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DO HEALTH-FINANCING REGIME SHIFTS MATTER FOR VULNERABLE EMPLOYMENT? EVIDENCE FROM A COUNTRY–YEAR STATE CLUSTERING APPROACH

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ABSTRACT. Rising out-of-pocket pressures and renewed attention to financial protection in health systems have intensified interest in how health-financing architecture may shape labour-market vulnerability. This article aims to investigate whether shifts in health-financing regimes, captured as data-driven country-year "states" of the financing mix, are associated with changes in vulnerable employment. An unbalanced annual panel merges OECD health-financing components (government, social insurance, private insurance, voluntary insurance, NPISH, enterprise financing, out-of-pocket and cost-sharing; % of GDP) with World Bank controls (GDP per capita; employment-to-population ratio) and the modelled ILO estimate of vulnerable employment; country–year observations are clustered on financing indicators (k-means) and linked to vulnerable employment using two-way fixed-effects models with country-clustered standard errors, complemented by switchers-only and entry/exit specifications. Relative to the modal social-insurance-dominant state (Cluster 5), the compulsory-private-insurance/high household-burden state (Cluster 6) is associated with higher vulnerable employment ($\beta = 5.802$, $p < 0.001$), a relationship that remains significant among switchers ($\beta = 4.244$, $p < 0.01$). Predicted vulnerable employment equals 21.77 in Cluster 5 versus 27.57 in Cluster 6 (baseline), rising to 34.77 for Cluster 6 when key channels (x3, x7, x8) are added. Entry into Cluster 6 is associated with a short-run decline ($\beta = -1.024$, $p < 0.01$), suggesting that transition dynamics may differ from long-run regime-level differences.

JEL Classification: I13, I18, J21, C38, C23

Keywords: health-financing regimes, vulnerable employment, out-of-pocket payments, cost-sharing, compulsory private insurance, k-means clustering, two-way fixed effects

Introduction

Health-financing reforms have re-entered policy agendas because financial protection remains fragile even where service coverage is relatively high. Recent global monitoring emphasises that progress towards Universal Health Coverage is inseparable from the way systems are funded, because out-of-pocket payments continue to generate financial hardship and can undermine access, equity and household welfare (World Bank & WHO, 2025). At the same time, early, WHO (2024a) has highlighted that many governments have not prioritised health spending sufficiently, and that substantial out-of-pocket shares persist even in high-income settings, which increases the risk that health shocks translate into broader socio-economic vulnerability.

The European context makes the issue especially visible: WHO/Europe reports that up to one in five households can face catastrophic spending due to health-care costs, driven mainly by out-of-pocket payments (notably medicines), which forces trade-offs with other essential needs (WHO, 2024b). In parallel, the European Commission's "State of Health in the EU" synthesis reports emphasise financial protection and the policy relevance of reducing out-of-pocket burdens through health-financing reforms, reflecting a renewed focus on resilience and inclusiveness of social systems (European Commission, Directorate-General for Health and Food Safety, 2023). OECD (2025) reporting likewise underlines that household health spending remains a non-trivial budget pressure in many countries, reinforcing the policy salience of how costs are shared between public schemes, insurance and households.

Against this background, the labour-market dimension is increasingly important. The World Bank's "vulnerable employment" indicator captures own-account and contributing family workers – forms of work that typically feature weaker income security and lower access to social protection (World Bank, n.d.). Because health-payment burdens and coverage gaps can push households to adjust labour supply, accept precarious work, or remain in informal arrangements to cope with costs, shifts in health-financing regimes (rather than spending levels alone) may plausibly matter for the prevalence of vulnerable employment. This makes it timely to move beyond static comparisons and test whether regime changes in the financing mix, between government schemes, social insurance, private insurance, enterprise schemes and direct household payments, are systematically associated with changes in labour-market vulnerability.

1. Literature review

Health-financing architecture has become more salient because recent overlapping shocks have exposed how quickly economic volatility can translate into fiscal stress, changes in relative prices, and pressures on social systems. Evidence on the macro-financial transmission of the COVID-19 crisis and the russia–Ukraine war underscores that external shocks can simultaneously affect commodity prices, exchange-rate dynamics, and inflationary conditions, thereby constraining policy space for social spending and public services (Aliu et al., 2024; Strouhal et al., 2024; Kuzior et al., 2024; Slyusarevskyy & Chunikhina, 2025). Health-system performance has also been shown to interact with crisis volatility, reinforcing the need to understand not only spending levels but also how financing responsibilities are distributed across schemes (Gheorghe & Panazan, 2024; Vysochyna et al., 2023).

A growing body of work links government expenditure patterns and institutional capacity to socio-economic outcomes, providing a foundation for analysing how health-financing regimes may co-evolve with labour-market vulnerability. Public spending dynamics have been examined through Wagner-type relationships, determinants of government expenditures, and the macroeconomic consequences of expenditure composition, including inflationary effects and growth linkages (Al-Qalawi, 2024; Filipova et al., 2025; Hamadouche, 2024; Hamadouche et al., 2024; Omerbegovic Arapovic & Kurtovic, 2024). Beyond aggregate fiscal relationships, evidence highlights the role of governance quality and public spending effectiveness in social development, including the connection between human development and social spending, as well as the risks of state capture undermining public service delivery (Artyukhov et al., 2024; Liashenko et al., 2024; Audu & Paul, 2025). This stream suggests that financing regimes should be viewed as institutional configurations, rather than merely accounting categories. Broader work on socio-economic welfare governance, including the role of digital and AI-enabled public governance models, further supports treating financing and welfare arrangements as part of an evolving institutional system rather than a static structure (Murko et al., 2024).

Research focusing directly on health insurance and system design provides further motivation for a regime-based approach. Insurance models have been linked to population health outcomes and broader welfare, including reductions in disease burden across OECD settings and associations with subjective well-being (Kuzior et al., 2025a; Mureșan et al., 2023). At the implementation level, attention has shifted to the operational and behavioural foundations of insurance functioning, claims management decision factors, willingness-to-pay for community-based insurance, and organisational trust, implying that formal scheme labels may mask substantial heterogeneity in how financing translates into access and risk protection (Al-Kahtani et al., 2025; Otieno et al., 2025; Gavurova et al., 2024). Reform-focused evidence suggests that health-financing arrangements can undergo significant changes across crisis and post-crisis periods, with policy trade-offs emerging regarding sustainability, access, and institutional capacity. (Alotaiby & Krenyácz, 2024). Complementary proof on the insurance sector's risk-profitability trade-offs further underscores that insurance-based regimes embed incentive and solvency constraints that may influence the design of cost-sharing and household exposure (Kamel & Naoual, 2024).

Household exposure to health costs is a central channel through which financing regimes can plausibly affect labour-market vulnerability. Health shocks have been shown to aggravate poverty risks, reduce resilience, and intensify difficulties in meeting unexpected expenses, indicating that the burden of direct payments may trigger coping strategies with labour-market consequences (Chhetri & Adhikari, 2025; Dudek & Landmesser, 2025; Flores et al., 2024; Iwu et al., 2023). Empirical evidence also links out-of-pocket health expenditures to real-economy outcomes, illustrating that health-payment burdens can influence production decisions and household economic behaviour beyond the health sector (Ukpe & Mohamadou, 2024). At the same time, disease cost profiles and risk-factor burdens, such as the direct costs of cancer, highlight that high-cost conditions can generate persistent financial pressure, making the distribution of financing across schemes consequential for both households and labour markets (Herțeliu et al., 2025). Broader determinants of environmental health and longevity, including non-linear and spatial influences, also reinforce the idea that multiple, interacting systems jointly shape health and socio-economic vulnerability (Dinu et al., 2024; Milon et al., 2024).

An emphasis on the interaction between health-system quality, labour productivity, and inclusive development strengthens the link between vulnerable employment and work. Vulnerable employment has been linked to productivity dynamics and public health quality in comparative European evidence, suggesting that labour-market vulnerability can be both a determinant and an outcome of social system performance. (Lyeonov et al., 2025). Inclusive growth research highlights that heterogeneity across countries often clusters into distinct development profiles, which supports moving from single-indicator explanations to typologies capturing joint configurations (Saher et al., 2025). A related policy framework is provided by SDG-oriented research, which emphasises the interdependence between economic growth, employment quality, and sustainability, as well as the role of social entrepreneurship and institutional arrangements in promoting health and decent work (Raman et al., 2025; Imo & Ekechukwu, 2024; Oe et al., 2025). Evidence from sectoral policy programmes and public–private interaction further indicates that institutional design and incentive structures matter for socio-economic outcomes, reinforcing interest in regime shifts rather than static levels (Ray, 2024; Topazly et al., 2025; Burrell et al., 2025; Prokop et al., 2024; Ruiz Molina et al., 2025). Concerns about the sustainability of other welfare-state pillars, including pension systems, also emphasise that financing design and demographic–institutional constraints shape the resilience of social protection in ways that can influence labour-market vulnerability (Otavová et al., 2025).

Methodologically, the literature is increasingly applying classification and data-driven methods to study complex institutional and welfare systems, providing a precedent for a country–year state clustering approach. Multivariate classification has been used to contrast governance and social development, while cluster-based insights have been employed to characterise inclusiveness profiles across EU countries (Liashenko et al., 2024; Saher et al., 2025). In adjacent domains, efficiency and performance have been studied using DEA/SFA and advanced modelling strategies, highlighting the value of combining structural measurement with empirical inference (Yu et al., 2024; Dinu et al., 2024; Orlova & Ovander, 2019). Within healthcare, technology integration and acceptance research indicates that system performance depends on organisational capacity and adoption drivers, which can condition how financing arrangements operate in practice and how reforms translate into household burden (Smolanka et al., 2024; Rudawska et al., 2024). Digital platforms facilitating solidarity and resource-sharing practices further illustrate how socio-technical arrangements can support resilience and access, complementing formal institutional schemes and shaping how households respond to shocks. (Guilliams et al., 2024). Reform experiences, including pre- and post-COVID health system adjustments, highlight the practical relevance of understanding how financing mixes evolve, rather than treating financing structures as fixed country traits (Alotaiby & Krenyácz, 2024; Megbowon & Zehirun, 2025; Kuzior et al., 2025b).

Prior work suggests that financing structures, institutional capacity, household exposure to health costs, and labour-market vulnerability are linked through multiple channels. Still, the empirical literature remains comparatively thin on whether changes in the financing mix are systematically associated with within-country changes in vulnerable employment. The present study addresses this gap by operationalising health-financing regimes as time-varying country–year states and testing their association with labour-market vulnerability in a panel setting. This contributes to a more policy-relevant understanding of whether regime shifts, in addition to spending levels, align with more inclusive labour market outcomes.

The aim of this article is to identify distinct country-year health-financing regimes and to quantify how within-country shifts between these regimes are associated with changes in vulnerable employment, controlling for GDP per capita and overall employment intensity. This analysis clarifies which health-financing configurations are most consistently linked to lower labour-market vulnerability.

2. Methodological approach

Data and variables

The empirical analysis relies on an annual, unbalanced country–year panel assembled by merging health-financing aggregates from the OECD Data Explorer (OECD, n.d.) with macro-labour controls from the World Bank Open Data (World Bank, n.d.). Eight financing indicators capture the composition of health-system funding as percentages of GDP: government schemes (x1), social health insurance schemes (x2), compulsory private insurance schemes (x3), voluntary health insurance schemes (x4), NPISH financing schemes (x5), enterprise financing schemes (x6), out-of-pocket spending excluding cost-sharing (x7), and cost-sharing with third-party payers (x8). Two control variables are included to account for development level and labour-market conditions: GDP per capita (in constant 2015 US dollars) (x9) and the employment-to-population ratio (15+, total %) (x10). The outcome variable is vulnerable employment, total (% of total employment) (modelled ILO estimate) (y1). The OECD (n.d.) and World Bank (n.d.) series were harmonised by country and year; because coverage differs across indicators and countries, the merged dataset is unbalanced. Observations with missing values in the clustering variables (x1–x8) were excluded from the clustering stage, and standard panel-data handling of missingness was used in the regression stage (listwise deletion within each specification).

Construction of time-varying financing "states"

Countries were classified into time-varying financing states by clustering country–year observations on the eight financing indicators (x1–x8). Before clustering, each indicator was standardised (z-scores) to ensure that variables measured in the same units (% of GDP) do not dominate the partition due to dispersion differences and to facilitate the interpretation of cluster centroids. K-means clustering (using Euclidean distance) was then applied to the standardised matrix, producing a categorical state variable. $cluster_{it}$for each country $i$and year t. The choice of the number of clusters k was guided by complementary diagnostics (e.g., silhouette, elbow/WSS, gap statistic, and multi-index rules such as NbClust). Because these criteria can point to different levels of granularity, a parsimonious solution ($k = 2$) can be treated as a baseline regime split, while $k = 6$ provides a more informative main specification that balances interpretability with non-trivial heterogeneity in financing mixes.

Econometric strategy: linking financing states to vulnerable employment

To evaluate how financing regimes relate to labour-market vulnerability, the analysis estimates two-way fixed-effects models of the form:

$$y1_{it} = \sum_{k \neq k_0} \beta_k \cdot \mathbb{1}(cluster_{it} = k) + \gamma_1 x9_{it} + \gamma_2 x10_{it} + \alpha_i + \lambda_t + \varepsilon_{it},$$

where $\alpha_i$are country fixed effects (absorbing time-invariant country characteristics such as institutions and long-run structure) and $\lambda_t$are year fixed effects (absorbing common shocks). Standard errors are clustered by country to account for serial correlation within each country. Because cluster membership is highly persistent in many countries, identification in the fixed-effects framework is driven primarily by the subset of within-country state transitions, and coefficients should be interpreted as associations linked to rare regime shifts rather than frequent annual movements across states.

Several robustness and interpretation extensions can be used within the same framework. First, lagged-state specifications replace $cluster_{it}$with $cluster_{i,t-1}$to reduce simultaneity concerns. Second, "switchers-only" estimations restrict the sample to countries that change state at least once, directly assessing sensitivity to the limited number of transitions. Third, transition indicators (e.g., $enter6_{it}$, $exit6_{it}$) capture short-run changes around entry into or exit from specific states; importantly, level differences between stable regimes (state dummies) need not match short-run changes at the entry moment, especially when transitions are rare and may occur during atypical adjustment periods. Finally, a mechanism check augments the baseline state model by including key financing components (e.g., x3, x7, x8) alongside state dummies to test whether the state effects are reducible to specific channels or reflect broader financing configurations.

3. Conducting research and results

The average silhouette coefficients reported in Figure 1 reinforce the visual inference that clustering quality improves with higher granularity. The coefficient increases from 0.232 at $k = 2$to 0.301 at $k = 5$, indicating a marked enhancement in cohesion and separation as additional states are introduced. The improvement remains substantial between $k = 5$ and $k = 6$(0.301 to 0.331), and then becomes more incremental thereafter, reaching 0.345 at $k = 7$. This sequence implies that a non-trivial share of heterogeneity across country–year observations is captured once the solution moves beyond very coarse partitions, with the strongest gains concentrated in the mid-range of the tested values. k values.

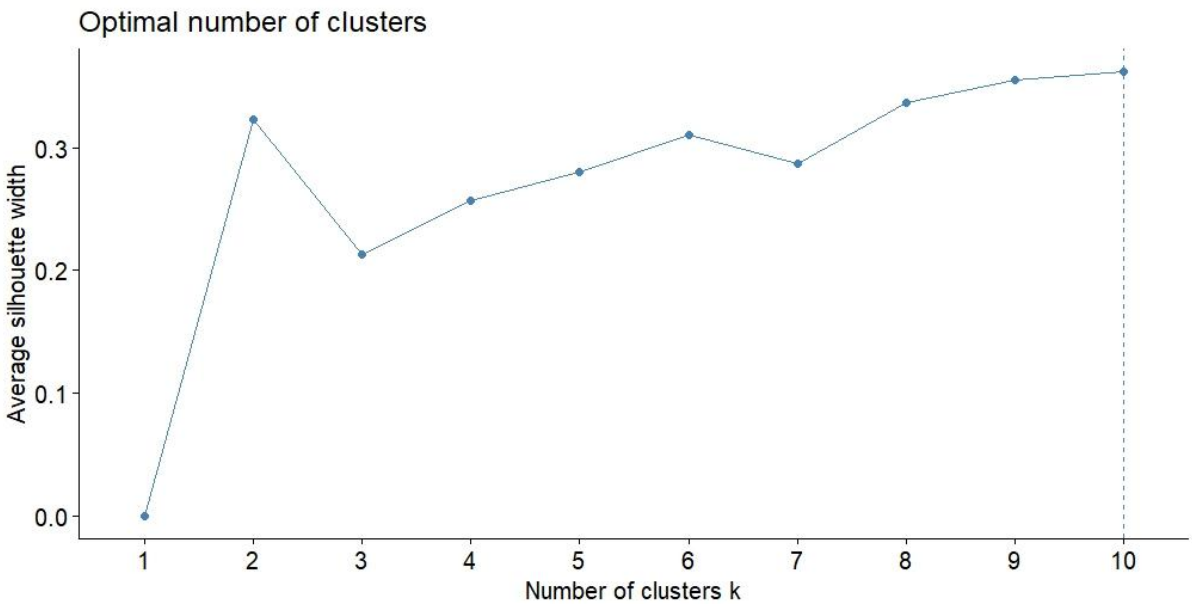

Figure 1. Average silhouette coefficients for selecting the optimal number of clusters (k = 2–10)
Source: authors' calculations in R Studio.

Beyond this point, the pattern suggests diminishing returns and mild non-monotonicity. The coefficient dips at $k = 8$ (0.336) before recovering at $k = 9$ (0.347) and attaining the highest value at $k = 10$(0.365). Such behaviour is typical when increasingly fine partitions begin to split previously coherent groups, improving separation for some observations but worsening it for others. Accordingly, the tabulated coefficients support a parsimonious choice in the region of $k = 6$–7 for the main specification, as this range secures most of the achievable increase in average silhouette while limiting the risk of over-fragmentation into small, less interpretable states; higher $k$values can be retained as robustness checks to confirm that the particular level of clustering granularity does not drive subsequent panel estimates.

Figures 2–4 show that the preferred number of "states" depends on whether emphasis is placed on fit, parsimony, or stability. The elbow plot in Figure 2 indicates a pronounced reduction in within-cluster dispersion as k increases from 1 to roughly 4–5, after which the curve flattens, and additional clusters yield progressively smaller improvements. This pattern is consistent with a situation in which a limited number of broad regimes capture most of the systematic heterogeneity in the eight financing indicators. In contrast, higher values of k primarily provide finer segmentation rather than a step change in explanatory structure. The minor irregularity around $k = 8$ (a slight increase in WSS relative to $k = 7$) is compatible with the known sensitivity of k-means to initialisation and does not alter the overall "diminishing returns" profile.

The gap statistic in Figure 3 rises steadily across the tested range. It reaches its maximum at $k = 10$, implying that progressively finer partitions continue to outperform a reference null distribution in terms of dispersion reduction. However, the near-monotonic increase also signals that the method, under a pure "maximum gap" rule, may favour the upper boundary of the search grid when the data contain gradual gradients rather than sharply separated groups. In such cases, the practical choice often relies on a balance between the gap pattern and other diagnostics, including interpretability and cluster sizes. Figure 4 provides complementary graphical evidence via the D-index: the second-difference curve exhibits its most prominent positive peak around $k \approx 5$, indicating a marked "knee" where the marginal gain from adding cluster changes most strongly, which supports an intermediate solution rather than the maximum-k outcome.

The multi-index synthesis from NbClust reinforces this ambiguity. While the majority rule points to $k = 2$, the votes are dispersed, with several criteria selecting 5, 7, 8, or 10, which is typical when the underlying structure combines a coarse regime split with additional sub-regimes. Importantly, the stability check strongly favours smaller k: the agreement metric declines from 0.492 at $k = 2$ to 0.203–0.204 at $k = 5$–6, and falls further thereafter, suggesting that fine-grained partitions are increasingly sensitive to k-means initialisation and therefore less reproducible. Taken together, Figures 2–4 and the stability evidence justify treating $k = 2$ as a highly stable "baseline regime" specification. In contrast, an intermediate solution around $k = 6$ can be defended as the main "state" specification that preserves interpretability and captures additional heterogeneity, with $k = 10$ reserved for robustness checks rather than the primary classification.

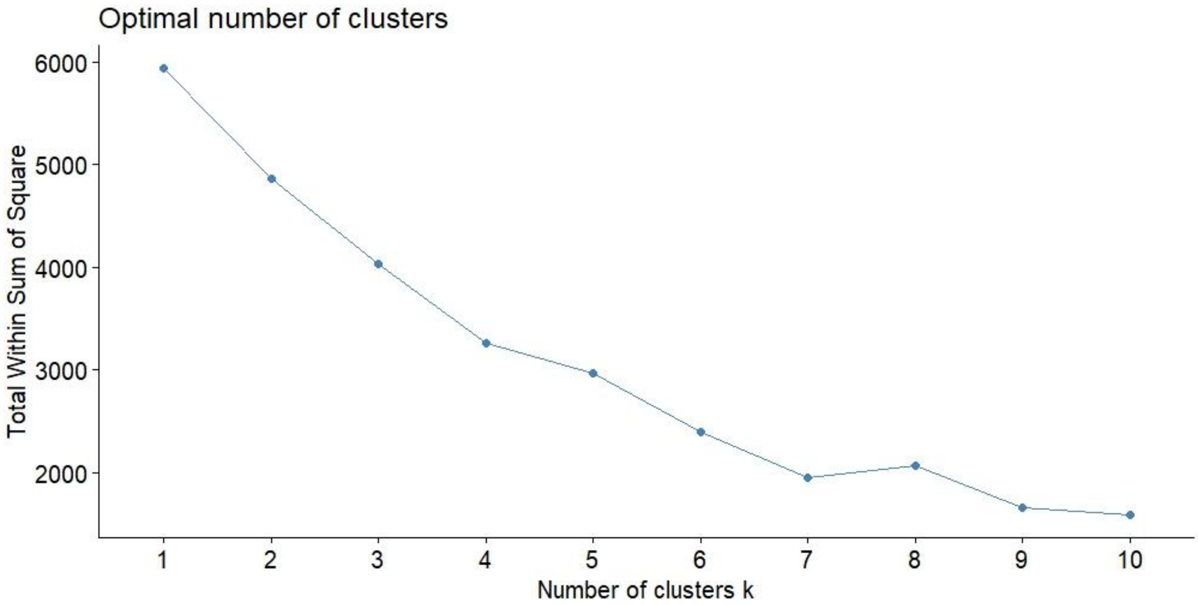

Figure 2. Elbow criterion for selecting the number of clusters: total within-cluster sum of squares (k = 1–10)
Source: authors' calculations in R Studio.

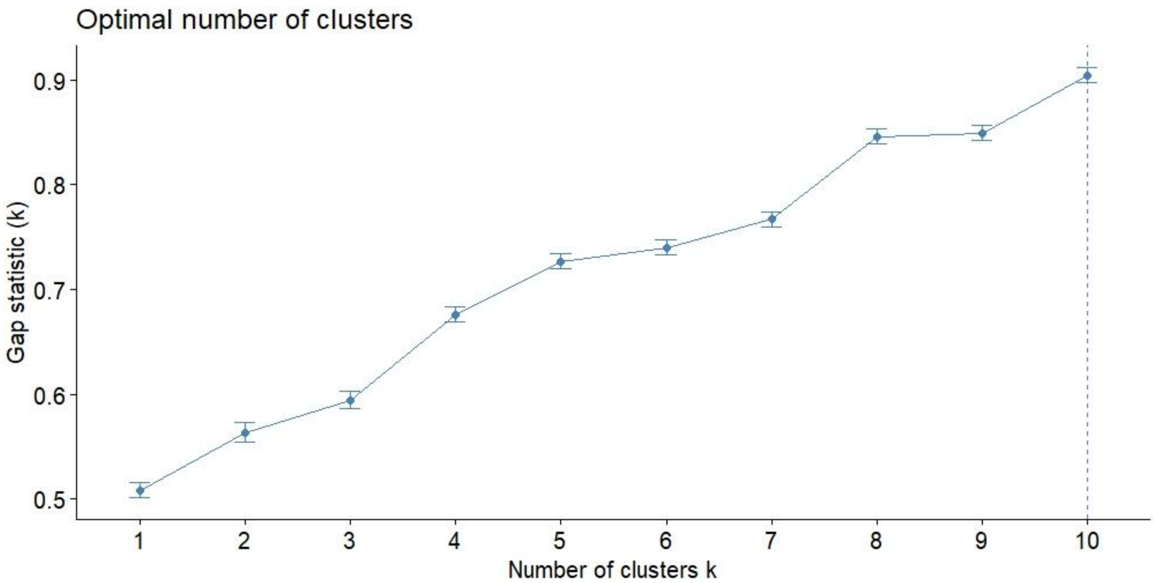

Figure 3. Gap statistic for selecting the number of clusters (k = 1–10)
Source: authors' calculations in R Studio.

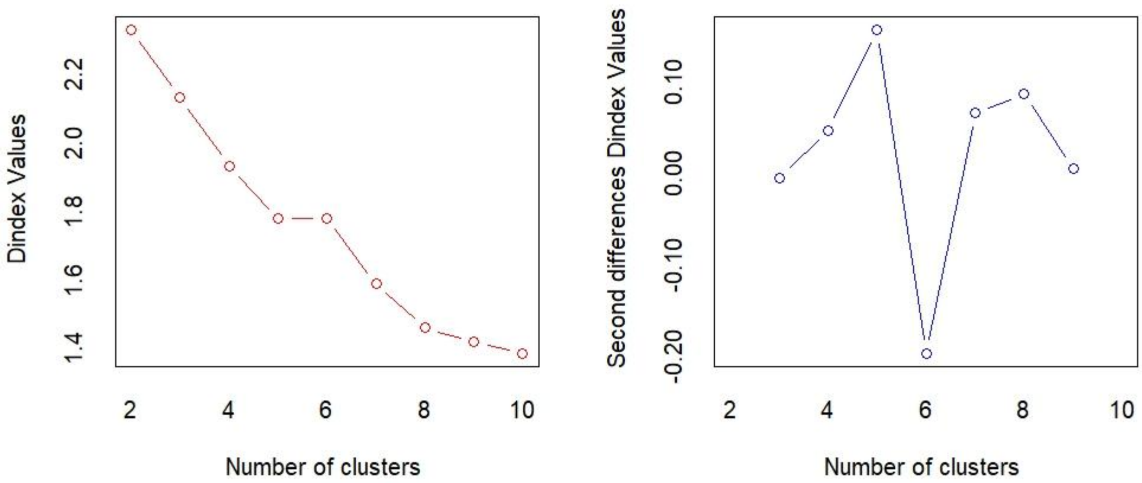

Figure 4. D-index diagnostics for determining the number of clusters: D-index values and second differences (k = 2–10)
Source: authors' calculations in R Studio.

Given that NbClust majority voting favours a parsimonious split ($k = 2$) while the gap statistic and silhouette values continue to improve at higher k but stability declines, the analysis adopts $k = 2$as a baseline regime partition, $k = 6$as the main time-varying 'state' specification to balance interpretability with meaningful heterogeneity, and uses higher-k solutions only for robustness. Country–year 'states' were obtained using k-means clustering on the standardised financing indicators $x1$–$x8$. Euclidean distance was used implicitly by k-means, and the algorithm was initialised with multiple random starts (nstart = 50) to mitigate sensitivity to initial centroids. Observations with missing values in the clustering variables were excluded from the clustering step, and the resulting cluster membership was then merged back to the unbalanced panel for subsequent fixed-effects estimation.

The six-state solution yields an uneven but workable partition of the country–year observations. The largest state (Cluster 5) accounts for 286 observations, representing 38.5% of the sample, suggesting that a sizeable share of country–years share a broadly similar configuration across the eight financing indicators. Two additional states (Clusters 3 and 4) also capture substantial mass, comprising 21.8% (162 observations) and 19.8% (147 observations), respectively. Together, these three clusters cover roughly four-fifths of the panel, indicating that the dominant variation in financing "states" can be represented by a small set of recurrent regimes, with the remaining clusters capturing more specific, less frequent configurations.

The remaining states are comparatively small but not trivial: Cluster 1 includes 57 observations (7.7%), Cluster 6 contains 52 observations (7.0%), and Cluster 2 is the smallest with 39 observations (5.3%). None of the clusters falls into a "rare" category that would typically undermine interpretability or subsequent inference. However, Cluster 2 merits closer scrutiny because smaller groups can be more sensitive to k-means initialisation and may reflect transitional or idiosyncratic country–year patterns. The size distribution supports the feasibility of using $k = 6$ as the main time-varying classification.

The outputs (Table 1) report the cluster profiles (centroids) for the six "state" clusters, expressed as mean standardised values (z-scores) of the eight financing indicators (x1–x8). Positive values indicate that, within the standardisation scheme used, the cluster is characterised by above-average intensity of the corresponding financing channel, whereas negative values indicate below-average intensity. The size structure shows that Cluster 5 is the dominant state (38.5% of observations), followed by Clusters 3 and 4 (approximately 22% and 20%), while Clusters 1, 2, and 6 represent more specialised configurations (each accounting for about 5–8%).

Substantively, the profiles suggest clear financing "regimes". Cluster 1 is distinguished by markedly elevated cost-sharing (x8) and higher out-of-pocket excluding cost-sharing (x7), combined with below-average values for most pooled financing channels, which is consistent with an OOP/cost-sharing-intensive state. Cluster 2 is dominated by exceptionally high voluntary insurance (x4) together with high NPISH financing (x5). In contrast, Cluster 6 is driven by extremely high compulsory private insurance (x3) alongside elevated OOP and cost-sharing, indicating a strongly private-insurance-oriented state. Cluster 3 exhibits high government schemes (x1) with broadly lower values across other channels, consistent with a government-led state. In contrast, Cluster 4 stands out due to high enterprise financing (x6), reflecting an employer/enterprise-led configuration. Cluster 5 combines higher social health insurance (x2) with moderately higher OOP (x7), indicating a broadly social-insurance-dominant state with a complementary household contribution.

Table 1 describes the six time-varying financing "states" in terms of standardised (z-score) deviations of the eight financing channels. Positive values indicate above-average intensity of a channel in that state, while negative values indicate below-average intensity. The profiles reveal that the regimes are not random mixtures but are anchored by one or two strongly defining channels, which supports substantive labelling and interpretation.

Cluster 1 is characterised by a very strong concentration in household contributions, with exceptionally high cost-sharing (x8 = 2.79) and elevated out-of-pocket spending excluding cost-sharing (x7 = 0.93), alongside low NPISH financing (x5 = −0.85) and lower enterprise contributions (x6 = −0.43). This pattern is consistent with a "cost-sharing/OOP-intensive" state. Cluster 2 stands out as an extreme "voluntary insurance and third-sector" configuration, driven by very high voluntary health insurance (x4 = 3.38) and above-average NPISH financing (x5 = 1.53), while social health insurance (x2 = −0.88), enterprise financing (x6 = −0.71), and household channels (x7, x8) are below average.

Table 1. Cluster profiles (mean standardised values of x1–x8)

Variables		Cluster					
		1	2	3	4	5	6
n_observation		57	39	162	147	286	52
n_countries		3	4	14	8	20	3
Government schemes, Percentage of GDP	x1	-0,82	0,32	1,58	-0,17	-0,65	-0,22
Social health insurance schemes, Percentage of GDP	x2	0,82	-0,88	-0,89	-0,67	0,84	-0,2
Compulsory private insurance schemes, Percentage of GDP	x3	-0,01	-0,32	-0,31	-0,3	-0,26	3,51
Voluntary health insurance schemes, Percentage of GDP	x4	-0,2	3,38	-0,08	-0,46	-0,16	0,11
NPISH financing schemes, Percentage of GDP	x5	-0,85	1,53	0,39	-0,48	0,04	-0,33
Enterprise financing schemes, Percentage of GDP	x6	-0,43	-0,71	-0,33	1,33	-0,4	0,47
Out-of-pocket, excluding cost-sharing, Percentage of GDP	x7	0,93	-0,59	-0,29	-0,6	0,22	0,81
Cost-sharing with third-party payers, Percentage of GDP	x8	2,79	-0,47	-0,36	-0,46	-0,24	1,04

Source: authors' calculations in R Studio.

Clusters 3–5 represent the dominant mainstream regimes in the panel. Cluster 3 reflects a “government-scheme dominant” state, with high government financing (x1 = 1.58) and modestly positive NPISH contributions (x5 = 0.39), whereas most other channels are below average. Cluster 4 is clearly “enterprise-financed”, distinguished by high enterprise financing (x6 = 1.33) and below-average levels across insurance and household channels. Cluster 5, the modal state, is “social health insurance dominant”, with above-average social health insurance (x2 = 0.84) and only mild positive out-of-pocket contributions (x7 = 0.22), while the remaining channels are slightly below average.

Cluster 6 captures a distinctly private-mandatory configuration: compulsory private insurance is exceptionally high (x3 = 3.51), and this state is also associated with relatively high household burdens (x7 = 0.81; x8 = 1.04) and moderately elevated enterprise financing (x6 = 0.47). Taken together, the centroid structure indicates that the clustering identifies interpretable “states” centred on (i) household cost-sharing/OOP, (ii) voluntary insurance/NPISH, (iii) government schemes, (iv) enterprise financing, (v) social insurance, and (vi) compulsory private insurance with substantial household contributions–providing a coherent basis for subsequent fixed-effects analysis of how within-country shifts between these financing regimes relate to changes in vulnerable employment.

The switching diagnostics (Table 2 and Table A1, Appendix A) indicate that the estimated “state” membership is highly persistent over time. For the vast majority of countries, the number of observed switches is zero, and the distribution summary confirms this: the median switching rate equals 0.000, and even the third quartile remains 0.000, meaning at least 75% of countries never change cluster during the observed years. The mean switching rate is only 0.014, which implies that, on average, cluster transitions are rare events rather than a common dynamic pattern in the panel.

Switching is concentrated in a small subset of countries, typically involving one transition over the whole period, with only one case exhibiting two changes (Argentina). The highest rate is observed for Latvia (0.167), but this value is mechanically inflated by the short series length (7 years), where a single switch translates into a large per-interval rate. Overall, the evidence suggests that the clustering primarily captures stable financing regimes. Therefore, any two-way fixed-effects estimates that use time-varying cluster membership will be identified mainly from a limited number of within-country transitions. This is informative, but it should be interpreted as the effect of rare regime shifts rather than the frequent year-to-year movement across states.

Table 2. Cluster switching intensity by country (countries with at least one switch)

ountry	n_years	n_switches	switch_rate
Argentina	17	2	0.125
Brazil	20	1	0.053
Canada	18	1	0.059
Finland	24	1	0.043
Ireland	13	1	0.083
Latvia	7	1	0.167
Netherlands	24	1	0.043
Ukraine	22	1	0.048
All countries (N = 44), summary	–	–	Min 0.000; Median 0.000; Mean 0.014; Max 0.167

Source: authors’ calculations in R Studio.

The switching diagnostics indicate that cluster membership is highly persistent, with most countries exhibiting no within-sample transitions and only a small subset contributing to switching events. Consequently, the two-way fixed-effects estimates using time-varying state indicators are primarily identified from the limited number of within-country regime changes rather than from cross-sectional differences between countries. The estimated coefficients should therefore be interpreted as associations linked to rare financing-regime shifts within countries over time, conditional on country- and year-fixed effects, as well as the control variables.

The transitions confirm that cluster membership behaves as a largely persistent "state" variable. Nearly all year-to-year movements remain on the diagonal, indicating that once a country–year observation is classified into a given financing regime, the subsequent observation for the same country is overwhelmingly likely to remain in the same regime. Diagonal counts are high across all six clusters (1→1 = 54; 2→2 = 35; 3→3 = 148; 4→4 = 139; 5→5 = 265; 6→6 = 49), implying strong within-country stability in the joint configuration of the eight financing indicators.

Off-diagonal transitions are rare and concentrated in a few specific pathways, amounting to only a handful of cases. Cluster 3 shows occasional movement to Cluster 2 (3→2 = 2) and Cluster 4 (3→4 = 2), while Cluster 4 records a single transition to Cluster 3 (4→3 = 1). Cluster 5 exhibits very limited switching to Clusters 2, 3 and 6 (each occurring once), and Cluster 2 records one transition to Cluster 5 (2→5 = 1). No transitions into or out of Cluster 1 and Cluster 6 are observed aside from remaining in the same state, suggesting that these regimes are either structurally distinct or only occupied by countries with particularly stable financing structures over the observed horizon. The transition matrix indicates that the subsequent two-way fixed-effects estimates, which utilise time-varying cluster membership, will be primarily identified by a small set of within-country regime changes rather than widespread annual reclassifications.

The normalised transition matrix provides direct evidence of exceptionally strong state persistence in the country–year classifications. All six clusters exhibit diagonal probabilities close to one, implying that once a country is observed in a given financing regime, it is overwhelmingly likely to remain in that same regime in the next year. Full persistence is observed for Clusters 1 and 6, where the estimated transition probability of remaining in the same cluster equals 1.000, and no off-diagonal movements are recorded. Similarly high persistence is found for Cluster 4 (0.993), Cluster 5 (0.989), Cluster 3 (0.974) and Cluster 2 (0.972), indicating that year-to-year reclassification is the exception rather than the rule.

When switching occurs, it follows a small number of low-probability pathways. Cluster 2 exhibits a 0.028 probability of transitioning to Cluster 5, indicating that the rare transitions from this state tend to converge toward the dominant regime. Cluster 3 has symmetric low-probability movement to Clusters 2 and 4 (each 0.013), consistent with occasional shifts toward neighbouring configurations rather than abrupt regime breaks. Cluster 4 exhibits only a 0.007 probability of moving to Cluster 3, while Cluster 5 displays extremely small probabilities of moving to Clusters 2, 3 or 6 (each 0.004). Overall, these probabilities imply that the panel analysis with time-varying cluster membership will primarily capture the effects associated with rare regime transitions, and the estimated coefficients should be interpreted accordingly as reflecting changes linked to infrequent structural shifts rather than continuous annual movement between states.

The two-way fixed-effects estimates (Table 3) indicate that changes in the country–year financing "state" (defined by clustering x1–x8) are systematically associated with vulnerable employment ($y1$), after controlling for unobserved time-invariant country characteristics and common year shocks. Relative to the reference financing state (Cluster 1), observations classified into Clusters 2–5 exhibit significantly lower shares of vulnerable employment. The largest contemporaneous difference is associated with Cluster 2 (β = −7.146, SE = 1.175, $p < 0.001$), followed by Cluster 5 (β = −5.802, SE = 1.030, $p < 0.001$) and Cluster 3 (β = −5.288, SE = 1.574, $p < 0.01$). Cluster 4 also shows a negative association (β = −3.600, SE = 1.614, $p < 0.05$). Interpreted in percentage-point terms, these coefficients suggest that, within the same country, being observed in these alternative financing states rather than Cluster 1 corresponds to a 3.6–7.1 percentage-point lower share of vulnerable employment, conditional on controls and fixed effects.

The control variables–GDP per capita (constant 2015 US$) $(x9)$and the employment-to-population ratio (15+, total %) $(x10)$–do not exhibit statistically significant associations with vulnerable employment within this two-way FE framework (x9 ≈ −0.0001; x10 ≈ 0.12). This implies that, once country and year fixed effects are included, the remaining within-country variation in income levels and overall employment intensity is not sufficient to explain changes in vulnerable employment over time in this sample. In contrast, shifts in the financing-state classification retain explanatory relevance. The overall $R^2$is very high (≈0.991), which is expected under two-way FE, given the large contribution of fixed effects; the within R^2(≈0.085) is more informative and indicates that the time-varying regressors account for a modest but meaningful share of within-country variation in vulnerable employment.

The lagged specification (Table 4), which relates vulnerable employment to the previous year's financing state, largely confirms the baseline pattern and strengthens the temporal interpretation. Lagged membership in Cluster 2 (β = −6.734, SE = 1.157, $p < 0.001$), Cluster 5 (β = −5.241, SE = 0.958, $p < 0.001$) and Cluster 3 (β = −4.400, SE = 1.609, $p < 0.01$) continues to predict lower vulnerable employment relative to the reference state, suggesting that the association is not purely contemporaneous. In contrast, the lagged effect for Cluster 4 becomes statistically insignificant (β = −2.532, SE = 1.508), indicating weaker persistence of this particular state's association over time. The omission of Cluster 6 due to collinearity reflects insufficient within-country variation for that category after accounting for country and year effects. It therefore does not affect inference for the remaining clusters.

The switchers-only fixed-effects model (145 observations) isolates identification to country–years in which at least one cluster transition occurs. In this subsample, Cluster 4 is the most frequent state and therefore serves as the reference. Relative to Cluster 4, Cluster 2 is associated with a statistically significant reduction in vulnerable employment (β = −4.135, SE = 0.787, $p < 0.01$). Cluster 3 also exhibits a negative association (β = −1.469), which is only marginally significant ($p < 0.10$). The remaining differences are imprecisely estimated: Cluster 5 is not statistically different from the reference (β = −1.811, SE = 1.260), while Cluster 6 exhibits a positive coefficient (β = 2.433, SE = 1.448) that does not reach conventional significance. Notably, the within-cap R-squared rises sharply (0.635), which is typical when the sample is restricted to observations that contain meaningful within-country variation in the key regressor; in other words, this subsample is mechanically more informative for estimating "state-change" effects.

Table 3. Two-way fixed-effects results: contemporaneous financing state and vulnerable employment

Regressor	Coefficient (β)	Clustered SE	p-value (approx.)	Interpretation (relative to Cluster 1)
Financing state: Cluster 2	-7.146	1.175	<0.001	Lower vulnerable employment by 7.146 p.p.
Financing state: Cluster 3	-5.288	1.574	<0.01	Lower vulnerable employment by 5.288 p.p.
Financing state: Cluster 4	-3.600	1.614	<0.05	Lower vulnerable employment by 3.600 p.p.
Financing state: Cluster 5	-5.802	1.030	<0.001	Lower vulnerable employment by 5.802 p.p.
GDP per capita (constant 2015 US$) ((x9))	-0.00010	0.000094	n.s.	No statistically significant within-country association
Employment-to-population ratio, 15+ ((x10))	0.1181	0.1371	n.s.	No statistically significant within-country association

Note: Dependent variable – Vulnerable employment, total (% of total employment) (modelled ILO estimate) (y1); Controls – GDP per capita (constant 2015 US$) (x9) and Employment to population ratio, 15+, total (%) (x10); Fixed effects country and year; SEs: clustered by country; Model diagnostics: Observations = 743; $R^2 = 0.99081$; Within $R^2 = 0.08474$; Reference category = Cluster 1. Cluster 6 was omitted due to collinearity with the fixed-effects structure. Significance: *** p<0.001, ** p<0.01, * p<0.05.
Source: authors' calculations in R Studio.

To improve comparability across specifications, the reference state was harmonised to Cluster 5 in both the full sample and the switchers-only subsample. Under this parameterisation, coefficients measure differences in vulnerable employment relative to the modal social-insurance-dominant state (Cluster 5). In the full sample, Cluster 6 is associated with a markedly higher share of vulnerable employment (β = 5.802, SE = 1.030, $p < 0.001$). In contrast, the other clusters do not differ significantly from Cluster 5 once country and year fixed effects are included. The switchers-only model yields a consistent direction and confirms statistical significance for Cluster 6 (β = 4.244, SE = 0.918, $p < 0.01$). This consistency indicates that the positive association of Cluster 6 relative to Cluster 5 is not driven solely by non-switching countries, but is also visible in the limited set of within-country transitions that define identification in the FE framework. The control variables remain weakly related to vulnerable employment: GDP per capita is marginally negative in the switchers-only sample ($p < 0.10$), whereas the employment-to-population ratio remains statistically insignificant.

Table 4. Two-way fixed-effects results: lagged financing state and vulnerable employment

Regressor	Coefficient (β)	Clustered SE	p-value (approx.)	Interpretation (relative to Cluster 1)
Lagged financing state: Cluster 2	-6.734	1.157	<0.001	Lower next-year vulnerable employment by 6.734 p.p.
Lagged financing state: Cluster 3	-4.400	1.609	<0.01	Lower next-year vulnerable employment by 4.400 p.p.
Lagged financing state: Cluster 4	-2.532	1.508	n.s.	No statistically significant lagged difference
Lagged financing state: Cluster 5	-5.241	0.9579	<0.001	Lower next-year vulnerable employment by 5.241 p.p.
GDP per capita (constant 2015 US$) ((x9))	-0.00010	0.000093	n.s.	No statistically significant within-country association
Employment-to-population ratio, 15+ ((x10))	0.0961	0.1375	n.s.	No statistically significant within-country association

Note: Dependent variable – Vulnerable employment, total (% of total employment) (modelled ILO estimate) (y1); Controls – GDP per capita (constant 2015 US$) (x9) and Employment to population ratio, 15+, total (%) (x10); Fixed effects – country and year; SEs: clustered by country. Model diagnostics: Observations = 699 (44 lost due to lag); $R^2 = 0.99138$; Within $R^2 = 0.08391$; Notes: Reference category = Cluster 1. Lagged Cluster 6 omitted due to collinearity. Significance: *** p<0.001, ** p<0.01, * p<0.05.
Source: authors' calculations in R Studio.

A key methodological implication is that the substantive conclusions depend on how the reference category is set and on where within-country variation exists. Because Cluster 1 is absent among switchers and Cluster 6 was previously collinear under the Cluster 1 reference, the most defensible presentation is to treat the modal state (Cluster 5) as the baseline and emphasise robust contrasts–particularly the persistent positive differential for Cluster 6–while interpreting other state differences more cautiously due to limited transition frequency.

Table 5. Two-way fixed-effects estimates for the switchers-only sample (reference = Cluster 4)

Regressor	Coefficient (β)	Clustered SE
Cluster 2 (vs Cluster 4)	-4.135	0.7868**
Cluster 3 (vs Cluster 4)	-1.469	0.7097.
Cluster 5 (vs Cluster 4)	-1.811	1.260
Cluster 6 (vs Cluster 4)	2.433	1.448
GDP per capita (x9)	-0.0000902	0.0000468ю
Employment-to-population ratio (x10)	-0.1721	0.1714

Note: Dependent variable – Vulnerable employment, total (% of total employment) (y1); Controls – GDP per capita (constant 2015 US$) (x9) and Employment to population ratio, 15+ (x10); Fixed effects – country and year; SEs: clustered by country; Observations – 145; R^2 – 0.98709; Within R^2 – 0.63452; Significance: *** p<0.001, ** p<0.01, * p<0.05, . p<0.10
Source: authors' calculations in R Studio.

The transition-based fixed-effects results (Table 7) provide a more direct interpretation of how moving into or out of the Cluster 6 financing state is linked to changes in vulnerable employment. To complement the state-dummy specification, transition indicators (entry/exit) are estimated to capture short-run changes around regime shifts. Level differences between stable regimes (state dummies) need not match short-run changes at the entry moment (enter indicators), especially when transitions are rare and may occur during atypical adjustment periods. Table 8 reports the entry and exit estimates for each state. When a country enters

Cluster 6 (i.e., switches from any other state into Cluster 6), vulnerable employment decreases by around 1.02 percentage points ($\beta = -1.024$, SE = 0.338, $p < 0.01$), conditional on country and year fixed effects and controlling for GDP per capita (x9) and the employment-to-population ratio (x10). Because this estimate is derived from within-country changes, it suggests that the transition into the Cluster 6 configuration is associated with a statistically significant reduction in the vulnerable employment share relative to the country's typical level in adjacent years.

Table 6. Two-way fixed-effects estimates with common reference category (reference = Cluster 5)

Regressor	Full sample β (SE)	Switchers-only β (SE)
Cluster 2 (vs Cluster 5)	-1.343 (0.8650)	-2.324 (1.240)
Cluster 3 (vs Cluster 5)	0.514 (0.8358)	0.342 (1.100)
Cluster 4 (vs Cluster 5)	2.203 (1.521)	1.811 (1.260)
Cluster 6 (vs Cluster 5)	5.802* (1.030)	4.244 (0.9176)
GDP per capita (x9)	-0.00010 (0.000094)	-0.0000902. (0.0000468)
Employment-to-population ratio (x10)	0.1181 (0.1371)	-0.1721 (0.1714)

Note: Dependent variable – Vulnerable employment, total (% of total employment) (y1); Controls – GDP per capita (constant 2015 US$) (x9) and Employment to population ratio, 15+ (x10); Fixed effects – country and year; SEs: clustered by country; Observations – 145; R^2 – 0.98709; Within R^2 – 0.63452; Significance: *** p<0.001, ** p<0.01, * p<0.05, . p<0.10
Source: authors' calculations in R Studio.

In contrast, the exit model does not report an estimated coefficient for exit6TRUE, which implies that there were either no observed "exit from Cluster 6" transitions in the usable sample, or that the exit indicator became collinear with the fixed-effects structure after accounting for missingness and the unbalanced panel. The decline in observations, from 741 to 700, is consistent with the loss of additional country–years due to lag/lead construction and missing values. Across both specifications, the control variables remain statistically insignificant, suggesting that–once country and year effects are absorbed–short-run variation in GDP per capita and the employment-to-population ratio does not explain vulnerable employment changes as strongly as the discrete state transition captured by enter6.

Although overall R^2remains very high (≈0.99) due to the two-way fixed effects, the within $R^2$is small (≈0.01–0.013), which is expected in a sparse-transition setting, where the model deliberately focuses on a limited set of transition events rather than broad continuous variation. Substantively, this makes the "enter6" result especially useful for the narrative, because it frames the cluster effect as a regime-shift association rather than as differences across largely static regimes.

Across the transition specifications, there is very limited evidence that entering most states is systematically associated with changes in vulnerable employment once country- and year-fixed effects are included. For Cluster 1, no enter/exit effect is estimated, indicating insufficient usable transitions (or collinearity) after accounting for the fixed-effects structure. For Cluster 2 and Cluster 3, entry effects are positive but statistically insignificant (enter2: $\beta = 0.550$, SE = 0.551; enter3: $\beta = 0.515$, SE = 0.487), and the corresponding exit effects are also insignificant (exit2: $\beta = 0.138$, SE = 0.383; exit3: $\beta = 0.264$, SE = 0.552), suggesting no robust within-country change in vulnerable employment around these transitions.

For Cluster 4 and Cluster 5, entry effects remain insignificant (enter4: $\beta = -0.096$, SE = 0.563; enter5: $\beta = 0.127$, SE = 0.390), and the exit effect from Cluster 5 is likewise insignificant (exit5: $\beta = -0.461$, SE = 0.412). The only transition coefficient reaching conventional significance is the exit from Cluster 4, which is associated with a 1.20 percentage point increase in vulnerable employment (exit4: $\beta = 1.199$, SE = 0.569, $p < 0.05$). Overall, these results are consistent with the earlier transition matrix evidence of strong state persistence: with relatively few transitions and modest within R^2values (around 0.01–0.015), most entry/exit estimates are imprecise, and statistically reliable effects emerge only for a small subset of regime changes.

An event-time specification was not feasible for Cluster 6 because the treated sample contained only a single observed entry and no pre-transition observations (minimum event time = 0), which prevented the identification of lead terms and yielded insufficient support for dynamic effects.

The results from Table 8 are a useful "mechanism" check because they show what happens to the cluster effects once the key financing channels that define the states are entered explicitly. Relative to the baseline model with cluster dummies only (reference = Cluster 5), adding compulsory private insurance (x3), out-of-pocket excluding cost-sharing (x7), and cost-sharing (x8) increases the explanatory content of the time-varying regressors: the within R^2rises from 0.0847 to 0.1371, indicating that these channels account for a non-trivial additional share of within-country variation in vulnerable employment.

Table 7. Transition-based two-way fixed-effects results: entry to and exit from financing states (Clusters 1–6) and vulnerable employment

Cluster (state)	Entry effect (β)	SE	Sig.	Exit effect (β)	SE	Sig.	Obs. entry	Obs. exit
				Entry				
1	– (not estimated)	–	–	– (not estimated)	–	–	740	701
2	0.5503	0.5509		0.1376	0.3830		742	699
3	0.5153	0.4872		0.2636	0.5520		731	711
4	-0.0961	0.5634		1.1990	0.5691	*	737	705
5	0.1267	0.3895		-0.4607	0.4120		723	719
6	-1.0240	0.3381	**	– (not estimated)	–	–	741	700
				Exit				
1	– (not estimated)	–	–	– (not estimated)	–	–	740	701
2	0.5503	0.5509		0.1376	0.3830		742	699
3	0.5153	0.4872		0.2636	0.5520		731	711
4	-0.0961	0.5634		1.1990	0.5691	*	737	705
5	0.1267	0.3895		-0.4607	0.4120		723	719
6	-1.0240	0.3381	**	– (not estimated)	–	–	741	700

Note: Dependent variable – Vulnerable employment, total (% of total employment) (modelled ILO estimate) (y1); Controls – GDP per capita (constant 2015 US$) (x9) and Employment to population ratio, 15+, total (%) (x10); Fixed effects – country and year; SEs – clustered by country; "Entry effect" is estimated from $y1_{it} = \alpha_i + \gamma_t + \beta \cdot 1[\text{enter k}] + \delta_1 x9_{it} + \delta_2 x10_{it} + \varepsilon_{it}$. "Exit effect" is defined analogously for leaving the state. Some entry/exit indicators are not estimated because there are no usable transitions or because the indicator becomes collinear with the fixed-effects structure after accounting for missingness in the unbalanced panel (as reported by fixest). Significance codes: *** p<0.001, ** p<0.01, * p<0.05, . p<0.10.
Source: authors' calculations in R Studio.

The added financing variables display interpretable associations with vulnerable employment. Higher out-of-pocket spending excluding cost-sharing (x7) is linked to higher vulnerable employment ($\beta = 1.551$, SE = 0.588, $p < 0.05$), and higher cost-sharing (x8) is also positively related ($\beta = 0.875$, SE = 0.458, $p < 0.10$). In contrast, compulsory private insurance (x3) is negatively associated ($\beta = -1.599$, SE = 0.907, $p < 0.10$), suggesting that greater reliance on this channel–holding other factors constant–is weakly related to lower vulnerable employment. GDP per capita (x9) and the employment-to-population ratio (x10) remain statistically insignificant.

Importantly, the estimated differential for Cluster 6 relative to Cluster 5 becomes substantially larger when x3, x7 and x8 are included (from 5.802* to 12.16). This pattern implies that Cluster 6 is not merely a proxy for these three channels; rather, once they are controlled for, the remaining cluster-specific composition (i.e., the joint configuration of the financing mix beyond x3, x7 and x8) is associated with additional variation in vulnerable employment. This is consistent with Cluster 6 capturing a broader regime characteristic, such as an institutional configuration of mandatory private insurance combined with other financing elements, that is not fully reducible to the included single-channel measures. The omission of Cluster 1 again reflects collinearity with the fixed-effects structure and the chosen reference category, rather than a substantive absence of relevance.

Table 8. Mechanism check: baseline two-way FE model versus augmented specification including key financing channels

Regressor	Baseline FE: state dummies only (β; SE)	Augmented FE: + x3, x7, x8 (β; SE)
Cluster 2 (vs Cluster 5)	-1.343 (0.865)	-1.321 (0.806)
Cluster 3 (vs Cluster 5)	0.514 (0.836)	0.511 (0.785)
Cluster 4 (vs Cluster 5)	2.203 (1.521)	2.005 (1.561)
Cluster 6 (vs Cluster 5)	5.802* (1.030)	12.16 (4.040)
Compulsory private insurance (x3)	–	-1.599. (0.907)
Out-of-pocket excl. cost-sharing (x7)	–	1.551* (0.588)
Cost-sharing (x8)	–	0.8748. (0.458)
GDP per capita (constant 2015 US$) (x9)	-0.00010 (0.000094)	-0.0000947 (0.0000969)
Employment-to-population ratio, 15+ (x10)	0.118 (0.137)	0.165 (0.144)

Note: Dependent variable – Vulnerable employment, total (% of total employment) (modelled ILO estimate) $(y1)$; Reference state – Cluster 5; Controls – GDP per capita (constant 2015 US$) $(x9)$; Employment to population ratio, 15+, total (%) $(x10)$; Fixed effects – country and year; SEs – clustered by country; Model diagnostics: Observations – 743 (both models); R^2 – 0.99081 (baseline); 0.99134 (augmented); Within R^2 – 0.08474 (baseline); 0.13707 (augmented); Country and year fixed effects included. Standard errors clustered by country. Cluster 1 was omitted due to collinearity with the fixed-effects structure under the chosen reference category. Significance: *** p<0.001, ** p<0.01, * p<0.05, . p<0.10.

Source: authors' calculations in R Studio.

The predicted values translate the fixed-effects coefficients into an intuitive ranking of financing "states" by expected vulnerable employment $(y1)$, holding GDP per capita $(x9)$and the employment-to-population ratio $(x10)$at their sample means and evaluating fixed effects at the same country–year (Table 9). In the baseline model (state dummies only, with Cluster 5 as the reference), the lowest predicted vulnerable employment is obtained for Cluster 2 (20.42), followed by Clusters 1 and 5 (both 21.77, by construction under the chosen fixed effects), and Cluster 3 (22.28). Cluster 4 yields a higher predicted level (23.97), while Cluster 6 stands out as the state with the highest risk (27.57). The gap between Cluster 6 and the modal Cluster 5 is about 5.80 percentage points, mirroring the estimated FE coefficient for Cluster 6 in Table 9.

Table 9. Predicted vulnerable employment by financing state (country–year fixed effects held constant)

Financing state (cluster)	Predicted (y1): baseline FE (states + x9, x10)	Predicted (y1): augmented FE (states + x3, x7, x8 + x9, x10)
1	21.77	22.60
2	20.42	21.28
3	22.28	23.11
4	23.97	24.61
5	21.77	22.60
6	27.57	34.77

Note: Predictions are computed from two-way fixed-effects models evaluated at the sample means of covariates and at the same reference country–year (fixed effects held constant). Values are predicted levels of Vulnerable employment, total (% of total employment) (y1). The baseline model corresponds to $\mathrm{y1} \sim \mathrm{i(cluster, ref} = 5) + \mathrm{x9} + \mathrm{x10}$ with country and year fixed effects. The augmented model additionally includes x3, x7, and x8. Because country and year fixed effects are included, predicted levels depend on the fixed-effects reference; differences across states are therefore the primary object of interpretation. Cluster 1 not identified in FE (collinear); predicted value equals baseline mechanically.
Source: authors' calculations in R Studio.

Once the key financing channels (x3, x7, x8) are added (mechanism specification), predicted levels shift upward slightly for most states, and the separation becomes sharper. Cluster 2 remains the lowest predicted state (21.28), while Cluster 3 and Cluster 4 increase to 23.11 and 24.61, respectively. Most importantly, the predicted level for Cluster 6 increases dramatically to 34.77, indicating that, conditional on the same country–year fixed effects and mean controls, the remaining cluster-specific composition captured by Cluster 6 is associated with a substantially higher vulnerable employment level even after accounting for compulsory private insurance (x3), out-of-pocket spending excluding cost-sharing (x7), and cost-sharing (x8). This aligns with Table 9, where the Cluster 6 coefficient rises in magnitude when these channels are controlled for, suggesting that the Cluster 6 regime reflects a broader institutional-financing configuration linked to elevated vulnerability in employment rather than being reducible to single financing components.

4. Discussion

The regime-level results, especially the higher predicted vulnerable employment in Cluster 6 relative to the modal Cluster 5 (27.57 vs. 21.77 in the baseline; 34.77 for Cluster 6 in the augmented model), align with the broader evidence that weaker financial protection and higher household exposure to health costs are linked to socio-economic fragility. Household vulnerability to health shocks has been shown to translate into poverty risks and constrained coping capacity, which is compatible with higher shares of vulnerable employment when households face persistent cost pressures. (Chhetri & Adhikari, 2025; Dudek & Landmesser, 2025; Flores et al., 2024). The same direction is consistent with evidence that out-of-pocket health spending has measurable spillovers into real-economy behaviour, implying that direct-payment burdens can affect household decisions beyond healthcare itself. (Ukpe & Mohamadou, 2024). In this sense, this state-based classification adds a structural layer: it shows that not only "spending more" but how financing is composed aligns with labour-market vulnerability.

The mechanism checks reinforce this interpretation through channels already emphasised in related work on insurance design and system functioning. The positive association between vulnerable employment and out-of-pocket spending, excluding cost-sharing (x7) and cost-sharing (x8), is consistent with the idea that greater reliance on household payments weakens protection against shocks, which can be reflected in labour market precariousness. (Chhetri & Adhikari, 2025; Ukpe & Mohamadou, 2024). At the same time, the literature suggests that insurance models can be linked to better health outcomes and welfare proxies, indicating a potential pathway from more effective pooling to improved socioeconomic stability. (Kuzior et al., 2025a; Mureșan et al., 2023). The finding that compulsory private insurance (x3) is weakly negative when entered directly, while the Cluster 6 regime effect remains large, is consistent with the view that outcomes depend on the operational and institutional configuration of insurance arrangements, such as claims processing, trust, and design features, rather than the formal presence of insurance alone. (Al-Kahtani et al., 2025; Gavurova et al., 2024).

Finally, the contrast between regime “levels” (Cluster 6 being higher than Cluster 5) and the short-run entry estimate (enter6 associated with a −1.024 percentage point) mirrors the reform dynamics discussed in the health-system reform literature. Reform episodes can generate transitional adjustments that do not immediately align with the long-run properties of the post-reform financing architecture, particularly when changes occur under crisis conditions or in the face of institutional constraints. (Alotaiby & Krenyácz, 2024; Gheorghe & Panazan, 2024). These empirical settings strengthen this interpretation: switching is rare, so identification is driven by a small number of transitions, meaning “entry effects” describe short-run adjustments around exceptional years rather than stable cross-regime differences. In combination, the evidence suggests a coherent story: persistent regimes with higher household liabilities are associated with higher vulnerable employment. At the same time, the immediate transition year can reflect temporary adjustment processes rather than the steady-state regime relationship. (Liashenko et al., 2024; Saher et al., 2025; Lyeonov et al., 2025).

Several limitations should be acknowledged. First, the panel is unbalanced, and coverage varies across countries and years, which reduces the number of usable observations for clustering and fixed-effects estimation and may limit external validity to the subset of countries and years with complete data. Second, the financing “states” are derived from k-means clustering and therefore depend on modelling choices such as standardisation, the selected number of clusters, and sensitivity to rare observations. At the same time, multiple diagnostics and robustness checks mitigate this concern; alternative algorithms could yield partially different partitions. Third, cluster membership is highly persistent for many countries, implying that the fixed-effects coefficients are identified mainly from a small subset of within-country transitions; estimates should therefore be interpreted as associations linked to rare regime shifts rather than broad cross-country comparisons. Fourth, the outcome variable is the World Bank’s modelled ILO estimate of vulnerable employment, which may introduce measurement error and constraints on cross-country comparability. Finally, despite the inclusion of country and year fixed effects and controls for GDP per capita and employment intensity, the analysis remains observational; time-varying confounders (e.g., labour-market regulation, social protection reforms, shocks to informality) may still bias estimates, so the results should not be interpreted as strictly causal effects.

Conclusion

This article aimed to identify distinct health-financing regimes and to assess how within-country shifts between these financing "states" are associated with labour-market vulnerability, proxied by vulnerable employment. The analysis employed an unbalanced annual country–year panel that combines OECD indicators of health-financing schemes (x1–x8, as shares of GDP) with World Bank controls for GDP per capita (x9) and the employment-to-population ratio (x10), as well as the World Bank modelled ILO estimate of vulnerable employment (y1). Country–year observations were clustered on x1–x8 to construct a time-varying regime indicator, which was then linked to y1 using two-way fixed-effects models with country and year fixed effects and country-clustered standard errors; additional specifications examined lagged regimes, switchers-only subsamples, transition (entry/exit) effects, and a mechanism check adding key financing channels (x3, x7, x8).

The findings reveal systematic differences in vulnerable employment across various financing regimes and during rare regime shifts. In the baseline two-way fixed-effects model (reference state = Cluster 5), Cluster 6 is associated with a substantially higher vulnerable employment share relative to Cluster 5 ($\beta = 5.802$, $p < 0.001$). This contrast remains significant in the switchers-only sample ($\beta = 4.244$, $p < 0.01$). Predicted values illustrate the magnitude: under the baseline specification, the predicted vulnerable employment is 21.77 in Cluster 5, versus 27.57 in Cluster 6, while Cluster 2 shows the lowest predicted level (20.42). When key financing components are included alongside regime indicators (x3, x7, x8), the within explanatory power rises (within R^2 from 0.0847 to 0.1371), out-of-pocket spending excluding cost-sharing is positively associated with vulnerable employment ($\beta = 1.551$, $p < 0.05$) and cost-sharing is weakly positive ($\beta = 0.875$, $p < 0.10$). In contrast, compulsory private insurance has a weakly negative effect ($\beta = -1.599$, $p < 0.10$). In this augmented model, the predicted vulnerable employment level for Cluster 6 increases further to 34.77, indicating that this regime captures an institutional-financing configuration associated with elevated labour-market vulnerability beyond the included single-channel measures. Transition models also suggest that entering Cluster 6 is associated with a short-run decline in vulnerable employment ($\beta = -1.024$, $p < 0.01$), underscoring that short-run adjustments around regime changes need not mirror long-run level differences between stable regimes.

Policy implications follow from the consistent role of household-burdened financing components and the elevated vulnerability associated with the Cluster 6 regime. First, strategies that reduce reliance on direct household payments, particularly out-of-pocket spending and cost-sharing, should be prioritised as part of inclusive health-financing reforms, as higher values of these channels are associated with higher vulnerable employment within countries. Second, where compulsory private insurance plays a major role, complementary measures are needed to prevent the transmission of vulnerability to labour markets, including stronger pooling, protection for low-income households, and tighter regulation of cost-sharing and benefit design. Third, reforms should be evaluated not only by their immediate effects during transition years but also by their medium-term regime characteristics, since level differences across financing states are sizeable even when entry effects are modest. Aligning health-financing architecture with stronger financial protection appears to be a relevant lever for reducing labour-market vulnerability, especially in contexts where household contributions remain structurally high.

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Appendix A.

Table A1. Cluster switching intensity by country (full sample, N = 44)

Country	n_years	n_switches	switch_rate	Country	n_years	n_switches	switch_rate
Argentina	17	2	0.125	Japan	12	0	0.000
Australia	10	0	0.000	Korea	24	0	0.000
Belgium	21	0	0.000	Latvia	7	1	0.167
Brazil	20	1	0.053	Lithuania	10	0	0.000
Canada	18	1	0.059	Luxembourg	12	0	0.000
Chile	21	0	0.000	Malta	8	0	0.000
China	23	0	0.000	Mexico	13	0	0.000
Colombia	24	0	0.000	Netherlands	24	1	0.043
Costa Rica	20	0	0.000	Norway	8	0	0.000
Croatia	10	0	0.000	Peru	22	0	0.000
Cyprus	14	0	0.000	Poland	11	0	0.000
Czechia	14	0	0.000	Slovak Republic	4	0	0.000
Denmark	14	0	0.000	Slovenia	6	0	0.000
Estonia	21	0	0.000	South Africa	23	0	0.000
Finland	24	1	0.043	Spain	21	0	0.000
Germany	24	0	0.000	Sweden	13	0	0.000
Hungary	18	0	0.000	Switzerland	24	0	0.000
Iceland	24	0	0.000	Thailand	24	0	0.000
India	23	0	0.000	Türkiye	6	0	0.000
Indonesia	24	0	0.000	Ukraine	22	1	0.048
Ireland	13	1	0.083	United Kingdom	24	0	0.000
Israel	18	0	0.000	United States	10	0	0.000

Note: Summary of switch_rate across countries: Min = 0.000; 1st Qu. = 0.000; Median = 0.000; Mean = 0.014; 3rd Qu. = 0.000; Max = 0.167.
Source: authors' calculations in R Studio.