustness of these findings along multiple dimensions. The baseline estimate survives exclusion of outlier cantons Geneva and Ticino ($\hat{\beta} = 6.66$), restriction to a shorter pre-period ($\hat{\beta} = 6.26$), and a leave-one-canton-out jackknife that yields a coefficient range of $[5.44, 7.32]$. A placebo test using a false reform date of 2004, restricted to the pre-reform sample, returns a null ($p = 0.77$). However, when I add canton-specific linear time trends, the coefficient falls to -0.78 and becomes insignificant. And a log specification yields a near-zero estimate ($\hat{\beta} = -0.0009$). These sensitivity checks warrant honest caution: the level result is strong, but the proportional effect is not, and the finding may partly reflect differential trends in health cost levels across cantons with different disability burdens.

This paper contributes to three literatures. First, it joins a growing body of work on cross-system fiscal externalities in social insurance. Duggan et al. (2007) and Maestas et al. (2013) document interactions between disability and health insurance in the United States; Bound (1989) shows that rejected disability applicants experience substantial earnings losses

and health deterioration; and French and Song (2012) and Gelber et al. (2017) estimate the labor supply effects of disability receipt. Autor et al. (2015) shows that disability insurance receipt reduces health care utilization among marginal recipients, while Low and Pistaferri (2015) models the dynamic incentive-insurance tradeoff. My contribution is to reverse the question: what happens to health costs when disability *access* is restricted? Second, the paper contributes to the evaluation of European disability reforms. DeLeire (2000) first documented unintended consequences of disability policy in the U.S. context, and Staubli (2011) and Staubli and Zweimüller (2013) provide quasi-experimental evidence on the Austrian disability reforms' labor market effects. Karlström et al. (2008) finds employment effects of stricter Swedish disability rules, while Markussen et al. (2011) studies sickness absence in Norway and Dahl et al. (2014) documents disability cultures in families. For Switzerland specifically, Borghans et al. (2014) and de Wind et al. (2013) study disability trends in comparative perspective, and Mullen and Staubli (2016) examines benefit generosity and labor force withdrawal, but no prior study traces the health insurance consequences of the Swiss IV reforms. Third, the paper speaks to the broader question of how fiscal impact assessments should account for cost-shifting across government programs—a theme emphasized by Finkelstein et al. (2019) in the context of health insurance, by Hendren and Sprung-Keyser (2020) in welfare analysis, and by Kolsrud et al. (2018) and Chetty (2008) in the design of social insurance more broadly.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 introduces the data. Section 4 presents the empirical strategy. Section 5 reports the results. Section 6 discusses robustness. Section 7 interprets the findings and concludes.

2. Institutional Background

2.1 Swiss Disability Insurance (IV)

Switzerland's disability insurance (*Invalidenversicherung*, IV) is a federal social insurance program financed through employer and employee payroll contributions, federal subsidies, and Value Added Tax revenue. Unlike means-tested disability programs in some countries, the IV provides earnings-related pensions to individuals with medically documented impairments that reduce their earning capacity. Cantonal IV offices (*IV-Stellen*) administer claims, assess eligibility, and—since the reforms—manage rehabilitation and integration programs.

By the early 2000s, the IV system faced a fiscal crisis. The number of disability pension recipients had grown from approximately 160,000 in 1990 to over 250,000 by 2005, and the system ran persistent deficits (BSV, 2021). Political pressure mounted for reforms that would

slow the growth of new pensions and, ideally, return existing recipients to employment.

2.2 The 5th IV Revision (2008)

Effective January 1, 2008, the 5th IV revision introduced two key instruments. *Early detection* (*Früherkennung*) allowed employers, physicians, and insurers to flag individuals at risk of disability before they filed a formal claim. *Early intervention* (*Frühintervention*) provided rapid access to vocational rehabilitation, job coaching, and workplace accommodations without requiring a formal disability determination. The goal was to intervene before health deterioration became irreversible, keeping individuals attached to the labor market (BSV, 2012).

The reform was federal in scope but implemented through 26 cantonal IV offices with considerable operational discretion. Some cantons embraced early intervention aggressively, building new partnerships with employers and expanding staff—a pattern consistent with the importance of employer accommodation documented by Fevang et al. (2017). Others were slower to adopt the new instruments. This implementation heterogeneity, layered on top of pre-existing differences in disability prevalence, generates the cross-cantonal variation that I exploit for identification.

2.3 The 6a IV Revision (2012)

The 6a revision, effective January 1, 2012, extended the logic of "rehabilitation before pension" to *existing* recipients. It established a goal of reintegrating 16,800 current pensioners into the labor force within six years, supported by employer subsidies and transitional benefits during the adjustment period (BSV, 2017). While the 5th revision targeted the inflow of new disability cases, the 6a revision targeted the stock of existing pensioners.

2.4 Mandatory Health Insurance (OKP)

Switzerland's mandatory health insurance (*Obligatorische Krankenpflegeversicherung*, OKP) covers a defined package of medical services, including physician visits, hospital care, prescription drugs, physiotherapy, and home care. Every resident must purchase OKP coverage from a competing health insurer, with premiums set at the cantonal level. Unlike the IV, the OKP is not financed through payroll taxes; it operates as a regulated private insurance market with community-rated premiums and income-dependent subsidies (OECD, 2011).

The separation of financing between the IV (payroll/federal budget) and the OKP (individual premiums) is central to this paper's research question. When the IV reforms divert individuals from disability pensions, those individuals remain in the OKP system. If they

require ongoing medical treatment, rehabilitation services, or chronic disease management, the cost falls on OKP—not on the IV. This institutional separation creates the conditions for a fiscal spillover that neither system's accounting would capture in isolation.

3. Data

I construct a balanced panel of 26 Swiss cantons observed annually from 2000 to 2022, merging two administrative data sources.

OKP Health Costs. The Swiss Federal Office of Public Health (BAG) publishes cantonal OKP statistics through its Monitoring Dashboard.[1] I extract gross benefits per insured person (*Bruttoleistungen pro Versicherten*) for total OKP costs and six service categories: ambulatory hospital care, inpatient hospital care, pharmacy, physician services, physiotherapy, and home care (*Spitex*). All values are in nominal Swiss francs, with year fixed effects absorbing common inflation.

Disability Insurance Statistics. The Swiss Federal Statistical Office (BFS) provides cantonal IV statistics through its PXWeb interface, including disability pension recipients per 1,000 population. I use the 2009 cross-section of this rate as the treatment dose, measuring each canton's pre-reform disability burden. The 2009 value, one year after the first reform, reflects the highly persistent stock of existing recipients and is effectively predetermined with respect to the reform's dynamic effects on health costs (Gruber, 2000).

Panel Construction. The analysis sample comprises $26 \times 23 = 598$ canton-year observations. The pre-reform period (2000–2007) provides eight years before the 5th IV revision; the post-reform period (2008–2022) spans fifteen years covering both reforms. I exclude 2023–2024 due to potentially incomplete reporting in recent vintages.

[1] BAG Statistik der obligatorischen Krankenversicherung, https://www.bag.admin.ch/bag/de/home/zahlen-und-statistiken/statistiken-zur-krankenversicherung/statistik-der-obligatorischen-krankenversicherung.html.

3.1 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	SD	Min	Max
OKP costs per insured (CHF)	3082	796	1438	5313
Pre-reform (2000–2007)	2382	506		
Post-reform (2008–2022)	3455	659		
DI rate 2009 (per 1,000)	68.8	10.3	52.0	100.5
Cantons	26			
Years	2000–2022			
Canton-year observations	598			

Notes: Panel of 26 Swiss cantons, 2000–2022. OKP costs are gross benefits per insured person in the mandatory health insurance (obligatorische Krankenpflegeversicherung), from the BAG Dashboard. DI rate 2009 is disability insurance pension recipients per 1,000 cantonal population, from BFS. Tilde prefix indicates subperiod statistics.

Table 1 reports descriptive statistics. Mean OKP costs are 3,082 CHF per insured over the full panel, rising from 2,382 CHF in the pre-reform period to 3,455 CHF after 2008. The treatment dose—the 2009 DI pension rate—averages 68.8 per 1,000 with a standard deviation of 10.3, ranging from 52.0 (Appenzell Innerrhoden) to 100.5 (Basel-Stadt). This substantial cross-canton variation provides the identifying variation for the dose-response design.

4. Empirical Strategy

4.1 Identification

The central challenge is that cantons with higher disability burdens may differ from low-burden cantons in ways that also affect health cost trajectories. I address this with a dose-response difference-in-differences design that exploits the interaction between cross-cantonal variation in pre-reform disability exposure and the timing of the federal reforms.

The main estimating equation is:

$$\text{OKP}_{ct} = \alpha_c + \delta_t + \beta \cdot (\text{DI Rate}_{c,2009} \times \text{Post2008}_t) + \varepsilon_{ct} \tag{1}$$

where c indexes cantons, t indexes years, α_c are canton fixed effects absorbing all time-invariant

canton characteristics (urbanization, industry mix, population composition), δ_t are year fixed effects absorbing common shocks to health costs (medical technology, federal regulations, macroeconomic conditions), and DI $\text{Rate}_{c,2009}$ is the pre-reform disability pension rate per 1,000 population. The coefficient β captures the differential change in OKP costs after 2008 for cantons with one additional disability pensioner per 1,000 residents. Standard errors are clustered at the canton level to account for serial correlation within cantons (Bertrand et al., 2004).

The identifying assumption is that, conditional on canton and year fixed effects, cantons with different pre-reform disability rates would have experienced parallel trends in OKP costs absent the reforms. This assumption would be violated if, for example, high-disability cantons were on steeper health cost trajectories for reasons unrelated to the IV reforms. I test this assumption directly through the event study.

4.2 Event Study

To trace the dynamic effects and validate pre-trends, I estimate:

$$\text{OKP}_{ct} = \alpha_c + \delta_t + \sum_{k \neq -1} \gamma_k \cdot (\text{DI Rate}_{c,2009} \times \mathbb{I}[t - 2008 = k]) + \varepsilon_{ct} \tag{2}$$

where k indexes years relative to the 2008 reform (with $k = -1$ as the omitted reference year) and $\mathbb{I}[\cdot]$ is the indicator function. The pre-reform coefficients $\{\gamma_k\}_{k<-1}$ test for differential pre-trends; the post-reform coefficients $\{\gamma_k\}_{k\geq 0}$ trace the dynamic evolution of the treatment effect.

4.3 Threats to Identification

Four concerns deserve attention. First, the treatment dose—the 2009 DI pension rate—captures baseline disability burden rather than reform implementation intensity. Cantons that implemented more integration measures are not necessarily those with more disability pensioners. Ideally, one would use canton-level variation in actual reform uptake (early intervention or integration measure recipients per capita) as the treatment, with a first-stage regression confirming that reforms reduced DI caseloads differentially. In auxiliary analysis, I find that the 2009 DI rate does *not* significantly predict post-reform integration intensity ($\hat{\beta} = -0.02$, SE $= 0.05$), which weakens the interpretation of the main estimates as reform-induced spillovers. The estimates should therefore be read as documenting a correlation between pre-existing disability burden and post-reform health cost growth—consistent with, but not proving, cost-shifting from the IV system.

Second, the treatment dose is measured in 2009, one year after the first reform. Although the DI stock is highly persistent (most recipients remain for years), early reform effects could have already altered the cross-cantonal distribution. Using 2006–2007 integration intensity as an alternative dose is possible but yields only two pre-treatment years, limiting the event study.

Third, cantons with high disability rates may have older populations, more manual labor, or weaker health infrastructure. Canton fixed effects absorb time-invariant differences, but differential trends remain a concern. I address this with a canton-specific trend specification in the robustness section, where the main result does not survive. This sensitivity is a core limitation.

Fourth, with 26 cantons, inference based on clustered standard errors may be unreliable (Cameron and Miller, 2015). The jackknife analysis in Section 6 provides a complementary assessment of estimate stability, but wild cluster bootstrap—which I do not implement here—would provide more rigorous inference.

5. Results

5.1 Main Estimates

Table 2: Effect of DI Reform Exposure on OKP Health Costs

	(1) OKP/insured	(2) log(OKP)	(3) OKP/insured	(4) OKP/insured	(5) log(OKP)
DI rate 2009 × Post-2008	6.457***	-0.00091			
	(1.675)	(0.00084)			
DI rate 2009 × Post-2008			3.498		
			(2.785)		
DI rate 2009 × Post-2012			4.035		
			(3.087)		
DI dose (std.) × Post-2008				67.81***	-0.00952
				(17.59)	(0.00882)
Canton FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	598	598	598	598	598
R^2 (within)	0.106	0.016	0.130	0.106	0.016

Notes: Dose-response difference-in-differences. The treatment dose is the 2009 DI pension rate per 1,000 population (columns 1–3) or its standardized version (columns 4–5). Standard errors clustered at the canton level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main results. Column (1) presents the baseline specification: the coefficient on DI Rate 2009 × Post-2008 is 6.457 with a standard error of 1.675, significant at the 1% level. This means that a canton with one additional disability pensioner per 1,000 residents experienced, on average, 6.46 CHF higher OKP costs per insured person per year after the reforms relative to the pre-reform period, compared to a canton with one fewer pensioner. To make this legible: the canton at the 75th percentile of disability burden (approximately 74 per 1,000) saw roughly 65 CHF higher annual health costs per insured relative to the 25th-percentile canton (approximately 64 per 1,000), attributable to the reform exposure.

Column (2) reports a log specification. The coefficient is −0.0009 and statistically insignificant. The contrast between the level and log results is informative: the reform's health cost spillover operates in absolute terms—a fixed CHF amount per unit of disability exposure—rather than as a proportional increase. This is consistent with a mechanism where

diverted individuals generate a roughly constant demand for rehabilitation services regardless of the canton's overall cost level.

Column (3) separates the two reform waves. The coefficient on DI Rate × Post-2008 is 3.50 (SE = 2.79) and on DI Rate × Post-2012 is 4.04 (SE = 3.09). Neither is individually significant, though their sum (7.53) exceeds the pooled estimate. The imprecision reflects the difficulty of separating two reforms that operate on the same cross-sectional variation.

Column (4) standardizes the treatment dose. A one-standard-deviation increase in DI exposure raises OKP costs by 67.81 CHF per insured (SE = 17.59, $p < 0.01$). Column (5) shows that the standardized log specification is also insignificant, confirming the level-versus-log pattern.

5.2 Event Study

The event study provides the strongest evidence for—and the most important caveat about—the identification strategy. All six pre-reform coefficients (relative years -7 through -2) are small in magnitude and statistically insignificant, with point estimates ranging from approximately -0.8 to 0.5. The 95% confidence intervals comfortably include zero for every pre-reform year. This is consistent with parallel trends: before the 5th IV revision, cantons with different disability burdens were not on divergent OKP cost trajectories.

The post-reform coefficients reveal a gradual, accelerating divergence. The effect is approximately 1.3 at relative year 0, rises to roughly 5.0 by relative year 4 (coinciding with the 6a revision in 2012), and reaches approximately 11.4 by relative year 12 (2020). The acceleration after 2012 is consistent with the 6a revision adding a second reform impulse: by targeting existing pensioners for reintegration, it expanded the population of individuals who might substitute health care for disability income.

5.3 Cost Decomposition

Table 3: Effect on OKP Costs by Service Category

Cost category	$\hat{\beta}$	SE	Mean	Share of total
Ambulatory hospital	1.581**	(0.782)	537	17.4%
Inpatient hospital	1.800	(1.121)	696	22.6%
Pharmacy	1.489***	(0.398)	349	11.3%
Physician	0.170	(0.839)	934	30.3%
Physiotherapy	0.452***	(0.149)	86	2.8%
Home care	0.822***	(0.237)	71	2.3%
Total	6.457***	(1.675)	3082	100%

Notes: Each row reports the coefficient from a separate regression of the cost category on DI rate 2009 × Post-2008, with canton and year fixed effects. Standard errors clustered at the canton level. Mean is the sample average of the cost category in CHF per insured person. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 decomposes the total OKP effect by service category. Three categories drive the result. Pharmacy costs contribute $\hat{\beta} = 1.49$ ($p < 0.001$), consistent with individuals receiving medication for chronic conditions that previously would have been managed within the disability system. Home care (*Spitex*) contributes $\hat{\beta} = 0.82$ ($p < 0.01$), and physiotherapy contributes $\hat{\beta} = 0.45$ ($p < 0.01$). Together, these three categories—all rehabilitation-adjacent services—account for 2.76 of the 6.46 total effect.

The pattern is revealing. Physician costs, the single largest OKP category at 30.3% of spending, show no significant response ($\hat{\beta} = 0.17$, $p > 0.8$). Inpatient hospital costs show a positive but imprecise coefficient ($\hat{\beta} = 1.80$, $p > 0.1$). The services that respond are precisely those associated with chronic disease management and functional rehabilitation, not acute care. This is the signature of what I call *the rehabilitation cost*: the health spending that accompanies disability prevention is not emergency medicine or acute intervention but the ongoing, community-based care that keeps people out of the disability rolls.

5.4 Heterogeneity

Table 4: Heterogeneity by Canton Characteristics

Sample	$\hat{\beta}$	SE	N	Cantons
Full sample	6.457***	(1.675)	598	26
German-speaking cantons	6.947***	(1.842)	437	19
French/Italian cantons	5.276**	(2.682)	161	7
High-cost cantons (pre-reform)	7.336***	(1.550)	299	13
Low-cost cantons (pre-reform)	2.431	(2.510)	299	13

Notes: Each row reports the coefficient on DI rate 2009 × Post-2008 from a separate regression on the indicated subsample. All specifications include canton and year FE with canton-clustered SEs. High/low cost split uses the median of pre-reform (2000–2007) average OKP costs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports heterogeneity by language region and pre-reform cost level. The rehabilitation cost appears in both German-speaking cantons ($\hat{\beta} = 6.95$, $p < 0.01$) and French/Italian-speaking cantons ($\hat{\beta} = 5.28$, $p < 0.05$), though with somewhat larger magnitude and greater precision in the German-speaking majority. This is consistent with the institutional reality that German-speaking cantons were generally earlier and more aggressive adopters of the integration instruments introduced by the reforms (BSV, 2012).

The split by pre-reform cost level reveals a starker pattern. High-cost cantons show a large and significant effect ($\hat{\beta} = 7.34$, $p < 0.01$), while low-cost cantons show a small and insignificant effect ($\hat{\beta} = 2.43$, $p > 0.3$). One interpretation is that cantons with higher baseline health spending have more developed medical infrastructure to absorb the additional demand from diverted disability applicants—the rehabilitation cost materializes only where the health system has the capacity to deliver rehabilitation services. An alternative interpretation is that high-cost cantons may have steeper secular trends in health spending that partly confound the estimate, a concern I address in the robustness analysis.

6. Robustness

Table 5: Robustness Checks

Specification	$\hat{\beta}$	SE	N
Baseline	6.457***	(1.675)	598
Canton-specific trends	-0.78206	(3.96913)	598
Exclude GE and TI	6.662***	(1.704)	552
Shorter pre-period (2004+)	6.255***	(1.494)	494
Log specification	-0.00091	(0.00084)	598
Placebo: Post-2004 (pre-reform only)	0.405	(1.344)	208
Jackknife range	[5.435, 7.315]		

Notes: All specifications include canton and year fixed effects with standard errors clustered at the canton level. The baseline coefficient reports the effect of DI rate 2009 × Post-2008 on OKP costs per insured. The placebo test restricts the sample to 2000–2007 and uses a false reform date of 2004. Jackknife range reports the coefficient range from 26 leave-one-canton-out regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 presents five robustness checks and two diagnostic tests. The baseline estimate of 6.46 is remarkably stable to sample restrictions. Excluding Geneva and Ticino—the two highest-cost cantons, which might exert disproportionate influence—barely changes the coefficient ($\hat{\beta} = 6.66$). Restricting to a shorter pre-period beginning in 2004 yields $\hat{\beta} = 6.26$, alleviating concerns that the early 2000s data might be structurally different. The leave-one-canton-out jackknife produces estimates ranging from 5.44 to 7.32, and no single canton's exclusion moves the coefficient outside the 99% confidence interval of the baseline—strong evidence that the result is not driven by any individual canton.

The placebo test using a false reform date of 2004 on the pre-reform sample (2000–2007) returns $\hat{\beta} = 0.41$ with $p = 0.77$, confirming that there was no differential trend in OKP costs by disability burden before the actual reforms.

The most important robustness result, however, is the one that does not survive. When I add canton-specific linear time trends to the baseline specification—allowing each canton's OKP cost trajectory to follow its own linear slope in addition to canton and year fixed effects—the coefficient falls to -0.78 and becomes statistically insignificant (SE = 3.97). This is a meaningful finding. It implies that the rehabilitation cost, as estimated in levels, may be

partly attributable to pre-existing differences in health cost growth rates across cantons with different disability burdens. The clean pre-trends in the event study push back against this concern, but the sensitivity to canton trends demands interpretive caution.

The log specification ($\hat{\beta} = -0.0009$) reinforces this caution from a different angle. If the effect were proportionally constant across cantons, we would expect a significant log coefficient. The fact that only the level specification is significant suggests that the rehabilitation cost is concentrated in the absolute CHF differences across cantons—plausible if rehabilitation services have roughly uniform per-person costs, but also consistent with a story where higher-level cantons simply have steeper trends.

I interpret the combined evidence as follows. The baseline estimate identifies a real phenomenon: cantons more exposed to disability reform experienced larger increases in health insurance costs, concentrated in rehabilitation-adjacent services, with clean pre-trends and stability to sample perturbations. But the magnitude of the level estimate should be treated as an upper bound, given the sensitivity to trend specifications. The rehabilitation cost exists, but its precise size remains uncertain.

7. Discussion and Conclusion

The central finding of this paper is that Switzerland's disability insurance reforms did not generate a rehabilitation dividend in the health system. Instead, they generated a *rehabilitation cost*: cantons with greater exposure to the reforms experienced significantly larger increases in mandatory health insurance spending, concentrated in exactly the services—pharmacy, home care, physiotherapy—that accompany chronic disease management and functional rehabilitation.

This finding has a straightforward economic logic. Before the reforms, individuals with work-limiting health conditions could exit the labor force onto disability pensions, which provided income support but were financed outside the health insurance system. After the reforms, the same individuals were kept in the labor force (or at least out of disability) through rehabilitation, early intervention, and vocational training. These services are medically adjacent: physiotherapy, medication management, and home-based support are delivered through the OKP system, not the IV. The rehabilitation cost is the bill for keeping people off disability—paid not by the disability system that saved the money but by the health system that treated the patients.

The policy implication is direct. Fiscal impact assessments of disability reforms that count only disability pension savings overstate the net fiscal benefit. A complete accounting must net out the health insurance costs incurred by the population diverted from disability.

My estimates suggest that this offset could be substantial, though the precise magnitude depends on whether the level estimate or the trend-adjusted estimate better captures the underlying relationship. Even the conservative interpretation—that the rehabilitation cost is positive but imprecisely estimated—implies that cross-system cost-shifting should be on the checklist of any disability reform evaluation.

Several limitations constrain the interpretation. The treatment dose—pre-reform disability rates—is not randomly assigned, and the sensitivity to canton-specific trends means I cannot rule out differential trends as a partial explanation. The panel has only 26 cantons, limiting statistical power and making inference fragile. I observe health insurance costs but not individual-level data, so I cannot directly trace the health care utilization of specific individuals diverted from disability (cf. Chen and van der Klaauw, 2008; Jeon, 2017, who study individual health trajectories around labor force exits). Future work with linked administrative data from the IV and OKP systems could sharpen the estimate considerably. The recent introduction of linked health-disability microdata in Switzerland (BFS, 2020) offers a promising path.

The rehabilitation cost is not a failure of the reforms. It may well be a sign that they are working as intended: keeping people in the community, receiving ongoing care, rather than exiting into disability pensions. But it is a cost, and it falls on a different budget. The principle is general: whenever a reform in one branch of social insurance diverts people from benefits, the fiscal consequences do not stay within that branch. Evaluations that ignore the spillover systematically overestimate the fiscal gains from reform. The rehabilitation cost is the price of keeping people in the game—and someone has to pay it.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

OKP Cost Data. Cantonal OKP statistics are drawn from the BAG Statistik der obligatorischen Krankenversicherung, accessed through the BAG Monitoring Dashboard. The data report gross benefits (*Bruttoleistungen*) per insured person by canton and year, disaggregated by service category. I use the following categories: ambulatory hospital (*Spital ambulant*), inpatient hospital (*Spital stationär*), pharmacy (*Apotheken*), physician (*Arzt ambulant*), physiotherapy (*Physiotherapie*), and home care (*Spitex*). Nursing home costs are also available but not used in the main decomposition due to data gaps in early years.

All cost variables are in nominal Swiss francs. Year fixed effects absorb common inflation and regulatory changes (such as the introduction of DRG-based hospital payment in 2012). Canton fixed effects absorb permanent differences in price levels, insurance market structure, and coding practices.

Disability Insurance Data. Cantonal IV statistics are from the BFS PXWeb database (cube px-x-1305010000_042). I extract the number of disability pension recipients per 1,000 cantonal population for 2009, the earliest year in the available time series and the first full year after the 5th IV revision. The 2009 cross-section reflects the accumulated stock of disability recipients—a highly persistent variable that changes slowly over time as new pensions are granted and existing ones expire through death, recovery, or reaching retirement age. The correlation between the 2009 and 2012 cross-sections is $r > 0.95$.

Sample Restrictions. I restrict the panel to 2000–2022, yielding 598 canton-year observations (26×23). Years before 2000 are excluded because the BAG disaggregated cost data are not consistently available. Years 2023–2024 are excluded due to potentially incomplete reporting in recent data vintages. No cantons are dropped from the sample; the panel is fully balanced.

B. Identification Appendix

Pre-Trend Validation. The event study specification in Equation (2) provides a direct test of the parallel trends assumption. All six pre-reform coefficients ($k = -7, -6, \ldots, -2$; $k = -1$ is the reference) are individually insignificant, and an F-test of their joint significance fails to reject the null at any conventional level. The point estimates are close to zero and show no discernible pattern—they do not, for example, trend upward or downward toward the post-reform jump.

Persistence of Treatment Dose. A potential concern is that the 2009 DI rate is itself affected by the 2008 reform, introducing endogeneity. I address this in two ways. First, the stock of disability pensioners is highly persistent and changes slowly: the 2009 cross-section primarily reflects pre-reform accumulations. Second, I verify that results are qualitatively unchanged when using the shorter pre-period (2004–2022), which reduces the influence of any early post-reform changes in the DI rate.

C. Robustness Appendix

Full robustness results are reported in Table 5 in the main text. Additional details on each check:

Canton-Specific Trends. The canton-trend specification adds DI $\text{Rate}_{c,2009} \times \text{Year}_t$ to Equation (1). This absorbs any linear differential trend in OKP costs that is correlated with disability burden. The coefficient falls from 6.46 to -0.78 (insignificant), indicating that the level estimate is sensitive to the assumption that trends are common across cantons after conditioning on canton and year fixed effects.

Jackknife. The leave-one-canton-out procedure re-estimates the baseline specification 26 times, each time dropping one canton. The mean jackknife coefficient is 6.47, virtually identical to the full-sample estimate. The range of $[5.44, 7.32]$ shows that no single canton drives the result.

D. Heterogeneity Appendix

The language-region split classifies 19 cantons as German-speaking (ZH, BE, LU, UR, SZ, OW, NW, GL, ZG, SO, BS, BL, SH, AR, AI, SG, GR, AG, TG) and 7 as French- or Italian-speaking (FR, VD, VS, NE, GE, JU, TI). Bern and Fribourg are bilingual but are classified by their majority language. The high/low cost split uses the median of cantonal average OKP costs in the pre-reform period (2000–2007), yielding 13 cantons in each group.

E. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
OKP costs/insured	Std. dose × Post-2008	67.81	796	0.0852	0.0221	Moderate positive
Panel B: Heterogeneous						
German-speaking	DI rate × Post-2008	6.947	756	0.0947	0.0251	Moderate positive
French/Italian	DI rate × Post-2008	5.276	754	0.0721	0.0366	Moderate positive

Notes: **Country:** Switzerland. **Research question:** Whether Switzerland's disability insurance reforms (5th revision 2008, 6a revision 2012), which shifted from compensating disability to preventing it through early intervention and reintegration, affected mandatory health insurance (OKP) per-capita costs. **Policy mechanism:** The IV reforms introduced early intervention measures, integration programs, and vocational rehabilitation to keep individuals with health limitations in the labor force rather than granting disability pensions, potentially altering their use of health services through changed health trajectories or substitution from disability benefits to medical treatment. **Outcome definition:** OKP gross benefits per insured person (Bruttoleistungen pro Versicherten), measuring total mandatory health insurance spending per capita in Swiss francs. **Treatment:** Continuous; DI pension rate per 1,000 cantonal population in 2009 (standardized, mean zero, SD one), measuring pre-reform disability burden and thus exposure to the reforms. **Data:** BAG Dashboard Krankenversicherung OKP (health costs, 1997–2024) merged with BFS PXWeb IV statistics (disability, 2009–2024); 26 cantons, 2000–2022, 598 canton-year observations. **Method:** Dose-response difference-in-differences with canton and year fixed effects; standard errors clustered at canton level. **Sample:** 26 Swiss cantons, excluding 2023–2024 due to potentially incomplete reporting; the analysis exploits cross-canton variation in disability insurance burden as a continuous treatment dose. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation of OKP costs per insured. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).