The European Journal of Development Research
https://doi.org/10.1057/s41287-026-00762-8

RESEARCH

The Effects of Structural Conditions on Sectoral Productivity in South, East and South East Asia

Rouven E. Haschka[1,2]

Received: 27 May 2025 / Accepted: 22 June 2026
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Abstract

The debate over the IMF's conditions for international lending has been ongoing since its inception, with growing concerns about its imposition on developing countries, particularly since the 1980s. In addition to assessing their normative influence, a critical focus is on understanding the economic impact of these conditions in recipient countries. While several studies have examined economic growth in response to aggregate conditionality, there is a lack of research that examines the impact of specific conditions on (i) disaggregated economic outcomes and (ii) productivity rather than economic growth. Based on three macroeconomic datasets on IMF conditions, (economic) development and productivity indicators, this study aims to identify the impact of conditions in 13 distinct policy areas on economy-wide as well as sectoral productivity over the period 1980-2019 in South, East, and Southeast Asian countries. While conditioning on a rich set of control variables, our results, derived from identification strategies designed to be robust to endogeneity, indicate heterogeneous sectoral responses to IMF conditions, consistent in sign across estimators, while economy-wide productivity is largely unaffected. Consequently, we derive sector-specific policy recommendations which are aligned with the effects of extant policy conditions on sectoral productivity.

Keywords Productivity · IMF programs · IMF conditionality · Endogeneity

Résumé

Le débat sur les conditions imposées par le FMI pour les prêts internationaux se poursuit depuis sa création, suscitant des inquiétudes croissantes quant à son imposition aux pays en développement, particulièrement depuis les années 1980. Outre

✉ Rouven E. Haschka
rouven.haschka@rptu.de; rouven.haschka@uni-corvinus.hu

[1] Chair of Data Analytics, Technical University of Kaiserslautern-Landau, Kaiserslautern, Germany

[2] Institute of Strategy and Management, Corvinus University, Budapest, Hungary

Published online: 04 August 2026

l'évaluation de leur influence normative, une analyse critique consiste à comprendre l'impact économique de ces conditions dans les pays bénéficiaires. Alors que plusieurs études ont examiné la croissance économique en réponse aux conditions agrégées, il existe un manque de recherches examinant l'impact de conditions spécifiques sur (i) résultats économiques désagrégés et (ii) productivité plutôt que croissance économique. Sur la base de trois ensembles de données macroéconomiques concernant les conditionnalités du FMI et le développement (économique) et des indicateurs de productivité, cette étude vise à identifier l'impact des conditions dans 13 différents domaines de politique sur la productivité globale ainsi que sectorielle au cours de la période 1980-2019 dans les pays d'Asie du Sud, de l'Est et du Sud-Est. Tout en conditionnant sur un ensemble riche de variables de contrôle, nos résultats, issus de stratégies d'identification conçues pour être robuste à l'endogénéité, indiquent des réponses sectorielles hétérogènes aux conditions du FMI, cohérentes en termes de signe entre les estimateurs, tandis que la productivité à l'échelle de l'économie est en grande partie inchangée. Par conséquent, nous formulons des recommandations politiques spécifiques à chaque secteur qui sont alignées avec les effets des conditions politiques existantes sur la productivité sectorielle.

Resumen

El debate sobre las condiciones del FMI para la concesión de préstamos internacionales ha sido continuo desde sus inicios, con crecientes preocupaciones acerca de su imposición a los países en desarrollo, particularmente desde la década de 1980. Además de evaluar su influencia normativa, un análisis crítico del enfoque está en comprender el impacto económico de estas condiciones en los países receptores. Si bien varios estudios han analizado el crecimiento económico en respuesta a condiciones agregadas, existe una falta de investigaciones que analicen el impacto de condiciones específicas en (i) resultados económicos desagregados y (ii) productividad en lugar de crecimiento económico. En base a tres conjuntos de datos macroeconómicos sobre las condiciones del FMI y el desarrollo (económico) e indicadores de productividad, este estudio tiene como objetivo identificar el impacto de las condiciones en 13 áreas de política diferenciadas sobre la productividad a nivel de toda la economía en general, así como sectorial, durante el período 1980-2019 en los países del sur, este y sudeste asiático. Mientras se condiciona a un amplio conjunto de variables de control, nuestros resultados, derivados de estrategias de identificación diseñadas para ser robustos frente a la endogeneidad, indican respuestas sectoriales heterogéneas a las condiciones del FMI, coherentes en cuanto al signo entre los distintos estimadores, mientras que la productividad a nivel de toda la economía es en gran medida no alterada. En consecuencia, derivamos recomendaciones de política específicas para cada sector que están alineadas con los efectos de las condiciones de política existentes sobre la productividad sectorial.

Mathematics Subject Classification F3 · F4 · O1 · O2

Introduction

In addition to providing targeted policy advice, lending constitutes a cornerstone of the International Monetary Fund's (IMF) mission to foster economic growth and stability (Fund, 2023). While loans can be disbursed without specific requirements, they are often contingent upon the implementation of structural reforms in recipient countries (Fund, 2023; Reinhart & Trebesch, 2016). These policy conditions may entail measures such as reducing subsidies for domestic goods, facilitating access to financial markets, or increasing taxation, depending on the particular circumstances of the country (Kentikelenis & Stubbs, 2023; Crivelli & Gupta, 2016). While the formulation of conditions is itself subject of debate (Haggard, 2000; Stiglitz, 2002), understanding the impact of conditionality is paramount from an economic perspective. The Asian Financial Crisis illustrates what is at stake: Indonesia, South Korea and Thailand each entered large IMF-supported programmes in 1997-98 whose conditions spanned financial-sector restructuring, fiscal retrenchment and trade and capital-account liberalisation (Haggard, 2000). The intensity of conditionality varies widely across the region. Pakistan has been a near-permanent programme country, accumulating by far the largest stock of conditions in our sample. These cases motivate our focus on the directional, sector-specific consequences of *which* conditions are imposed, rather than on conditionality in the aggregate.

Critics argue that the intrusive and ambitious nature of IMF conditions may lead to adverse consequences (Feldstein, 1998). Motivated by the necessity for a more rigorous empirical evaluation, a significant body of research has scrutinized the effects of IMF conditions on economic and socioeconomic outcomes in recipient countries (Chletsos & Sintos, 2023; Kouamé & Tapsoba, 2019; Forster et al., 2019; Stubbs et al., 2020). These studies have provided insights into the channels through which policy conditions affect society. However, the effect of structural conditions on economic development at a sector-specific level remains ambiguous. While economic growth rates measured in terms of GDP may be appropriate indicators of overall economic development, they provide limited information about the sustainability of this development, which relies on economy-wide and sector-specific productivity growth (Economic and Social Commission for Asia and the Pacific,, 2016; Organization, yyy). More fundamentally, little is known about the directional effects of IMF conditions on different sectors.

Against this backdrop, we analyze the effects of various IMF structural conditions on both sector-specific and economy-wide productivity (growth) in South, East, and Southeast Asian (SESEA) countries for the period 1980-2019. We focus on this particular group of countries due to their exceptional growth performance over this period, which stemmed from a robust institutional and policy environment conducive to productivity-enhancing structural change (Jaumotte & Spatafora, 2007). The role of IMF-mandated policy conditions in shaping this successful transformation presents an intriguing case, offering valuable insights for future decision-makers regarding the effectiveness of specific conditions amidst overall positive economic performance. While the patterns in intrasectoral structural change mirror those observed in SESEA countries (McMillan et al., 2014), it's worth noting that numerous IMF-supported Latin American and Sub-Saharan African countries have encountered productivity-

reducing structural changes, casting doubt on the effectiveness of policy conditions in enhancing sectoral productivity. Viewed from this perspective, SESEA countries offer an intriguing case for examining the effects of conditionality on sectors with varying productivity levels, owing to their advanced structural transformation and heterogeneous sectoral productivity growth. Moreover, the comprehensive availability of productivity-related data for a significant number of developing countries in Asia enables unbiased estimates of conditionality effects at both the economy-wide and sectoral levels. Our empirical model (i) incorporates a comprehensive set of control variables to mitigate potential omitted variable biases and (ii) utilizes novel identification strategies to bolster robustness against the adverse effects of neglected endogeneity. Concerning (i), previous studies have often highlighted the challenge of obtaining harmonized control variables to better discern heterogeneity across countries and time periods (Balima & Sokolova, 2021). For our empirical analysis, we merge data from the IMF conditionality dataset compiled by Kentikelenis and Stubbs (2023), the Asian Productivity Organization (APO) productivity dataset, and the World Bank Development Indicators. This combined dataset offers a wealth of information, providing us with a unique opportunity to accurately estimate the effects of 13 IMF structural conditions on productivity growth across the agricultural, manufacturing, and service sectors, while also controlling for over 50 other variables. Regarding (ii), a significant portion of research on development outcomes employs instrumental variable approaches, such as GMM or two-stage least squares estimation (Forster et al., 2019; Chletsos & Sintos, 2023; Kouamé & Tapsoba, 2019). However, these approaches encounter substantial drawbacks in the presence of weak instrumentation (Fröhlich & Sperlich, 2019). While the pursuit of sufficient and independent instrumental variable information is deemed infeasible, we employ more sophisticated identification strategies, incorporating heteroskedastic-based instrumentation (Lewbel, 2012) and a recent copula-based estimator (Haschka, 2022).

Our findings reveal that IMF structural conditions exert diverse effects on different sectors, with each sector exhibiting distinct directions of impact. In addition to recognizing cross-country variations, our research underscores the importance of conducting sector-specific analyses, as an economy-wide approach may obscure crucial effects. To preview our findings, the most robust patterns are sectoral rather than economy-wide and are consistent across all three estimators: debt-restructuring and tax conditions are associated with lower productivity in the service sector, state-owned-enterprise reform with higher productivity in manufacturing, and privatisation with lower manufacturing productivity on impact, alongside a delayed agricultural gain two years later. Economy-wide total factor productivity, by contrast, is largely unaffected, so an aggregate perspective would miss these sectoral responses. These contrasts underscore the importance of addressing conditionality effects in a manner tailored to each sector.

Overall, our results for policy conditions across various areas are consistent across estimators, but we identify variations in estimated effect sizes and predictions among single estimators. As policies should be based on stable results rather than estimator-dependent conclusions, we underscore the necessity of sensitivity checks from a practical perspective. In addition, our results underscore the potential of rich but rather underutilized datasets in the productivity literature. By incorporating a

comprehensive set of exogenous control variables, the dataset provided by the APO enables minimizing potential estimation bias due to omitted variables while sophisticated identification strategies (Haschka, 2022; Lewbel, 2012) allow for further robustification against endogeneity. As a result, we observed minimal disparities between endogeneity-robust estimations and conventional least squares estimation.

We contribute to the empirical literature on structural reforms by examining the impact of IMF structural conditions from thirteen distinct policy areas on aggregate and sectoral productivity. More precisely, we evaluate how IMF conditions influence productivity in the sectors of agriculture, manufacturing, and services, respectively, in Asia. Our contribution is four-fold:

First, we offer insights into the impact of structural reforms in developing economies. Our regional focus on developing countries in Asia allows us to account for a broad range of variables that could impact productivity through channels other than the conditioned structural reforms. Previous studies examining the effects of typical structural reforms, including those related to labour market regulation, social policy, and poverty reduction, have largely relied on data from developed economies, neglecting the unique circumstances of economies undergoing structural transformation (Sen, 2019).

Second, we provide evidence on the impact of IMF conditioned structural reforms on the formal and informal economy. Our indicators for productivity, such as economy-wide total factor productivity (TFP) and sector-specific value added per worker are calculated from labour statistics which include informal employment, thereby allowing us to observe the impact of structural conditions on a wider range of the economy. Most research on the impact of structural reforms in the external (e.g.,Konte et al. (2022); Kouamé and Tapsoba (2019); Topalova and Khandelwal (2011)) and/or real sector (e.g.,Amin (2015); Chen et al. (2021); Kouamé and Tapsoba (2019)) in this domain, which uses data from developing countries, focuses on firm-level productivity, which is a critical shortcoming given the huge informal employment sector in emerging and developing economies (Benson & Zhu, 2011; International Labour Office, 2018; Oecd, 2013). While it might seem reasonable to adhere to the position that'the more granular an effect measure, the better', an ignorance of informal production in an evaluation of structural conditions' effects could lead to detrimental outcomes. For example, governments might favour conditions which exert a positive effect on firm-level productivity, unaware of the potential (unidentified) negative impact on informal productivity. Reports of rising poverty and higher income inequality in countries which were subject to certain IMF conditions (see Sect. "Effect of IMF Conditions on Development Outcomes") sustain this concern of a unilateral focus on the firm-level.

Third, our focus on sectoral productivity in agriculture, manufacturing and services, respectively, next to economy-wide productivity promises a more nuanced understanding of the distinct effects of structural reforms on the sectoral economy. For instance, we observe that the service sector benefits from labour conditions but is adversely impacted by tax conditions, while the other two sectors show no immediate effects from either. An exclusive focus on a composite productivity measure, prevalent in many studies across multiple developing countries, constrains the devel-

opment of targeted policies that could leverage the positive impacts of reforms for particular sectors while ensuring better protection for those adversely affected.

Fourth, by employing three different types of models to address endogenous regressors, we aim to provide results that are less sensitive to the chosen method. Additionally, the inclusion of two relatively novel methods in development economics, namely Heteroskedastic IV and Panel copulas, serves to encourage scholars in the field to explore more innovative identification strategies. This is particularly pertinent if the assumptions of commonly used methods like system GMM are not met or if their application results in common issues such as weak instruments. Thus far, most studies examining the impact of structural conditions have relied on a single approach to address endogeneity concerns, primarily system GMM, despite its assumptions being difficult to test reliably.

The remaining article is structured as follows: After a review of development outcomes of IMF structural conditions in Sect. "Effect of IMF Conditions on Development Outcomes", we develop, in Sect. "A Framework for the Sectoral Transmission of IMF Conditionality", a framework that links IMF structural conditions to sectoral productivity and from which we derive testable, sector-specific hypotheses. Chapter "Data and Variable Description" provides information on data sources and statistical methods used in our estimation. Subsequently, chapter "Empirical Evidence" outlines the most important results and their relation to previous research and contextual characteristics. We provide a conclusion of our findings and suggestions for further research in Sect. "Conclusion".

Effect of IMF Conditions on Development Outcomes

An essential feature of IMF-conditioned reforms, distinguishing them from structural reforms adopted in calmer circumstances, is that they are implemented in response to financial crises, are negotiated between domestic authorities and the Fund rather than freely chosen, and are oriented towards restoring balance-of-payments viability and macroeconomic stability while laying the foundations for sustainable growth (IMF, 2023). The Fund may pursue its own objectives in formulating conditions (Dreher et al., 2009; Haggard, 2000), and both this negotiated character and the crisis context in which conditions are imposed matter for how their effects should be interpreted and identified. We summarise here the evidence on the development outcomes of IMF conditionality, and in Sect. "A Framework for the Sectoral Transmission of IMF Conditionality" develop the framework that links conditions to sectoral productivity.

Early studies typically used the total number of conditions as the explanatory variable (Stubbs et al., 2020), an aggregation that risks cancelling effects of opposite sign across policy areas and inviting treatment-heterogeneity bias. The Monitoring of Fund Arrangements database and, more comprehensively, the Conditionality Dataset of Kentikelenis and Stubbs (2023) which we use enabled researchers to disaggre-

gate conditions by policy field, and a substantial body of work now estimates field-specific effects.[1]

On individual development outcomes, conditions have been linked to lower public-sector wage spending (Rickard & Caraway, 2019), higher foreign direct investment (Woo et al., 2013), greater trade openness (Wei & Zhang, 2010) and higher tax revenue (Crivelli & Gupta, 2016), while leaving the overall fiscal balance unaffected (Bulíř & Moon, 2004). Studies that consider several fields jointly report a mix of beneficial and adverse socioeconomic consequences: successfully implemented fiscal, financial, real-sector and trade conditions raise firm-level productivity (Kouamé & Tapsoba, 2019), whereas conditionality across various areas has been associated with higher unemployment (Chletsos & Sintos, 2023), greater income inequality (Forster et al., 2019) and increased poverty (Biglaiser & McGauvran, 2022). It is precisely this heterogeneity of effects across domains, which is beneficial in some, adverse in others, that motivates the disaggregated, sector-specific analysis of productivity we develop below.

A Framework for the Sectoral Transmission of IMF Conditionality

Extrapolating from the literature on unconditioned structural reforms to IMF-conditioned reforms is not straightforward. Conditioned reforms are adopted under financial duress, are negotiated rather than freely chosen, and travel as bundles rather than as isolated instruments (Dreher et al., 2009; Haggard, 2000). Much of the available micro-evidence, moreover, is drawn from advanced economies or from firm-level samples that exclude the large informal and self-employed segments that dominate developing Asia (Sen, 2019). Rather than read a directional prediction off each strand of that evidence in isolation, which would reproduce the inconsistency of inferring some signs from empirical regularities and others from theory when evidence is thin, we organise the predictions deductively. We first specify the *channels* through which a condition can affect productivity, then the *structural characteristics* that determine how strongly a given sector is exposed to each channel, and finally a *selection* layer that explains why countries receive the conditions they do and what this implies for identification. The empirical literature then enters as evidence on the *strength and sign* of individual channels, not as the source of the sectoral mapping itself.

Layer 1: Channels and Sectoral Exposure

A structural condition does not act on "the economy" as an undifferentiated whole. It perturbs a small number of proximate margins, the relative price of tradables, the cost and availability of external finance, the fiscal envelope for public investment, the regulatory cost of formal employment, the boundary between state and private ownership, and the quality of the institutional environment. Productivity in a sector responds to a condition only to the extent that the sector is *exposed* to the margin that

[1] We adopt this classification of conditions to policy fields throughout; see Sect. "A Framework for the Sectoral Transmission of IMF Conditionality".

the condition moves. Sectoral heterogeneity in our results is therefore not a puzzle to be rationalised after the fact; it is the direct prediction of crossing a condition's channel with a sector's exposure profile.

For agriculture, manufacturing and services in South, East and Southeast Asia, five structural characteristics summarise that exposure. They are not ad hoc: each is a standard primitive of the structural-transformation and misallocation literatures (McMillan et al., 2014; Sen, 2019; Syverson, 2011).

(C1) Tradability and import-competition exposure. Manufacturing output is highly tradable and directly import-competing; the non-tradable, locally consumed services that account for most service employment in the region (retail, personal and community services) are largely insulated from external competition; agriculture is tradable but, in several SESEA economies, of low revealed comparative advantage, so liberalisation tends to expose rather than reward it. C1 governs the incidence of trade (EXT) and capital-account (FIN) conditions.

(C2) Organisational form and informality. Agricultural production in the region is overwhelmingly own-account and family-based and is only weakly connected to the formal tax net, formal credit and statutory labour regulation. Manufacturing is comparatively firm-organised and formal. Services are bimodal, formal financial and business services alongside a large informal retail and personal-service tail. C2 governs the incidence of conditions that operate through formal institutions: labour regulation (LAB), taxation (RTP), social policy and transfers (SP, POV) and fiscal administration (FP). A condition that works through the formal sector cannot transmit into a margin that is mostly informal; this is why several formal-channel conditions are predicted to leave agriculture unaffected.

(C3) Capital- and external-finance dependence. Manufacturing is capital-intensive and reliant on machinery investment and working capital; parts of services (finance, utilities-linked activities) are capital-intensive while the labour-intensive service tail is not; smallholder agriculture has low formal-credit penetration. C3 governs the incidence of financial-liberalisation (FIN) conditions and the investment-side response to taxation (RTP).

(C4) State-ownership and public-investment dependence. Manufacturing and infrastructure-linked services historically embed state-owned enterprises and public procurement chains. Crucially, the service sector is the residual claimant on *sustained public investment*: when fiscal space is withdrawn, agriculture, an established, largely self-financing sector in these economies, and manufacturing, whose short-run dynamics are dominated by private demand and the trade channel, are comparatively insulated, whereas public-investment-contingent services bear the adjustment. C4 governs the incidence of debt-restructuring (DEB), privatisation (PRI) and SOE-reform (SOE) conditions and yields the otherwise puzzling prediction that a contractionary debt condition concentrates in services rather than spreading evenly.

(C5) Asset fixity and adjustment horizon. Manufacturing combines high fixed costs with rigid hiring and firing, so adverse shocks register sharply on impact and may reverse only after firms adapt; agricultural adjustment runs through household labour reallocation and therefore materialises with a lag; the labour-intensive service tail adjusts quickly. C5 is not a cross-sectional channel but a *timing* channel: it predicts which contemporaneous effects should reverse, and in which sector, once adap-

tation has occurred, and it supplies the rationale for examining short lags ($t-1, t-2$) rather than treating the lag length as arbitrary. Because the majority of conditions in the Conditionality Dataset are classified as low-to-medium depth, small or one-off structural steps designed for prompt implementation (Kentikelenis & Stubbs, 2023), the adaptation horizon relevant to these instruments is short, which is why C5 points to one- and two-year, rather than multi-year, reversals.

Layer 2: Selection into Conditionality and its Identification Implication

The conditions a country receives are not assigned at random. They reflect the Fund's diagnosis of the country's binding constraint, the outcome of bargaining between the Fund and domestic authorities (Dreher et al., 2009; Haggard, 2000), and the country's own structural profile. A country entering an arrangement with a distressed external and fiscal position receives debt, fiscal and tax conditions *because* those margins are impaired; a more agrarian economy receives a different bundle than an industrialising one. This selection layer has two consequences that the framework must carry into the empirical design rather than leave implicit.

First, the wide cross-country dispersion in condition counts, from no conditions in China and Myanmar to 486 in Pakistan, is itself an observable footprint of selection on pre-programme distress, not of differences in the Fund's appetite for reform alone. Any credible estimate of conditionality effects must therefore confront the possibility that countries receiving, say, debt conditions were already on a worse productivity trajectory, so that a negative association partly reflects the conditions under which the instrument is deployed.

Second, and more constructively, the selection variables the Fund itself uses, reserves, the fiscal balance, debt service, recent growth and the external position, are observed. Selection that operates through observed pre-programme fundamentals is, by construction, a selection-on-observables problem, which both motivates conditioning on a rich, theory-guided set of macroeconomic and structural controls (rather than an atheoretic "kitchen sink") and motivates the explicit pre-treatment balance and reweighting checks we report in Sect. "Identification Strategy". The two layers thus interlock: Layer 1 specifies *which* sectors should respond to *which* conditions and through what margin; Layer 2 specifies *why* a country carries a given bundle and *what must be held fixed* for the Layer 1 predictions to be identified.

Sector-Specific Predictions

Applying the channel-exposure logic of Sect. "Layer 1: Channels and Sectoral Exposure" to each policy area yields the directional hypotheses summarised in Table 1. We retain the empirical literature as evidence on the sign and strength of individual channels but compress it; a "?" entry denotes a sector in which two channels of opposite sign are predicted to operate, not an absence of theory.

Conditions transmitted through public investment and the state-private boundary (C4): DEB, PRI, SOE. Debt-restructuring conditions bundle expenditure cuts, tighter control of public outlays and limits on new borrowing (Kentikelenis & Stubbs, 2023); the literature links restructurings to weaker growth except for final

Table 1 Sector-specific hypotheses derived from the framework of Sect. "A Framework for the Sectoral Transmission of IMF Conditionality"

Structural reform area	Primary channel(s)	Dominant sector exposure	TFP	Agriculture	Manufacturing	Services
External debt (DEB)	C4 (public investment)	Public-investment-contingent services	–	?	?	–
Fiscal policy (FP)	C2 (formal administration)	Formal sectors; allocative efficiency	+	+	+	+
External sector: trade & exchange (EXT)	C1 (tradability)	Tradable manufacturing; exposed services	+	?	+	+
Financial sector & monetary policy (FIN)	C3 (external finance)	Capital/credit-dependent sectors	+	+	+	+
SOE privatisation (PRI)	C4, C5 (ownership; fixity)	State-supply-linked manufacturing	–	–	–	–
SOE reform & pricing (SOE)	C4 (ownership; restructuring)	State-linked manufacturing/services	+	+	+	+
Revenues & tax policy (RTP)	C2, C3 (formal net; investment)	Capital-intensive formal firms	–	–	–	–
Labour issues (LAB)	C2 (formal regulation)	Formally organised manufacturing/services	+	+	+	+
Social policy (SP)	C2 (human capital; prices)	Wage-employed sectors	+	+	+	+
Poverty reduction (POV)	C2 (human capital; labour supply)	Wage-employed sectors	+	?	+	+
Institutional reforms (INS)	C2 (institutional quality)	System-wide; formal sectors	+	+	+	+
Land & environment (ENV)	Agricultural resource base	Agriculture; derived manufacturing demand	+	+	+	?

Each row states the primary transmission channel (C1-C5, defined in Sect. "Layer 1: Channels and Sectoral Exposure"), the sectoral exposure through which the channel operates, and the predicted sign of the effect on TFP and on productivity in agriculture, manufacturing and services. A "?" denotes a sector in which two channels of opposite sign are predicted to operate. The residual category (RES) carries no directional prediction and is omitted

restructurings (Forni et al., 2021) and warns that front-loaded, expenditure-based adjustment is contractionary when credit is constrained (Baldacci et al., 2015), against a long-run debt-relief benefit (Henri, 2019; Johansson, 2010). Through C4 the contraction is predicted to concentrate in the public-investment-contingent service sector while leaving the established agricultural sector and the private-demand- and trade-driven manufacturing sector comparatively insulated on impact, hence the DEB row $(-, ?, ?, -)$, with the agricultural and manufacturing entries left ambiguous because in each the expenditure contraction and any growth-stabilising benefit pull in opposite directions, so that no sharp contemporaneous sign is predicted for either.

Privatisation removes profit-generating activity from incumbent public suppliers before efficient private replacement matures; through C4 and the fixity channel C5 this predicts a contemporaneous decline that is sharpest in manufacturing and a delayed recovery once restructuring completes (PRI $(-, -, -, -)$ on impact, with the reversal examined in the lagged analysis). SOE reform, by contrast, embeds internal restructuring and gradual price liberalisation (Taghizadeh-hesary et al., 2021), so the same C4 exposure carries the opposite sign (SOE $(+, +, +, +)$). The opposing predictions for PRI and SOE, two instruments often collapsed into a single "real-sector" composite (Kouamé & Tapsoba, 2019), are a direct prediction of the framework and a reason not to aggregate them.

Conditions transmitted through tradability and external finance (C1, C3): EXT, FIN. Trade theory and the bulk of the firm-level evidence associate liberalisation with productivity gains in tradable manufacturing and in internationally exposed services (Krugman & Obstfeld, 2009; Melitz, 2003; Topalova & Khandelwal, 2011; Konte et al., 2022; Balakrishnan et al., 2006), while finding little effect for agriculture. Through C1 this yields EXT $(+, ?, +, +)$, the agricultural entry ambiguous because import substitution against low-comparative-advantage domestic agriculture offsets any efficiency gain. Financial-liberalisation conditions ease access to credit and foreign capital (Prati et al., 2013; Kose et al., 2009; Christiansen et al., 2013; Guillaumont Jeanneney et al., 2006); through C3 the predicted benefit is general but operates most strongly where external-finance dependence is highest, giving FIN $(+, +, +, +)$ with the understanding that the agricultural effect runs through investment finance rather than trade.

Conditions transmitted through formal institutions and administration (C2): FP, RTP, LAB, SP, POV, INS. These conditions act on formal-sector margins and are therefore predicted to transmit where formal organisation exists (manufacturing, formal services) and to attenuate in the own-account agricultural sector. Fiscal administration and transparency reforms raise allocative efficiency where institutions are weak (De Rosa et al., 2015; Lambsdorff, 2003; de Mendonça & Calafate, 2021; Karhunen & Huovari, 2015), predicting FP $(+, +, +, +)$, though the C2 logic already anticipates that the agricultural response may be muted by adaptation costs in loosely administered units. Tax conditions in the region raise rates and broaden bases; the investment-side evidence implies a contemporaneous drag where capital intensity is high (Li et al., 2022; Fang et al., 2022; Muthitacharoen, 2023; Dabla-Norris et al., 2020), predicting RTP $(-, -, -, -)$, with C5 implying a manufacturing reversal once firms adjust and informal units formalise. Flexibility-oriented labour conditions raise measured productivity by easing factor adjustment (Haltiwanger et al., 2014; Kang,

2015; Oecd, 2007; Bjuggren, 2018; Mohommad et al., 2021); since the dataset contains no minimum-wage conditions (Xu et al., 2023), we retain LAB $(+,+,+,+)$ while noting via C2 that the binding response is expected in the formally organised service and manufacturing segments. Social-policy and poverty conditions operate through human capital and the price of basic goods (Becker, 2007; Barrientos, 2012; Li et al., 2023; Yang, 2017; Huang et al., 2023; Ajide & Dada, 2023); through C2 the predicted gains (SP $(+,+,+,+)$; POV $(+,?,+,+)$) accrue to wage-employed manufacturing and services, while the self- and family-employed agricultural sector, where work routines respond little to small income changes, is the ambiguous case for POV. Institutional reforms raise TFP where they bind (Acemoglu et al., 2005; Dreher et al., 2014; Krammer, 2015; Nguyen et al., 2021; Le et al., 2021), with mixed granular evidence (Thanh Dinh & Nguyen, 2023; Khanna & Sharma, 2018), yielding INS $(+,+,+,+)$ as a system-wide prediction whose contemporaneous agricultural component is expected to be weak.

Conditions transmitted through land and the agricultural resource base (C2, agricultural margin): ENV. Land-registry, property-rights and environmental conditions (Kentikelenis & Stubbs, 2023) act directly on the agricultural resource base, with evidence of gains from titling and liberal land reform (Subramanian & Kumar, 2019; Kompas et al., 2012) but losses from restrictive land legislation (Adamopoulos & Restuccia, 2020; Ghatak & Roy, 2007). The predicted incidence is on agriculture and, through derived demand for agricultural technology, manufacturing (ENV $(+,+,+,?)$), the service entry ambiguous because the channel reaches services only indirectly.

The residual category (RES) collects conditions that cannot be assigned to a policy field and therefore to a channel; it carries no directional prediction and is excluded from Table 1, which is why the table lists twelve substantive areas while the empirical model in Sect. "Identification Strategy" estimates all thirteen.

Data and Variable Description

IMF structural conditions For information on structural conditions, we draw on the Conditionality Dataset of Kentikelenis and Stubbs (2023), which provides details on conditions attached to IMF programs in 220 countries since 1980.[2] Structural conditions consist out of prior actions, structural performance criteria and structural benchmarks: Prior actions are binding reform conditions which must be undertaken before the Executive Board approves new financing or concludes a review, they are accordingly assigned to pre-program years. Structural performance criteria are equally binding microeconomic reform conditions which must be undertaken - or require waivers - before the Executive Board concludes a review. Structural benchmarks are non-binding reforms which are intended as markers to assess broader progress on program goals. According to Kentikelenis and Stubbs (2023), the Conditionality

[2] As outlined in the previous section, we elabourate on twelve policy fields, since the thirteenth one is coded as 'residual' because it contains all conditions which could not be allocated to any of the other policy fields.

Dataset is superior to the MONA Dataset because it is based on enhanced data collection and processing efforts, which enable researchers to cover a larger number of countries and a longer time period (data are available from 1980 to 2019).

As indicated in Table 2,[3] the largest number of conditions falls on the financial sector (551), and every programme country faces at least one financial-sector reform. It is followed by taxation (317), the external sector (209), state-owned enterprise reform (195) and fiscal policy (194), policy fields whose effects on economic growth and productivity are comparatively well documented and which may be of primary importance to foreign investors and creditors. In contrast, land and environment (16), poverty reduction (19), debt (24) and social policy (26) conditions are few in number, which may indicate that these areas are either of lesser importance for re-stabilising the economy or that the situation in them is considered acceptable or not urgent to target. Across countries, Pakistan and Mongolia face the most conditional reforms over the period, with 486 and 210 conditions respectively, followed by Sri Lanka (169) and Nepal (154), whereas Thailand (15), the Philippines (33) and South Korea (36) face the fewest among programme countries; China and Myanmar received no structural conditions and enter the analysis as never-treated comparison units. Several of the countries facing comparatively many conditions (Pakistan, Mongolia, Sri Lanka and Nepal) are rarely singled out as particular cases in the reviewed literature, in contrast to South Korea, India and Vietnam, which are comparatively free of quantitative conditionality yet are predominantly investigated.

Productivity indicators Following the methodology of Christiansen et al. (2013), Kose et al. (2009) and Oecd (2013), we utilize Total Factor Productivity as key indicator of economy-wide productivity and channel for long-term economic growth. TFP captures effective output by considering a specified function of (weighted) production inputs, thus adjusting for changes in productivity resulting from factor reallocation (Syverson, 2011). Data on TFP and a broad array of national indicators are sourced from the Asian Productivity Database managed by the Asian Productivity Organization. Although the APO Database encompasses information from 31 Asian countries spanning from 1970 to 2020, our analysis covers fifteen South, East and Southeast Asian countries observed over 1980-2019: the thirteen that received IMF structural conditions during this period, together with China and Myanmar, which received none and therefore enter the analysis as never-treated comparison units. While most indicator values originate from national statistics, yearbooks, or international organizations such as the ILO, OECD, and World Bank, disparities among national reports impede direct cross-country comparisons.[4]

[3] The counts comprise all prior actions, structural performance criteria and structural benchmarks recorded for each country-year over 1980-2019, i.e. the same country-year basis as the condition variables that enter Equation (1); dividing each policy-area total by the 600 country-years in the panel therefore reproduces the per-condition means reported in Table 3 and Appendix A (for example, $551/600 = 0.918$ for the financial sector). On this basis the totals are larger than those in the originally submitted Table 2, which counted distinct conditions and did not reconcile with the estimation sample; the figures reported here are those underlying every estimate in the paper.

[4] Although differences across national reports may complicate interpretative comparisons, since our focus lies on domestic changes in productivity, this aspect should not significantly affect the main analysis.

Table 2 Total number of IMF structural conditions by country and policy area, 1980-2019

Country	DEB	ENV	EXT	FIN	FP	INS	LAB	RES	POV	PRI	RTP	SOE	SP	Total
BGD	4	0	21	46	16	2	1	0	1	2	35	10	0	138
CHN	0	0	0	0	0	0	0	0	0	0	0	0	0	0
IDN	0	1	5	84	7	8	0	0	0	9	8	14	2	138
IND	0	0	15	9	6	1	2	0	0	2	7	8	0	50
KHM	2	12	14	31	16	7	15	0	3	3	24	1	0	128
KOR	0	0	8	25	1	2	0	0	0	0	0	0	0	36
LAO	2	1	12	38	19	2	4	3	0	4	21	5	0	111
LKA	3	0	20	41	20	4	6	0	0	13	34	24	4	169
MMR	0	0	0	0	0	0	0	0	0	0	0	0	0	0
MNG	5	2	10	91	38	7	8	1	1	8	15	16	8	210
NPL	1	0	21	40	15	5	12	2	0	10	32	16	0	154
PAK	7	0	60	94	48	21	3	4	13	12	121	91	12	486
PHL	0	0	9	1	2	2	1	0	0	2	14	2	0	33
THA	0	0	0	14	0	1	0	0	0	0	0	0	0	15
VNM	0	0	14	37	6	0	0	0	1	1	6	8	0	73
Total	24	16	209	551	194	62	52	10	19	66	317	195	26	1741

Each cell is the number of conditions of the respective type imposed on the country over the sample period. RES is the residual category of conditions that could not be attributed to any other policy field (Kentikelenis & Stubbs, 2023)

Next, we use value added per worker in three distinct sectors as indicator for sector-specific productivity similar to (Kouamé & Tapsoba, 2019) and Konte et al. (2022)(see also Sect. "A Framework for the Sectoral Transmission of IMF Conditionality"). For data on sector-specific value added per worker, we consult the World Bank Development Indicator Database which provides this information for the agricultural, manufacturing and service sector, respectively, based on the Standard Industrial Classification system. Value added is defined as the gross output of producers minus the intermediate goods and services consumed in production, before factoring in the consumption of fixed capital during production (WordBank Metadata Glossary, 2023). Value added per worker is calculated by dividing the value added by sector by the number of persons employed in that sector. For this indicator, data are available since 1990 for the majority of countries. Data on employment are obtained from modelled estimates by the ILOSTAT database, where employment is defined as individuals above a certain age who worked or held a job during a reference period (WordBank Metadata Glossary, 2023; International Labor Organization, 2023). This definition includes contributing family workers in family businesses, even if they do not receive a regular wage or salary (ICSE-18 manual). Incorporating informal employment in our measure of labour productivity further accommodates the significant potential for productivity-related transitions from formal to informal employment (or unemployment) due to structural reforms, which could lead to an apparent increase in formal labour productivity in formal employment, albeit possibly at the expense of an (unobserved) overall decline in labour productivity levels in a country.

Summary statistics for the main variables are reported in Table 3; the full set of 50+ controls is documented in Appendix A. On average, TFP slightly declined over the period of interest, with a mean of 0.93 compared to the reference year. The lowest value added per worker is observed in the agricultural sector at 230 million US dollars, which is substantially lower than that of the manufacturing sector (615 million US dollars) and the service sector (767 million US dollars). Intra-sectoral variation (within SD) increases with value added per worker, likely due to more pronounced efficiency differences in sectors that are more dependent on technology and the quality of input factors. Differences in real value added per worker between countries (between SD) are moderate for the agricultural and service sectors but reach a height of 1,794 million US dollars for the manufacturing sector. Unsurprisingly, the agricultural sector employs the largest number of workers, comprising over half of the total workforce in the countries included in our sample. The remaining nine industries each represent a varying percentage of the workforce, ranging from less than one percent (transport, storage and communication; electricity, gas, and water supply) to approximately 13 percent (trade, repair of vehicles and household goods, hotels and restaurant). Our sectors of interest, manufacturing and services, contribute around 11 and 27 percent, respectively.[5] On average, 60 percent of the population in our coun-

[5] Note that Services, according to the International Standard Industrial Classification (ISIC) tabulation categories G-P (revision 3), include wholesale and retail trade and restaurants and hotels; transport, storage, and communications; financing, insurance, real estate, and business services; and community, social and personal services.

Table 3 Compact summary statistics for outcomes and structural conditions

Variable	Source	Unit	Mean	SD	Min	Max
Outcome variables						
Total factor productivity	APO	index	0.9329	0.1925	–	–
Value added/worker, agriculture	WBDI	1mio	230.7	344.0	0.1401	2125
Value added/worker, manufacturing	WBDI	1mio	615.7	600.6	1.140	7412
Value added/worker, services	WBDI	1mio	766.9	938.1	0.7874	4762
Structural conditions (counts; see Appendix A *for totals)*						
DEB	IMF CD	count	0.0400	0.2419	0	2
ENV	IMF CD	count	0.0267	0.2368	0	3
EXT	IMF CD	count	0.3483	1.166	0	10
FIN	IMF CD	count	0.9183	2.673	0	28
FP	IMF CD	count	0.3233	1.035	0	11
INS	IMF CD	count	0.1033	0.5596	0	8
LAB	IMF CD	count	0.0867	0.4894	0	5
POV	IMF CD	count	0.0317	0.2252	0	3
PRI	IMF CD	count	0.1100	0.4778	0	5
RTP	IMF CD	count	0.5283	1.671	0	16
SOE	IMF CD	count	0.3250	1.215	0	13
SP	IMF CD	count	0.0433	0.3027	0	4
RES	IMF CD	count	0.0167	0.1725	0	3
Controls (50+ variables)						

Full descriptive statistics in Appendix A (see Table 10)

The full control set is in Appendix A

tries of interest are of working age (15–64 years), with more than two-thirds residing in rural areas.

Identification Strategy

Understanding the impact of structural reforms on economic development is complicated because stylized (panel) data models commonly used in this field are vulnerable to endogeneity bias (Wei & Zhang, 2010). Subsequently, we present the empirical model, review extant approaches to handle endogenous regressors used in related studies, and present our identification strategy.

Empirical Model

In order to identify the effects of structural conditions on (sector-specific) productivity, we estimate the following model:

$$\begin{aligned}\Delta y_{it} =& \alpha + \mu_i + \delta_0 y_{it-1} + \delta_1 deb_{it} + \delta_2 env_{it} + \delta_3 fin_{it} \\ &+ \delta_4 fp_{it} + \delta_5 ins_{it} + \delta_6 lab_{it} + \delta_7 res_{it} + \delta_8 pov_{it} + \delta_9 pri_{it} \\ &+ \delta_{10} rtp_{it} + \delta_{11} soe_{it} + \delta_{12} sp_{it} + \boldsymbol{X}'_{it}\boldsymbol{\beta} + e_{it},\end{aligned} \tag{1}$$

where Δy_{it} represents a measure of productivity growth (total factor productivity, and sector-specific productivity for the agricultural, manufacturing, and service sector, respectively),[6] and $deb_{it}, \ldots, sp_{it}$ denote the number of structural conditions of a respective type occurring for country *i* in year *t*. The lagged effect of the dependent variable (y_{it-1}) is included to account for effects of initial conditions that determine productivity, such as knowledge accumulation (Oecd, 2013; Nicoletti & Scarpetta, 2003). The vector $\boldsymbol{X}_{it}$ contains the set of control variables (see lower panel in Appendix A), and e_{it} is the error term.

To enhance the model's robustness against endogeneity arising from the correlation between country-specific effects and model regressors, we treat country-specific effects μ_i as fixed effects and eliminate them through a fixed-effects transformation. Finally, the correlations between the structural conditions variables $dep_{it}, \ldots, sp_{it}$ and the error e_{it} are allowed to be nonzero as a reflection of another direction of endogeneity.

Our focus on contemporary rather than medium-term and long-term effects of IMF structural conditions is motivated by the fact that IMF conditions are designed to improve the situation in recipient countries in an urgent manner. More complex reforms being split up into smaller reform conditions, to diminish the risks of reform fatigue and larger government changes due to political cycles (International Monetary Fund, 2018). With the majority of structural conditions having been classified as low-to-medium depth (International Monetary Fund, 2018), with low depth conditions aiming at smaller steps to structural change and medium depth conditions at significant but one-off changes, and given that IMF Conditionality has been criticised as short-run oriented (Buira, 2003; Committeri et al., 2019), a focus on short-term effects seems appropriate.

Handling Endogenous Regressors

Empirical efforts to estimate the effects of structural conditions on economic development often face complications due to the necessity of addressing potential endogeneity biases (Demir, 2022; Stubbs et al., 2020). Potential biases might arise because economic governance may behave differently depending on the type and impact of structural conditions affecting the country (e.g., Kouamé and Tapsoba (2019); Demir (2022)). Even with a rich set of control variables, there might be determinants of productivity that are not accounted for in the model, introducing omitted variables as a second source of endogeneity (e.g., Forster et al. (2019)). Additionally, at macro levels, potential reverse causality from economic development to structural conditions can pose a serious challenge (e.g., Kouamé and Tapsoba (2019); Wei and Zhang (2010)). In particular, a decline in productivity may lead to the triggering of structural conditions (given the overall goal of reforms to increase productivity and thereby the probability of quick paypacks), indicating the presence of adverse feedback effects.

The prevailing approach commonly involves employing a 2SLS/3SLS or GMM estimation technique with suitable instrumental variables (IVs) that meet both exclu-

[6] While y_{it} represents productivity, computing the first differences transforms it into stationary time series, making Δy_{it} a measure of growth.

sion and relevance criteria. To satisfy the exclusion criterion in the present context, the chosen IVs should only impact productivity through IMF structural conditions, while for the relevance criterion, the IVs must be causally linked to IMF structural conditions variables. However, the task of finding suitable IVs that meet both conditions is challenging (see Stubbs et al. (2020) for a review). First studies rely on 2SLS/3SLS with reportedly exogenous instruments (Chapman et al. (2017); Stubbs et al. (2017),[7], incomprehensibly justified diff-in-diff methodology (comparing outcomes between treatment and control group; Wei and Zhang (2010)), side with lagged values of the variable of interest as exogenous predictor and/or a system GMM approach with few macroeconomic covariates (Crivelli & Gupta, 2016; Kouamé & Tapsoba, 2019), or discuss but do not tackle endogeneity concerns at all (Rickard & Caraway, 2019; Woo et al., 2013). In example, Wei and Zhang (2010) estimate the treatment effect of trade conditions based on a comparison of outcomes between IMF program countries with and without trade conditions in their period of interest, while using an augmented gravity model to account for further ways in which these groups may be different. This approach is likely insufficient to account for reverse causality since the value of their outcome variable (trade openness, i.e., bilateral import volume) might have an impact on the decision if a country gets trade conditions or not. Kouamé and Tapsoba (2019) rely entirely on lagged values of the variable of interest and few control variables in response to reverse causality concerns. Endogeneity induced by self-selection into program conditions is discussed and tackled only to a small extent, for example by means of balance checks for seven macroeconomic variables three years prior to program implementation between countries with and without IMF structural conditions (Kouamé & Tapsoba, 2019). We address the potential for time-varying selection on observables, i.e., the possibility that countries enter IMF programmes precisely in years when productivity dynamics are about to change, biasing the contemporaneous coefficients in two complementary ways. The country fixed effects in our main specification absorb time-invariant cross-country differences.

The remaining time-varying selection on observables is investigated in Appendix G, where we compute pre-treatment balance of ten lagged macroeconomic and structural covariates across country-years in which any structural condition was imposed and those in which none was, and re-estimate the four main specifications after Hainmueller (2012) entropy-balancing reweighting. We adopt entropy balancing rather than a quasi-experimental design, i.e., difference-in-differences, an event study, or regression discontinuity, because such designs are ill-suited to the treatment under study: it is a count of conditions imposed in a country-year rather than a binary indicator of programme entry, it recurs within countries as programmes begin and lapse, and the thirteen condition categories arrive jointly with overlapping timing, so there is no clean cohort of newly-treated units in any calendar year on which an event-study could be built. Entropy balancing instead confronts the substantive selection concern directly, that country-years receiving conditions differ in their pre-

[7] Stubbs et al. (2017) use United Nations General Assembly voting affinity with the United States and the total number of countries under IMF programmes as instruments to estimate government health spending. Chapman et al. (2017) rely on the number of countries under an IMF program, the ratio of prior commitments of IMF financing to IMF quota and a dummy for the existence of an extended programme as instruments to project sovereign bond yields.

treatment fundamentals from those that do not, without imposing the cohort structure those designs require.

More recent studies (Chletsos & Sintos, 2023; Demir, 2022; Forster et al., 2019; Stubbs et al., 2020) use a structural equations framework with three simultaneous equations and excludable instruments with maximum likelihood and fixed effects on various levels to account for endogeneity. For example, Chletsos and Sintos (2023) and Stubbs et al. (2020) apply the interaction of the within-country average of IMF conditions with the Fund's budget constraint as compound instrument for IMF conditionality.[8] As potential caveat of this approach, Stubbs et al. (2020) and Demir (2022) mention the requirement of at least moderately strong instruments. The choice of instruments - the natural log of the IMF's liquidity ratios (liquid resources divided by liquid liabilities) (Demir, 2022; Stubbs et al., 2020) or the number countries with an IMF program in a given year (Chletsos & Sintos, 2023) as proxy for IMF budget constraints - is however debatable. For example, the mean of conditions in a specific country could be highly influenced by the circumstances therein which may vary widely over time. While some countries might have resorted to financial mismanagement in earlier year, they might have had comparatively solid finances later on, and thereby receive fewer conditions. The authors rely on the Kleibergen-Paap F-Statistic to justify the strength of their instruments, however this statistic has - in line with the Sargan-Hansen test - been subject to substantial criticism (Kiviet & Kripfganz, 2021). In addition, Stubbs et al. (2020) highlight that multivariate normality is required for errors of structural equations in order to obtain consistent estimates, an assumption that is hardly fulfilled in empirical settings.

While endogeneity biases are a credible threat for standard panel data modelling in the present context, the choice of uncontroversial instrumental information seems complicated. Moreover, even if one would agree on the suitability of particular instrumental variables, their construction might lack feasibility in practice (Haschka et al., 2022). Recently, the handling of potential endogeneity biases has provoked several new approaches beyond traditional instrumental variable techniques (for a comprehensive literature review, see,Ebbes et al. (2009); Papies et al. (2017, 2022)). Against this background, we pursue two alternative approaches. Specifically, we opt for data-based identification approaches to unbiased parameter estimation by considering the widely-employed panel two-stage-least-squares estimators based on heteroskedastic instruments (Lewbel, 2012), and a recently-proposed joint estimation method using copulas (Haschka, 2022). While the common system GMM estimator (Arellano & Bond, 1991) builds on instrumentation based on lagged regressors, the heteroskedasticity-based identification method proposed by Lewbel (2012) allows identification by means of internal instruments without imposing any exclusion restrictions. The copula estimator allows endogeneity-robust estimation without the need for any instrumental variable information (Haschka, 2022, 2025). In the fol-

[8] Covariates include (lagged) values of GDP growth and GDP per capita, inflation, a selected population-level variable (e.g., total or a specific share) and/or a selection of additional control variables depending on the outcome of interest. Stubbs et al. (2020) use current account balance, share of general government final expenditures, trade openness, private credit and annual average exchange rate as additional covariates. Demir (2022) relay on urbanisation, dependency ratio, democracy, government balance and trade. Chletsos and Sintos (2023)

lowing, we provide a brief intuitive explanation of the identification approaches by Lewbel (2012) and Haschka (2022).[9] Table 4 provides a comparison between the approaches employed and contrasts the identification requirements.

Heteroskedasticity-Based Identification Let Y_1, Y_2, and X denote the dependent variable, the endogenous variables, and the exogenous variables, respectively. To build the instruments, we first regress each endogenous variable in Y_2 on the exogenous variables X and retrieve the vector of residuals $\hat{V}$. Subsequently, the instruments are obtained as $(X - \bar{X})\hat{V}$, where $\bar{X}$ is the mean of X. An important requirement for the validity of these instruments is that the residuals $\hat{V}$ are heteroskedastic. In light of the difficulty in finding valid external instruments in empirical research, it is not surprising that a rapidly growing number of studies have applied this identification strategy to deal with endogeneity problems (Gründler & Potrafke, 2019; Tran et al., 2018, 2020). To apply this method to panel data, we follow the procedure suggested by Baum and Schaffer (2021), which involves eliminating country-specific fixed effects by means of within transformation and applying the estimation method suggested by Lewbel (2012) on the transformed data. Controlling for country fixed effects is important to minimise potential endogeneity arising from cross-country heterogeneity.

Copula-Based identification Let Y_2, X, and E denote the endogenous variables, the exogenous variables, and the error, respectively. The goal is to approximate the joint distribution of Y_2, X, and E by starting from the marginals and using Gaussian copula function by means of Sklar's theorem (Sklar, 1959). In the context of modelling panel data, such an assessment is complicated by the fact that panel regressions are typically evaluated after removal of the incidental parameters, i.e. the fixed effects α_i in Equation (1), by means of fixed-effects transformation. Using forward orthogonal deviations transformation, the fixed effects are removed while the error covariance matrix becomes spherical (Haschka, 2022). Assuming normally distributed errors and obtaining the empirical cumulative distribution functions for the observed (transformed) Y_2 and X, the joint distribution then obtains by employing

Table 4 Comparison of identification strategies to handle endogenous regressors (for more detailed summary, see Papies et al. (2022))

Estimator	Identification requirements
Heteroskedastic IV (Lewbel, 2012)	- Endogenous regressor consists of an exogenous heteroskedastic component - No weak instruments
Panel copula (Haschka, 2022)	- Non-normally distributed endogenous regressors - Normally distributed errors - Gaussian-type regressor-error dependence

[9] We initially also implemented the two-step system GMM estimator (Arellano & Bond, 1991; Blundell & Bond, 1998). In this setting it performed unreliably: the structural conditions are sparse counts (Table 3), so their lagged levels and differences are weak internal instruments, and the estimator returned implausibly large, estimator-specific coefficients (for example, social-policy and residual conditions on agricultural value added) that were not corroborated by any other method. Because conclusions should rest on stable rather than estimator-dependent results, we do not report system GMM and base inference on OLS together with the heteroskedasticity-based (Lewbel, 2012) and copula (Haschka, 2022a) estimators.

the probability integral transform on the transformed Y_2, X, and E, which are then plugged into Gaussian copula function Haschka, (2026). The density of this copula is then multiplied with the density of the normal distribution (for the errors), which constitutes the (individual) likelihood function. Taking the logarithm and summing up for all observations, the (log) likelihood is then maximised. Important requirements for the validity of the approach is that the endogenous regressors are non-normally distributed, the errors are normal, and the regressor-error dependence is of Gaussian type. For recent successful application of copula-based identification in related areas, see (Haschka et al. 2022; Haschka & Herwatz, 2020).

Empirical Evidence

The dependent variables are rescaled to the range $[-100, 100]$ so that effect sizes are comparable across estimators, and Tables 5-8 report the results for total factor productivity and for value added per worker in agriculture, manufacturing and services. Because the heteroskedasticity-based estimator relies on internally generated instruments, we report a full identification battery (Appendix C): the Kleibergen-Paap rk Wald F ranges from 192 to 241 across outcomes and every per-condition first-stage F exceeds 19, both far above the Staiger and Stock (1997) threshold of 10; the Kleibergen-Paap LM test rejects under-identification at $p < .001$ in every outcome; and Hansen's J does not reject the exogeneity of the generated instruments (p between 0.23 and 0.51). For the panel copula estimator, the identifying non-normality of the endogenous regressors is strongly confirmed (Shapiro-Wilk and Jarque-Bera $p < 10^{-30}$ for all thirteen conditions, Mardia $p < .001$ jointly; Appendix D), and the residual skewness reported beneath Tables 5-8 is negligible, so the symmetry the estimator requires is satisfied.

The three estimators yield qualitatively similar results, and OLS and the heteroskedasticity-based estimates are close in magnitude throughout; the copula occasionally yields larger point estimates, but the three agree on the sign and significance of all four central findings (the effects identified in Sect. "Robustness to multiple-testing adjustment"), so these do not depend on the choice of estimator. As a byproduct the copula reports estimated correlations between the conditions and the model errors, several of which are significant: evidence that some endogeneity is present and that an endogeneity-robust approach is warranted. That OLS and the heteroskedasticity-based estimates nonetheless remain close suggests the rich set of controls already absorbs much of the associated bias. We now turn to the sector-specific results.

Total factor productivity Table 5 presents the estimation results for total factor productivity. The majority of conditions do not yield a significant impact across all estimators, and when significant effects are observed, their effect sizes remain below 1.3%. In light of related literature findings for various reforms (as indicated in Table 1), this result is unexpected. However, the absence of large effects may be due to contextual differences between IMF structural conditions and ordinary structural reforms, as well as the inclusion of a rich set of control variables, which could have introduced omitted variable bias in other studies using fewer control variables. The fact that all endogeneity robust estimators show little variation in effect

Table 5 The dependent variable is total factor productivity

	OLS		Heteroskedasticity-based (Lewbel, 2012)		Copula (Haschka, 2022)			
	Est.	SE	Est.	SE	Est.	SE	Est. Corr	SE
Deb	0.3953	0.8031	0.3478	0.5266	0.6906	1.168	−0.0644	0.0711
Env	−0.3316	0.6244	−0.7237	0.6176	−0.5902	1.209	0.0092	0.0658
Ext	0.0679	0.1111	0.0806	0.1249	0.0643	0.2432	0.0469	0.0658
Fin	0.0722	0.0902	0.0836	0.0625	0.1199	0.0989	0.0220	0.0691
Fp	−0.2423*	0.1329	−0.2581	0.1620	−0.3947	0.3323	0.0970	0.0765
Ins	0.0695	0.1855	0.0556	0.2385	0.3876	0.4920	−0.0466	0.0659
Lab	0.1807	0.4739	0.1469	0.2919	−0.6024	0.6335	0.1845	0.0659
Res	0.2626	0.9027	0.3425	0.7256	0.0569	1.334	0.0806	0.0720
Pov	0.7807**	0.3293	0.7635	0.6552	1.472	0.9373	−0.0478	0.0729
Pri	0.2191	0.2559	0.2951	0.3262	0.0230	0.6861	0.0508	0.0785
Rtp	−0.1489	0.1350	−0.1424	0.1096	−0.2852	0.1743	0.0753	0.0753
Soe	−0.2459	0.1433	−0.2559*	0.1345	−0.3655	0.2844	0.0283	0.0743
Sp	−0.4679	0.5465	−0.4367	0.4788	0.5005	0.9584	−0.1091	0.0729
y_{t-1}	−0.8942***	0.0311	−0.8794***	0.0154	−0.8591***	0.0150	−0.0850***	0.0197
Control Variables	Yes		Yes		Yes			
Country FE	Yes		Yes		Yes			
Adj. R^2	0.8950		0.8812		–			
N	600		600		600			
KP rk Wald F (weak-ID)			222.7					
KP rk LM p (under-ID)			< 0.001					
Hansen J p (over-ID)			0.5050					
Residual skewness					0.0053			

Columns Est. report estimated effects and SE are corresponding standard errors. The copula estimator further yields correlations between endogenous regressors and the error, shown in column Est. Corr, with corresponding standard errors. Standard errors of the Haschka (2022) estimator are obtained by bootstrapping with 199 replications. Significance levels: *: 10%, **: 5%, ***: 1%, applied to point estimates only. Standard errors are non-clustered. Kleibergen-Paap LM is the p-value of the Kleibergen-Paap rk LM under-identification statistic; Hansen J is the p-value of Hansen's J over-identification statistic for the Lewbel (2012) internal-instrument estimator. The Kleibergen-Paap rk Wald F is the weak-identification statistic; Stock-Yogo critical values are not tabulated for more than three endogenous regressors, so we use the Staiger and Stock (1997) rule-of-thumb threshold of $F > 10$ to indicate sufficient instrument strength; detailed first-stage diagnostics are in Appendix C. For control variables, see Appendix a. The estimation sample comprises fifteen countries over 1980-2019; the total-factor-productivity panel is balanced ($N = 600$), whereas the sectoral value-added panels (Tables 6-8) are unbalanced ($N = 400$, 343 and 331 for agriculture, manufacturing and services) because value-added data begin in 1990 for most countries.

sizes compared to OLS supports this perspective. Moreover, since TFP is considered a‘ general’measure of productivity, the insignificance of most of the conditions underlines the need for a disaggregated approach. By focusing on sector-specific productivity,‘zooming in’is likely to reveal further insights.

Agriculture When looking at agricultural productivity, as documented in Table 6, a higher number of effects become significant and the effect sizes also increase. We observe a significant and negative effect of fiscal policy (FP) conditions on value added per worker across all estimators. This runs counter to the positive sign the framework assigns fiscal-policy conditions in agriculture (Table 1): the allocative-efficiency gain the C2 channel anticipates is evidently outweighed, on impact, by the bureaucratic cost of adapting loosely regulated administrative systems to new monitoring and transparency requirements, so that better fiscal administration is associated with lower, not higher, agricultural productivity in the year of imposition.

Table 6 The dependent variable is value added per worker in the agricultural sector

	OLS		Heteroskedasticity-based (Lewbel, 2012)		Copula (Haschka, 2022)			
	Est.	SE	Est.	SE	Est.	SE	Est. Corr	SE
Deb	−2.119	1.947	−2.903	3.296	2.670	4.895	−0.0646	0.0747
Env	2.935	3.377	4.949	4.121	−2.381	4.815	0.1073	0.0665
Ext	−1.329	1.365	−1.543*	0.9361	−0.3059	1.407	−0.0598	0.0695
Fin	0.8216**	0.3763	0.7539*	0.4088	0.5985*	0.2887	0.0432***	0.0065
Fp	−3.461***	1.004	−2.658**	1.099	−4.734**	1.618	0.0881***	0.0080
Ins	−0.0976	0.8348	−0.6500	1.472	−0.9941	2.087	0.0536	0.0714
Lab	2.804	1.992	1.358	2.026	3.326	3.068	−0.0055	0.0914
Res	4.050	5.869	2.157	4.971	6.032	5.653	−0.0084	0.1003
Pov	−0.9353	1.732	−1.518	4.248	4.773	5.546	−0.0630	0.0921
Pri	−2.753	2.043	−3.201	2.285	0.5010*	0.3008	−0.0480*	0.0257
Rtp	−0.4283	0.5039	0.3012	0.7432	−0.0777	1.042	0.0304	0.0865
Soe	0.5385	1.453	0.9406	1.233	−2.184	1.659	0.0646	0.0844
Sp	2.044	3.312	2.196	3.558	1.856	4.995	0.0252	0.0871
y_{t-1}	−1.155***	0.0494	−1.125***	0.0490	−1.134***	0.0952	−0.0214***	0.0076
Control Variables	Yes		Yes		Yes			
Country FE	Yes		Yes		Yes			
Adj.R^2	0.5177		0.5938		–			
N	400		400		400			
KP rk Wald F (weak-ID)			192.0					
KP rk LM p (under-ID)			<0.001					
Hansen J p (over-ID)			0.487					
Residual skewness					−0.0029			

For further notes, see Table 5

Support programs aimed at assisting with administrative tasks could be beneficial in enabling agricultural workers to manage such tasks without sacrificing significant amounts of productive work. In particular, institutional support should be provided to people in poverty and those with low human capital in order to prevent an increase in social inequality (Chudnovsky & Peeters, 2021).

For financial conditions (FIN), we find a small positive effect that is significant for OLS, Het-IV and panel copula. This effect could be driven by access to (cheaper) private and foreign credit, which facilitates productive investment (such as machinery). Considering that increases in private and foreign direct investment have played a significant role in driving economic development in SESEA (Jaumotte & Spatafora, 2007; Oecd, 2013), it can therefore be inferred that FIN conditions have played a role in contributing to this success.

For external sector (EXT) conditions, our results indicate a negative effect, which is significant for the Het-IV estimator, but not for any of the other estimators. This effect could be explained by the result of substitution effects, i.e. the substitution of uncompetitive domestic agricultural products by (cheaper) foreign imports, and slow labour outflows from the agricultural sector following technological upgrading and structural transformation(Bathla & D'souza, 2015; Binswanger-Mkhize et al., 2009; Fund, 1988; Goretti et al., 2019). While most South and Southeast Asian countries nowadays achieve high international agricultural competitiveness ratings, agricultural commodities in Pakistan, the Philippines and Indonesia still suffered from low competitiveness in 2001 (Zhou & Tong, 2022). It seems that conditional trade liberalisation, while it may have had positive effects in other sectors, does not seem to have improved agricultural productivity. In cases where trade liberalisation is deemed unavoidable, the introduction of incentives or regulations to help firms adapt to changing demand and individuals transition out of agriculture in response to structural change and new technologies could potentially mitigate these adverse effects. Our results contrast with those reported by Kouamé and Tapsoba (2019) on firm-level productivity growth following IMF conditional trade reforms. The inclusion of informal employment in our outcome measures, the focus on the agricultural sector and SESEA, and the addition of a wide range of control variables in our estimation may have caused this divergence, alongside the different identification methods employed. A one-sided focus on firm productivity is therefore likely to be short-sighted in developing countries, as there may be other (informal) workers who do not benefit from reforms but instead lose productivity. Moreover, given that most research on trade reforms has indeed focused on the different sectors of manufacturing, industry and services (see chapter A Framework for the Sectoral Transmission of IMF Conditionality"), it is necessary to adopt a sectoral approach in evaluating existing reforms.

The absence of a significant effect of environmental (ENV) conditions across estimators suggests that environmental and land conditions may not be of contemporary relevance to agricultural production.

The lack of significance observed for labour (LAB), social policy (SP), and poverty (POV) conditions across most estimators suggests that self-employed individuals, as well as family-run businesses, which are the dominant production units in the agricultural sector in SESEA (Organization, 2022), do not directly benefit from

existing conditions. In addition to structural reasons, the challenges of distributing poverty reduction grants in developing countries may be another reason why individuals in rural areas who live far from public institutions in particular do not benefit from contemporary POV conditions. Furthermore, we attribute the lack of an effect of LAB conditions to challenges of labour units to the rather poor adaptation of labour market institutions and regulations to new guidelines (Benson & Zhu, 2011). The lack of a discernible effect of SP conditions is of concern, as it is precisely the increase in basic commodity prices that should increase value added in agriculture. Taking into account the negative impact of external sector reforms, in conjuction with low tariff rates (Fund, 1988) this price increase may have led to a reduction in demand for domestic goods (possibly in exchange for foreign imports) in countries with a low comparative advantage, so that workers in agriculture did not benefit from the reforms. Alternatively, increases in the prices of electricity and other non-agricultural goods may have offset a positive effect,[10] so further reforms that support the agricultural sector in particular, such as improvements in health monitoring and education (Fink & Masiye, 2015; Reimers & Klasen, 2013; Wouterse & Badiane, 2019), may be appropriate.

The absence of an effect of institutional (INS) conditions on value added per worker across all estimators suggests that anti-corruption measures or other institutional framework adjustments may be laudable from a policy perspective, but not for contemporary productivity. Conditions in the residual category (RES), which mainly concern improvements in statistical reporting and programme monitoring (Kentikelenis & Stubbs, 2023) likewise show no significant contemporaneous effect on agricultural productivity in any estimator.

Manufacturing For the manufacturing sector, the results documented in Table 7 show significant and positive effects of ENV, SOE and SP conditions and a significant and negative effect of POV and PRI conditions; revenues-and-tax (RTP) conditions carry a negative coefficient that is significant only in the heteroskedasticity-based estimator. The positive effect of ENV conditions suggests an increased demand for manufactured goods and services in response to forest management requirements and new land rights regulations. The comparatively large effect sizes for SP conditions suggest that more effective transfers and social spending support human capital development, while increases in the prices of basic goods and services may have contributed to increased revenues for firms operating in this segment. In contrast, higher anti-poverty spending has a large negative effect, the opposite of the positive sign the framework assigns POV (Table 1), which could be due to a reduced need to work long hours in (low-paid) jobs as transfers raise reservation wages. While individuals may utilize the time and financial resources afforded by poverty assistance to pursue educational opportunities, personal development, and preventative healthcare, we caution against interpreting this outcome as a policy recommendation advocating for a reduction in expenditure on poverty alleviation.

The adverse impact of PRI conditions implies that entities operating in the manufacturing sector face challenges in promptly adapting to reduced public investments and orders, which results in the persistence of fixed costs coupled with diminished

[10] The significant and positive effects of SP conditions for the other two sectors point in this direction.

Table 7 The dependent variable is value added per worker in the manufacturing sector

	OLS		Heteroskedasticity-based (Lewbel, 2012)		Copula (Haschka, 2022)			
	Est.	SE	Est.	SE	Est.	SE	Est. Corr	SE
Deb	−0.8052	4.352	−0.8445	4.466	−15.46	15.15	0.0930	0.1455
Env	13.05**	4.687	11.00*	5.737	3.946*	1.895	0.1056*	0.0509
Ext	−0.0677	1.174	0.2122	1.319	−1.120	2.255	−0.0584	0.1465
Fin	−0.4746	0.9065	−.5851	0.5757	−0.2132	0.5060	−0.0559	0.7804
Fp	2.009*	0.9384	1.994	1.472	2.641	3.341	−0.1101	1.951
Ins	2.319	1.569	3.119	1.986	2.184	3.985	0.0094	0.0949
Lab	0.6318	3.292	2.588	2.714	1.519	2.205	−0.0413	0.6061
Res	4.109	4.246	2.339	6.505	−1.252	8.057	0.0155	1.585
Pov	−9.627**	4.129	−10.95*	5.604	−18.16**	6.662	0.0216***	0.0091
Pri	−7.662**	2.828	−7.533**	3.009	−8.906**	3.247	−0.0291**	0.0103
Rtp	−1.925	1.220	−2.313**	0.9706	0.4237	1.414	−0.1207	0.9855
Soe	4.450**	1.449	4.254**	1.661	8.759**	3.091	−0.1739**	0.0884
Sp	12.54***	2.56	10.86**	4.662	9.905*	4.561	−0.0509*	0.2627
y_{t-1}	−1.230***	0.0343	−1.176***	0.0527	−0.8423***	0.0881	−0.3025***	0.0092
Control Variables	Yes		Yes		Yes			
Country FE	Yes		Yes		Yes			
Adj.R^2	0.5354		0.6213		–			
N	343		343		343			
KP rk Wald F (weak-ID)			198.1					
KP rk LM p (under-ID)			<0.001					
Hansen J p (over-ID)			0.3774					
Residual skewness					−0.0196			

For further notes, see Table 5

revenue. The comparatively substantial effect size may also be attributed to rigid firing systems and a gradual exodus of labour from SOEs. Early announcements regarding privatization plans may, therefore, prove beneficial in mitigating delayed responses to fluctuations in demand and/or production costs. The negative coefficient on RTP conditions, which reaches significance only in the heteroskedasticity-based estimator, is consistent with the difficulty manufacturing businesses face in adjusting to rising taxation, which leaves them reporting less value added as costs rise; we read it with caution given the disagreement across estimators. Although tax increases may be deemed necessary to alleviate government debt, adopting a more gradual approach, as opposed to implementing stringent measures in the early stages of a program, may be more favourable for ensuring medium-term growth (Baldacci et al., 2015).

In contrast to our expectations, we find no significant impact of FIN and EXT conditions. Initiatives which provide information on sectoral growth opportunities as well as government-backed concessions for investors (e.g., by allowing temporary tax exemptions or reductions) might be beneficial in supporting the manufacturing (and service) sector to reap benefits from increased credit accessibility. Particularly within the context of ongoing IMF programs, acknowledged as indicators of governmental commitment to reform by investors and capital markets (Bird et al., 2004; Woo et al., 2013), governments should actively endorse structural transformation by providing institutional assistance to entities aiming for growth through efficient trade and financial liberalization.

The non-significance of LAB conditions points to a potential inadequacy in the forms of reforms implemented to bolster productivity in manufacturing. As a result, consideration should be given to alternative labour reforms, such as the provision of vocational training opportunities (Betcherman, 2015), either as substitutes or complementary measures to the existing reforms. The absence of a significant effect in FP conditions can be interpreted in two possible ways: either as a success, indicating enhanced administration, or as a failure, if the anticipation was that improved administration would translate into increased profits through enhanced production management. Despite the ambiguity in the interpretation, the enhanced quantification of production, beneficial from governmental and research perspectives, suggests the prudence of maintaining the current form of manufacturing-related FP conditions. Additionally, similar to the findings in the agricultural sector, we observe a non-significant effect of INS conditions.

Services The estimation results for the service sector, as presented in Table 8, indicate a negative impact of RTP conditions on productivity. Despite the smaller effect size observed for services compared to manufacturing, the recommendation to proceed cautiously with tax increases remains unchanged. The need for effective strategies to augment tax revenues in line with IMF requirements, without imposing undue burdens on businesses in developing countries, warrants further investigation. Moreover, the favorable influence of LAB conditions underscores the benefits of more flexible labour markets and reductions in public sector employment and/or income for the service sector.

The substantial and statistically significant adverse effect of DEB conditions across all estimators aligns with findings in research on economic growth (see, e.g. Forni et al. (2021)), however, the ambiguity arises from the exclusive manifestation of this effect in the service sector. Setting aside the large point estimate for the panel copula,[11] the effect sizes of -13.51 percent for OLS and -12.66 for Het-IV could be ascribed to withdrawals or other reductions in public investment in response to debt restrictions. Drawing on the finding by Forni et al. (2021) that debt restructurings have a negative impact on economic growth, we conclude that the service sector seems to be one of the channels through which growth is affected. Since the agricultural sector is considered as well established in developing economies and the manufacturing sector as 'in transformation', especially the service sector might be hit by forborne public

[11] Some estimators yield large point estimates in absolute terms; the associated standard errors indicate considerable dispersion around the point values, i.e. a notable degree of estimation uncertainty.

Table 8 The dependent variable is value added per worker in the service sector

	OLS		Heteroskedasticity-based (Lewbel, 2012)		Copula (Haschka, 2022a)			
	Est.	SE	Est.	SE	Est.	SE	Est. Corr	SE
Deb	−13.51***	3.894	−12.66***	4.400	−31.27***	6.205	0.2233***	0.0591
Env	−4.195	4.549	−6.396	5.430	−5.068	5.929	−0.0212	0.0892
Ext	0.1392	1.175	0.4199	1.228	−1.990	1.691	0.1488	0.0991
Fin	0.3189	0.8780	0.1794	0.6144	0.7532	0.6509	0.0936	0.0882
Fp	0.9410	1.456	0.4124	1.928	1.507	2.001	0.0440	0.0582
Ins	1.217	1.695	2.121	1.901	−1.363	2.207	0.1690	1.473
Lab	8.290**	3.746	7.207***	2.648	7.562***	2.563	0.0643***	0.0214
Res	8.677**	2.974	8.339	6.990	12.58***	2.209	−0.0865***	0.0204
Pov	6.342	6.323	5.886	5.131	4.060	5.091	0.0638	0.0591
Pri	0.2121	1.300	−0.2209	3.099	2.324	2.871	0.0576	0.0680
Rtp	−3.684***	0.8255	−3.582***	0.9622	−3.800***	1.004	0.1113***	0.0301
Soe	1.212	1.815	0.7041	1.680	0.0080	0.0103	0.1032	0.1106
Sp	7.566***	2.189	10.63**	4.725	13.27***	4.441	0.0149***	0.0044
y_{t-1}	−1.110***	0.0340	−1.067***	0.0525	−0.7534***	0.0902	−0.3258***	0.0087
Control Variables	Yes		Yes		Yes			
Country FE	Yes		Yes		Yes			
Adj.R^2	0.5203		0.6111		–			
N	331		331		331			
KP rk Wald F (weak-ID)			240.5					
KP rk LM p (under-ID)			<0.001					
Hansen J p (over-ID)			0.2309					
Residual skewness					−0.0266			

For further notes, see Table 5

investments. The lack of significant effects for EXT and FIN conditions supports this conclusion, indicating that the service sector does not benefit from increased trade openness and financial liberalization. When aiming for structural transformation and considering the relatively higher value added in services compared to sectors like agriculture, governments could potentially benefit from implementing more market-oriented reforms in other areas to support the service sector. Increased productivity could also attract more interest from foreign and private investors, ultimately benefiting service enterprises from corresponding liberalizations.

The nonexistence of significant effects for SOE and PRI conditions may imply that state-owned enterprises in SESEA, which were affected by the reforms, were predominantly not engaged in services or closely associated sectors. This finding deviates from the outcomes presented by Kouamé and Tapsoba (2019), who propose that IMF-conditioned real sector reforms (a composite variable of PRI, SOE, and

LAB conditions) enhance productivity in a sample of firms in both the manufacturing and service sectors. Yet a direct comparison with our estimates is challenging given Kouamé and Tapsoba (2019)'s emphasis on successfully implemented reforms and on firm-level productivity. Their utilization of a restricted set of control variables, a lack of sectoral differentiation beyond sector-fixed effects, and a dependence on a composite variable for PRI, SOE, and LAB conditions adds complexity to any direct comparison. While SOE and LAB conditions have a significant and positive impact, PRI conditions are associated with a decrease in value added per worker in manufacturing.[12] Accordingly, the use of composite instruments should be discouraged. More broadly, we recommend relying on interpretations that are consistent across the OLS, heteroskedasticity-based and copula estimators to mitigate the risk of being driven by any single estimator.

The estimates reported above are contemporaneous point estimates; the remainder of this Sect. "asks which of them are robust. The next four subsections examine, in turn, the statistical power of the design for the sparse conditions, robustness to multiple testing, selection on observables, and a battery of subsample, aggregate-specification and placebo checks, before we turn to effects beyond the year of imposition.

Statistical Power for Sparse Conditions

The conditions differ markedly in how often they are imposed, and with them the power of the design to detect an effect. Appendix E reports the minimum detectable effect (MDE) for each condition-outcome pair, computed from the OLS standard errors at $\alpha = 0.05$ and power 0.80. For the five sparse conditions (RES, ENV, POV, SP and DEB, each imposed in 18 or fewer of the 600 country-years) the sectoral MDEs are large, roughly 5 to 18 in absolute value, so effects of the size typically reported in the structural-reform literature would not be reliably detected. We therefore do not interpret a non-significant coefficient on a sparse condition as evidence of no effect, and we base the discussion and the policy recommendations on condition-outcome pairs that are both adequately powered and consistent across the three estimators.

Robustness to Multiple-Testing Adjustment

We adjust the joint family of $13 \times 4 \times 3 = 156$ contemporaneous tests in Tables 5-8 for multiple comparisons using the Benjamini and Hochberg (1995) false-discovery-rate procedure (Appendix F). All four central findings survive: the negative effects of debt-restructuring (DEB) and revenues-and-tax (RTP) conditionality on services productivity clear $q \leq 0.05$ in every estimator, and the positive effect of state-enterprise reform (SOE) and the negative effect of privatisation (PRI) on manufacturing productivity clear $q \leq 0.10$ in every estimator. Because the 156-test family counts the three estimators of each coefficient as separate hypotheses and assumes almost

[12] A similar conclusion arises for the differentiation between DEB, RTP, FP and SP conditions which are simultaneously treated as'fiscal sector conditions'in Kouamé and Tapsoba (2019) and further studies relying on the MONA database.

all tested effects are null, it is conservative here, so the q-values bound the false-discovery probability from above and the survival of the four findings is the more telling result.

Selection on Observables: Balance and Reweighted Estimates

To address the possibility that countries enter programmes in years when their productivity trajectory is already changing, we compare pre-treatment characteristics of treated and untreated country-years and re-estimate the four main specifications after entropy-balancing reweighting (Hainmueller, 2012). Treatment is defined as the imposition of any structural condition (140 of 600 country-years); balance is assessed on ten lagged macroeconomic and structural covariates, of which six differ by more than $|0.25|$ standardised units before reweighting and all by less than 0.002 afterwards. The reweighted estimates (Appendix G) retain the sign and significance of all four central findings: DEB→Services (-12.87^{***} vs. -13.51^{***} unweighted), RTP→Services (-3.30^{***} vs. -3.68^{***}), SOE→Manufacturing ($+4.33^{***}$ vs. $+4.45^{**}$) and PRI→Manufacturing (-5.33^{*} vs. -7.66^{**}). The associations most sensitive to reweighting are the more tentative ones that did not clear the multiple-testing adjustment uniformly, so the central findings are unaffected.

Further Robustness Checks

In addition to the power, multiple-testing and selection analyses of Sections "Statistical Power for Sparse Conditions-Selection on Observables: Balance and Reweighted Estimates", we conduct a set of robustness checks addressing subsample heterogeneity, alternative specifications, and placebo testing. The four findings identified as strong evidence in Sect. ""Robustness to Multiple-Testing Adjustment" are the focus of these checks; full results are reported in Appendix H.

Income-tier subsamples. Splitting the panel at the median of GDP per capita, DEB→Services, SOE→Manufacturing and PRI→Manufacturing retain their sign and significance in all three estimators in the better-powered middle-income subsample, with larger magnitudes than in the pooled sample. In the smaller low-income subsample SOE→Manufacturing and PRI→Manufacturing keep their sign across estimators and DEB→Services remains negative and significant in the heteroskedasticity-based estimator, while RTP→Services, the smallest effect, stays negative in five of the six subsample-estimator combinations; reductions in significance there reflect the loss of precision in the split.

Aggregate conditionality. Replacing the thirteen counts with their sum produces no significant effect on any sectoral outcome, as expected when category effects of opposite sign offset, which reinforces the case for disaggregation. Interacting the aggregate with log GDP per capita leaves the manufacturing and services coefficients insignificant, so the central findings are not an artefact of income-dependent heterogeneity; the only income gradient robust across estimators is a positive one for agriculture, where aggregate conditionality is less detrimental at higher income.

Leading-condition placebo. Replacing the contemporaneous conditions with their one- and two-period leads tests for anticipation or reverse causation. At the two-

period lead, the cleaner test given mild autocorrelation in the condition series, all four findings are insignificant and smaller than their contemporaneous values, consistent with a causal reading. The one significant lead among the four, the one-period PRI→ Manufacturing coefficient, is consistent with brief anticipation of privatisation and disappears by the two-period horizon. A few leads on non-central conditions remain individually significant at the two-period horizon, as is to be expected when so many coefficients are examined, but none belongs to the four findings on which the conclusions rest.

Beyond Contemporary Effects

Having established the four contemporaneous findings and their robustness, we now ask whether condition effects persist, estimating the impact of structural conditions on productivity one and two years after their imposition. We narrow the focus to sectoral productivity, since TFP was minimally affected by conditionality in the primary analysis. The lagged specifications are estimated with OLS, the heteroskedasticity-based estimator and the panel copula, and the results are presented in Table 9.

To preview, many effects are insignificant and consistent across all estimators. In this regard, the lagged estimates affirm the prevailing view that IMF conditions exhibit strong contemporaneous effects, manifesting immediately rather than with a lag. While this aligns with the IMF's emphasis on prompt action, our earlier estimation results suggest that certain conditions may benefit from longer time horizons to mitigate adverse effects.

Agriculture EXT conditions which previously had a negative and (for Het-IV) significant effect on value added per worker in the agricultural sector now yield only insignificant effects across all estimators, indicating a successful adaptation or at least a sufficient resistance to the enhanced competition. Similarly, the insignificance of FP conditions across estimators points towards a more efficient bureaucratic performance and an improved handling capability of units in the agricultural sector with regard to documentation requirements. The positive and significant effect observed for PRI conditions two years after their imposition suggests that agricultural units can benefit from the privatization of SOEs after an initial period of adaptation. However, we also find a small but negative effect of FIN conditions after one time lag, which could potentially explained by challenges in repaying loans or credits (Friedman et al., 2002; Knowles et al., 1999). Furthermore, a significant effect of POV conditions after one year for HET-IV and copula obtains. This outcome aligns with our earlier findings regarding the contemporary effects of POV conditions on productivity in the manufacturing sector. One plausible explanation is that workers in rural areas have now received their fair share of money and, subsequently, decide to reduce their (paid) workload. As a result, additional incentives to sustain labour supply in the agricultural sector at present levels may be necessary to prevent a reduction in productivity. Alternatively, incentives for increases in human capital might be stimulated to compensate for a reduction in working hours. Furthermore, a negative and significant effect of INS conditions is observed in the year following their directive for Het-IV, though not for the other estimators.

Manufacturing For the manufacturing sector, we find a large and significant negative effect of ENV conditions two years after their imposition for Het-IV, but not for any of the other two estimators. Against their initial large and positive effect, this reverse might hint towards either implementation difficulties, subsequent declines in productivity in favour of increased environmental sustainability or methodological issues due to the small sample size for extant conditions. The positive and significant effects for RTP conditions one and two years after their imposition, significant in the OLS and heteroskedasticity-based estimators, though not in the copula, suggest that units in manufacturing adapt to greater tax burdens and broader tax coverage after an initial shock. Moreover, it is likely that efficient informal units transferred to the formal sector (Besley & Persson, 2014) and that inefficient formal units were either forced to leave the sector or pressured to change to more productive structures in response to greater taxation. The positive and significant effect observed for INS conditions two years post-conditioning may indicate that the implemented reforms have led to improved bureaucratic performance (United Nations, 1999). In conjunction with the potential negative impact on the agricultural sector, it could be concluded that efforts are being made to re-allocate capacity to sectors that generate a higher level of value added. In this case, a more sustainable reorganisation might be preferable. The insignificance of SOE, SP, and POV conditions, which initially yielded either positive or negative significant effects, points to the short-lived nature of IMF conditionality. Prolonged support for reform-willing state-owned enterprises and a more sustainable management of social institutions might contribute to extending the positive effects over a longer period.

Services At first glance, the service sector seems to be minimally affected by IMF conditions beyond immediate effects. Despite documenting several statistically significant effects, all effect sizes remain below one percent, rendering them inadequate as indicators for motivating policy recommendations.[13] Notably, the absence of sustained negative effects for DEB and RTP conditions for more than one year can be interpreted as a success. Moreover, it seems that the service sector quickly adapted to relaxed labour regulations, as indicated by the initially positive but now insignificant effect of LAB conditions, implying that any potential efficiency gains are absorbed within one year.

Synthesis of Contemporary and Lagged Effects

Taken together, the contemporaneous and lagged estimates indicate that most conditions affect sector-specific productivity only in the year of imposition, with two robust exceptions that the framework's fixity channel (C5) anticipates. Taxation conditions weigh on manufacturing on impact (significantly so only in the heteroskedasticity-based estimator) but carry positive coefficients one and two years out in OLS and the heteroskedasticity-based estimator, consistent with lock-in giving way to adaptation; privatisation, negative for manufacturing on impact, is followed by a positive

[13] Although there is a substantial and significant effect of POV conditions in the OLS estimator, the deviation from both other estimators and the size of the standard error do not warrant the formulation of policy recommendations based on this result.

Table 9 Standard errors in parentheses

	Agricultural			Manufacturing			Service		
	OLS	Het-IV	Copula	OLS	Het-IV	Copula	OLS	Het-IV	Copula
Deb_{t-1}	1.891	1.614	1.257	4.020**	3.021	3.094	2.062	0.1060	0.0042
	(2.532)	(2.232)	(1.045)	(1.818)	(3.588)	(3.101)	(4.453)	(.1713)	(0.0993)
Deb_{t-2}	−0.6182	−0.8940	−0.9348	−1.804	3.042	3.161	8.407	0.4116*	0.5510**
	(3.096)	(2.472)	(2.229)	(5.369)	(5.768)	(5.901)	(5.441)	(0.2383)	(0.2102)
Env_{t-1}	−0.8934	2.921	−0.0319	−4.339	−3.475	−3.128	5.342	0.2697	0.4491
	(3.643)	(2.680)	(2.909)	(6.943)	(5.113)	(4.981)	(6.843)	(0.1872)	(0.3919)
Env_{t-2}	−1.215	1.244	1.001	−12.13*	−9.566*	−8.943	−5.892	−0.3245	−0.8988
	(4.058)	(3.498)	(3.255)	(6.727)	(4.884)	(5.202)	(5.762)	(0.2525)	(0.5919)
Ext_{t-1}	0.8632	1.247	1.024	−1.159	−1.742	−2.103	0.5280	0.0151	0.0099
	(0.5565)	(1.020)	(0.9902)	(1.231)	(1.426)	(1.591)	(0.6722)	(0.0359)	(0.0200)
Ext_{t-2}	0.8003	0.8803	0.9063	−0.0557	−0.2204	−0.0136	1.481	0.0689*	0.0441*
	(.9489)	(.9287)	(1.120)	(1.671)	(1.493)	(2.081)	(1.010)	(0.0353)	(0.0258)
Fin_{t-1}	−0.7744*	−0.6906*	−0.6032	0.2114	0.1576	0.0899	−0.1522	−0.0144	0.0091
	(0.4365)	(0.3769)	(0.4021)	(0.6492)	(0.6383)	(0.5578)	(0.5619)	(0.0212)	(0.0184)
Fin_{t-2}	−0.8937	−0.5967	−0.6047	−0.2868	−0.8287	−1.037	−1.126	−0.0529*	−0.0941
	(0.5391)	(0.3970)	(0.3918)	(1.219)	(1.136)	(0.9981)	(0.7047)	(0.0292)	(0.0781)
Fp_{t-1}	−0.6971	−0.5823	−0.5028	0.3099	1.032	1.003	−0.7824	0.0179	0.0549
	(1.401)	(1.065)	(0.9784)	(2.478)	(1.693)	(1,848)	(1.673)	(0.0488)	(0.0581)
Fp_{t-2}	1.012	0.4175	0.4054	−0.0736	1.130	1.1323	−0.6704	0.0193	−0.0572
	(0.8172)	(0.9631)	(0.8467)	(1.834)	(1.232)	(1.951)	(1.546)	(0.0802)	(0.0617)
Ins_{t-1}	−1.642	−2.652*	−2.149	1.093	1.901	2.018	1.552	0.1013	0.2247
	(1.650)	(1.421)	(1.441)	(1.345)	(1.433)	(1.671)	(1.504)	(0.0703)	(0.1571)
Ins_{t-2}	−0.7124	−1.464	−1.093	1.772	3.169*	3.260**	2.959	0.2001*	0.1104
	(1.497)	(1.082)	(1.144)	(2.154)	(1.614)	(1.481)	(2.198)	(.0788)	(.0699)
Lab_{t-1}	−0.5351	−1.524	−1.112	−.6844	0.2123	0.1357	4.199*	0.1007	0.2159
	(1.482)	(1.385)	(1.734)	(3.848)	(2.587)	(2.206)	(1.812)	(0.0816)	(0.1718)

Table 9 (continued)

	Agricultural			Manufacturing			Service		
	OLS	Het-IV	Copula	OLS	Het-IV	Copula	OLS	Het-IV	Copula
Lab_{t-2}	−4.398*	−4.469	−4.307	−5.133	−4.362	−5.517	4.781	0.1301	0.6629
	(2.304)	(1.842)	(1.671)	(5.441)	(3.565)	(3.301)	(2.938)	(0.1032)	(0.4819)
Res_{t-1}	−0.5813	0.5523	0.2629	1.868	−2.615	−2.503	0.6881	−0.2618	−0.0195
	(3.080)	(3.629)	(3.918)	(6.032)	(4.545)	(4.381)	(6.484)	(0.1896)	(0.1501)
Res_{t-2}	−5.088	−4.175	−4.495	1.631	2.636	2.266	16.83	0.4923*	0.3381*
	(4.854)	(3.440)	(5.910)	(4.563)	(7.346)	(7.816)	(11.38)	(0.2945)	(0.1518)
Pov_{t-1}	−7.801	−8.306*	−8.231**	−5.089	−6.976	−6.761	−0.5565	−0.0827	−0.1858
	(3.843)	(4.537)	(4.018)	(6.285)	(5.087)	(6.411)	(5.889)	(0.2104)	(0.2295)
Pov_{t-2}	3.178*	2.895	2.000	1.228	−1.958	−1.551	14.19**	0.5186**	0.3917*
	(4.070)	(3.816)	(4.291)	(4.629)	(5.052)	(4.816)	(6.204)	(0.2124)	(0.2094)
Pri_{t-1}	1.873	1.954	1.805	1.338	−0.3585	−0.3027	−4.277	−0.1237	−0.8711
	(1.991)	(1.607)	(1.983)	(5.376)	(3.229)	(5.174)	(4.518)	(0.1397)	(0.6759)
Pri_{t-2}	3.049*	3.672**	3.837***	8.840	4.735	3.568	−1.330	−0.0919	−0.0471
	(1.619)	(1.578)	(1.893)	(6.595)	(4.328)	(5.581)	(2.865)	(0.0942)	(0.1178)
Rtp_{t-1}	0.3084	0.6116	0.6244	2.352**	1.440*	1.023	−0.3328	−0.0050	−0.1858*
	(0.8625)	(0.6041)	(0.8877)	(0.9829)	(0.8258)	(0.9850)	(0.9241)	(0.0321)	(0.1009)
Rtp_{t-2}	0.3599	0.6894	0.6107	3.407**	2.445**	2.085	0.3208	0.0163	0.1802
	(1.482)	(.6075)	(.9871)	(1.474)	(.9784)	(1.673)	(1.124)	(0.0410)	(0.1591)
Soe_{t-1}	−0.4452	−0.3624	−0.3472	−1.463	0.0934	0.1281	−0.4358	−0.0302	−0.3398
	(1.483)	(1.263)	(1.561)	(1.999)	(1.654)	(1.627)	(1.370)	(.0481)	(.2041)
Soe_{t-2}	−0.7435	−.3863	−.4147	−3.205*	−2.184	−2.118	−3.574	−0.1846***	−0.0984*
	(0.5713)	(0.7920)	(0.6720)	(1.626)	(1.701)	(1.882)	(2.202)	(0.0593)	(0.0556)
Sp_{t-1}	3.754	3.772	3.268	−3.135	−6.646	−5.312	−2.755	−0.2586	−0.5185
	(4.102)	(2.741)	(4.918)	(3.528)	(4.550)	(4.291)	(4.791)	(0.1600)	(0.3782)
Sp_{t-2}	2.816	0.6521	6.014	0.3681	2.372	1.529	9.231	0.2764	0.5570
	(4.288)	(2.825)	(5.911)	(3.503)	(4.554)	(3.891)	(6.760)	(0.1810)	(0.3800)

Table 9 (continued)

	Agricultural			Manufacturing			Service		
	OLS	Het-IV	Copula	OLS	Het-IV	Copula	OLS	Het-IV	Copula
y_{t-1}	-1.183^{***}	-1.138^{***}	-1.157^{***}	-1.207^{***}	-1.145^{***}	$-.8321^{***}$	-1.144^{***}	-1.098^{***}	-1.041^{***}
	(0.0569)	(0.0587)	(0.0591)	(0.0386)	(0.0626)	(0.0668)	(0.0334)	(0.0544)	(0.0615)
Control Variables	Yes			Yes			Yes		
Country FE	Yes			Yes			Yes		

Estimates of correlations between endogenous regressors and errors obtained by the copula estimator are not shown due to space considerations. For further notes, see Table 5

agricultural coefficient at the two-year horizon, consistent with the reallocation of resources from restructured state enterprises. We carry only these sign-stable patterns into the policy discussion of Sect. "Conclusion, where their implications are stated.

Conclusion

This paper asks how the structural conditions attached to IMF programmes affect productivity, and it does so at a level of disaggregation the prior literature has not attempted: thirteen distinct condition types, four productivity outcomes, and three estimators with different identifying assumptions, applied to fifteen South, East and Southeast Asian economies over 1980-2019. Two design choices distinguish the exercise. First, the predictions are derived rather than asserted: the framework of Sect. "A Framework for the Sectoral Transmission of IMF Conditionality" crosses the channel through which each condition operates with the structural characteristics that determine a sector's exposure to that channel, so that sectoral heterogeneity is an ex-ante implication of the theory rather than a pattern rationalised after the fact. Second, because the conditions a country receives are not assigned at random, identification rests not on a single technique but on the agreement of ordinary least squares with two endogeneity-robust estimators (Lewbel, 2012; Haschka, 2022), supplemented by a pre-treatment balance and reweighting analysis, a power assessment, and a multiple-testing adjustment that together separate the findings that can bear weight from those that cannot.

The central empirical result is that IMF conditionality operates on productivity through sectoral reallocation rather than through the aggregate. No structural condition has a robust contemporaneous effect on total factor productivity; the action is entirely sectoral, and an economy-wide lens would miss it. Four sectoral effects constitute the strong evidence of the paper, in the precise sense that each is consistent in sign and significance across all three estimators and survives the false-discovery-rate adjustment in every one of them (Sect. "Robustness to Multiple-Testing Adjustment"): debt-restructuring and revenues-and-tax conditions reduce productivity in services, state-owned-enterprise reform raises productivity in manufacturing, and privatisation lowers it. These four are also the findings that retain their sign and significance under entropy-balancing reweighting (Sect. "Selection on Observables: Balance and Reweighted Estimates") and that satisfy the leading-condition placebo at the two-year horizon (Sect. "Further Robustness Checks"); three of them, the two manufacturing effects and the debt effect on services, reappear with larger magnitudes in the better-powered middle-income subsample, while the smallest, the tax effect on services, is reproduced there only in part (Sect. "Further Robustness Checks"). A second group of associations, the positive financial-sector and negative fiscal-policy effects on agriculture, the negative poverty effect on manufacturing, social-policy, labour and environmental conditions on manufacturing or services, and the residual category on services, are significant under conventional inference, several of them in all three estimators, but do not clear the multiple-testing adjustment in every estimator; we regard them as suggestive rather than as established effects. Most accord with the channels of Sect. "A Framework for the Sectoral Transmission of IMF Condi-

tionality". Two run counter to it: fiscal-policy conditions on agriculture and poverty conditions on manufacturing, each predicted positive but estimated robustly negative, the former attributed above to the cost of administrative adjustment, the latter consistent with poverty transfers reducing labour supply, while the residual category carries no directional prediction by construction. For the sparse conditions, finally, the minimum-detectable-effect calculations (Sect. "Statistical Power for Sparse Conditions") show that a non-significant coefficient cannot be read as a zero, so we draw no conclusion from those nulls.

These findings carry a coherent set of policy implications, which we state prescriptively only for the four effects supported by the strong evidence. Debt-restructuring conditionality concentrates its cost in the public-investment-dependent service sector, exactly as the public-investment channel predicts; where such conditions are unavoidable, sequencing the adjustment so as to preserve public investment, rather than front-loading expenditure cuts, would limit the productivity loss (Baldacci et al., 2015). Revenues-and-tax conditionality likewise reduces services productivity on impact; phasing tax increases in over a longer horizon, rather than implementing them sharply at the start of a programme, would attenuate this effect, and there is suggestive evidence that manufacturing recovers as firms adapt over the following two years. The two instruments that act on the state-private boundary pull in opposite directions and should not be bundled: reform and gradual restructuring of state-owned enterprises raises manufacturing productivity, whereas outright privatisation lowers it contemporaneously, with a delayed agricultural gain emerging two years on. Privatisation plans should therefore be announced and sequenced early, so that affected manufacturing units can adjust, and the common practice of collapsing privatisation, enterprise reform and labour conditions into a single "real-sector" instrument should be avoided, since it conceals effects of opposite sign. More broadly, the heterogeneity we document argues for tailoring conditionality to the sector through which each instrument transmits, and against drawing inferences from aggregate condition counts, which our own aggregate specification shows to be uninformative precisely because opposite-signed sectoral effects cancel (Sect. "Further Robustness Checks").

Limitations and Future Research

Several limitations qualify these conclusions and indicate where further work would be valuable. Our identification addresses selection on observables, through the theory-guided control set, the country fixed effects, and the entropy-balancing reweighting of Sect. "Selection on Observables: Balance and Reweighted Estimates", but it cannot rule out selection on unobservables that varies within countries over time, and the estimates should be read as conditional on that assumption. A further caveat concerns the identification strategy itself. Both endogeneity-robust estimators we employ achieve identification from internal moment and distributional restrictions: heteroskedasticity-generated instruments (Lewbel, 2012) and non-normality of the endogenous regressors (Haschka, 2022), rather than from a source of plausibly exogenous external variation. Their agreement with OLS and with one another is therefore reassuring, but, because all three share a reliance on functional-form and distributional assumptions, it is not equivalent to replication across genuinely inde-

pendent designs. We accordingly read the four central results as robust, sign-stable associations whose causal interpretation is supported by the consistent placebo and reweighting evidence (Sections "Further Robustness checks-Selection on Observables: Balance and Reweighted Estimates"); identification from a natural experiment or an external instrument, where future data permit, would place them on firmer ground. Several condition types are imposed in very few country-years, and for these the design has limited power (Sect. "Statistical Power for Sparse Conditions"); we have been explicit about which nulls are therefore uninformative. The panel copula estimator is identified for continuous endogenous regressors, whereas our condition counts are discrete and sparse; we rely on the simulation evidence that the estimator is robust to this and on the agreement of the copula with the other two estimators, but the approximation remains a caveat (Appendix D). The multiple-testing burden inherent in a disaggregated design is addressed by the false-discovery-rate adjustment, though some residual risk of false positives is unavoidable. Our productivity measures rely on modelled employment series that include informal and contributing family workers; we regard their inclusiveness as a strength, but the underlying data are measured with error. Finally, the fifteen economies we study are precisely the regional growth successes, so the sample is selected on a regional outcome and the magnitudes we report need not carry over to other settings; extending the framework and the disaggregated approach to Latin America, Sub-Saharan Africa and elsewhere is a natural next step, as is the construction of depth- and implementation-weighted condition measures once data of sufficient coverage become available (Table 10).

In sum, a rich and underused panel, a deductive account of why sectors should respond differently, and an identification strategy that leans on the agreement of several methods rather than on any one of them together yield a small number of robust, sector-specific results about IMF conditionality-results that are obscured at the aggregate level and that point to concrete, sector-aware refinements of how structural conditions are designed and sequenced.

Appendices

A. Full Descriptive Statistics

See Table 10.

Table 10 Descriptive statistics

Variable	Data Source	Unit	Mean	SD	Be-tween SD	Within SD	Median	25% Q.	75% Q.	Min	Max
Output variables											
Total factor productivity	APO Database	index	0.9329	0.1925	0.1321	0.1441	0.9647	0.8214	1.043		
Value added per workerin agricultural sector/100	WBDI	1mio	230.7	344.0	300.7	174.9	141.8	82.26	230.2	0.1401	2125
Value added per workerin manufacturing sector/100	WBDI	1mio	615.7	600.6	1794	325.1	448.6	243.7	1018	1.140	7412
Value added per workerin service sector/100	WBDI	1mio	766.9	938.1	820.4	358.5	472.7	335.1	737.4	0.7874	4762
Structural conditions variables											
Deb	IMF CD	count	0.0400	0.2419	0.0557	0.2358	0	0	0	0	2
Env	IMF CD	count	0.0267	0.2368	0.0770	0.2247	0	0	0	0	3
Ext	IMF CD	count	0.3483	1.166	0.3660	1.111	0	0	0	0	10
Fin	IMF CD	count	0.9183	2.673	0.7908	2.562	0	0	0	0	28
Fp	IMF CD	count	0.3233	1.035	0.3575	0.9752	0	0	0	0	11
Ins	IMF CD	count	0.1033	0.5596	0.1346	0.5443	0	0	0	0	8
Lab	IMF CD	count	0.0867	0.4894	0.1195	0.4756	0	0	0	0	5
Res	IMF CD	count	0.0167	0.1725	0.0323	0.1697	0	0	0	0	3
Pov	IMF CD	count	0.0317	0.2252	0.0837	0.2102	0	0	0	0	3
Pri	IMF CD	count	0.1100	0.4778	0.1168	0.4642	0	0	0	0	5
Rtp	IMF CD	count	0.5283	1.671	0.7599	1.500	0	0	0	0	16
Soe	IMF CD	count	0.3250	1.215	0.5704	1.082	0	0	0	0	13
Sp	IMF CD	count	0.0433	0.3027	0.0904	0.2898	0	0	0	0	4
Control variables											
GDP of agricult. sector/1000	APO (on nat.acc.)	1Bio$	0.1497	0.2960	0.2623	0.1527	0.0342	0.0118	0.1070	0.0012	1.798
Share of agricult. GDP in total GDP	Own cal. based on APO	Share	0.2575	0.1240	0.0972	0.0809	0.2480	0.1611	0.3419	0.0194	0.6121
No. of employees in agricult. ind./1000	APO (on labour data)	1,000 people	45.76	92.20	93.47	18.34	10.98	2.688	24.59	0.1949	405.3

Table 10 (continued)

Variable	Data Source	Unit	Mean	SD	Between SD	Within SD	Median	25% Q.	75% Q.	Min	Max
Share of total workforceemployed in agricult. ind.	Own cal. based on APO	Share	0.5330	0.1873	0.1565	0.1104	0.5253	0.4080	0.6724	0.0479	0.9406
GDP of manufact. ind./10	APO (on nat.acc.)	1Bio$	19.96	67.87	42.94	53.69	2.496	0.3641	12.17	0.0110	669.2
Share of manufact. GDP in total GDP	Own cal. based on APO	Share	0.1438	0.0815	0.0744	0.0384	0.1267	0.0742	0.2042	0.0201	0.3543
No. of employees in manufact. ind./1000	APO (on labour data)	1,000 people	12.02	26.88	25.82	9.974	2.639	0.6968	5.819	0.0098	147.7
Share of total workforceemployed in manufact. ind.	Own cal. based on APO	Share	0.1088	0.0492	0.0431	0.0262	0.1103	0.0801	0.1379	0.0053	0.2672
GDP of construct. ind.	APO (on nat.acc.)	1Bio$	61.95	177.6	113.6	139.5	9.698	1.998	40.97	0.0858	1667
Share of construct. GDP in total GDP	Own cal. based on APO	Share	0.0648	0.0433	0.0314	0.0308	0.0565	0.0389	0.0770	0.0043	0.2837
No. of employees in construct. ind./100	APO (on labour data)	1,000 people	51.02	133.1	108.1	82.39	8.723	1.874	24.13	0.0218	752.8
Share of total workforceemployed in construct. ind.	Own cal. based on APO	Share	0.0418	0.0246	0.0167	0.0185	0.0404	0.0235	0.0578	0.0003	0.1306
GDP of mining and quarrying ind.	APO (on nat.acc.)	1Bio$	35.43	87.29	64.52	61.07	4.299	1.090	21.00	0.0058	634.5
Share of mining and quarrying GDP in total GDP	Own cal. based on APO	Share	0.0630	0.1008	0.0871	0.0555	0.0202	0.0087	0.0447	0.0008	0.5634
No. of employees in mining and quarrying ind./10	APO (on labour data)	1,000 people	74.82	196.9	195.5	55.26	6.484	2.176	19.85	0.0028	1078
Share of total workforceemployed in mining and quarrying ind.	Own cal. based on APO	Share	0.0060	0.0065	0.0056	0.0036	0.0041	0.0019	0.0075	5.8E-6	0.0508
GDP of electricity, gas, and water supply in.	APO (on nat.acc.)	1Bio$	19.51	60.81	37.85	48.56	2.080	.4308	13.34	.0151	608.7
Share of electricity, gas, and waterwater supply GDP in total GDP	Own cal. based on APO	Share	0.0179	0.0135	0.0106	0.0088	0.0144	0.0078	0.0251	0.0013	0.1038
No. of employees in electricity, gas,and water supply ind./10	APO (on labour data)	1,000 people	41.38	95.21	90.71	37.08	8.184	1.714	19.64	0.1186	533.7

Table 10 (continued)

Variable	Data Source	Unit	Mean	SD	Be-tween SD	Within SD	Median	25% Q.	75% Q.	Min	Max
Share of total workforcein electricity, gas, and water supply ind.	Own cal. based on APO	Share	0.0047	0.0037	0.0033	0.0019	0.0038	0.0024	0.0057	0.0003	0.0264
GDP of wholesale and retail trade, repair of vehiclesand household goods, hotels and restaurants ind./10	APO (on nat.acc.)	1Bio$	12.49	31.35	19.57	25.00	2.804	0.6679	11.58	0.0218	273.40
Share of wholesale and retail trade, repairof vehicles, and household goods, hotels andrestaurants GDP in total GDP	Own cal. based on APO	Share	0.1530	0.0559	0.0488	0.0300	0.1465	0.1112	0.1972	0.0400	0.3182
No. of employees in wholesale and retail trade, repairof vehicles and household goods, hotelsand restaurants GDP ind./1000	APO (on labour data)	1,000 people	90.09	165.6	141.09	93.90	30.33	7.491	84.53	0.2952	1037
Share of total workforce employed in wholesale andretail trade, repair of vehicles and householdgoods, hotels and restaurants ind.	Own cal. based on APO	Share	0.1279	0.0585	0.0485	0.0351	0.1178	0.0934	0.1639	0.0137	0.2726
GDP of transport, storage and communications ind.	APO (on nat.acc.)	1Bio$	63.21	189.8	118.3	151.5	9.657	2.570	42.41	0.0929	1993
Share of transport, storage and communications ind.GDP in total GDP	Own cal. based on APO	Share	0.0637	0.0289	0.0251	0.0156	0.0646	0.0378	0.0822	0.0170	0.1384
No. of employees in transport, storage andcommunications ind./10	APO (on labour data)	1,000 people	22.24	21.74	33.32	7.237	17.42	1.550	43.89	0.6169	79.52
Share of total workforce employed in transportstorage and communications ind.	Own cal. based on APO	Share	0.0079	0.0084	0.0085	0.0023	0.0045	0.0008	0.0174	0.0002	0.0233
GDP of financial intermediation, real estate,renting and business activities ind./10	APO (on nat.acc.)	1Bio$	15.73	49.52	35.14	36.04	1.803	0.2761	8.008	5.5E-4	445.1
Share of financial intermediation, realestate, renting and business activities ind.in GDP in total GDP	Own cal. based on APO	Share	0.1104	0.0601	0.0572	0.0235	0.1011	0.0741	0.1360	2.2E-4	0.2868

Table 10 (continued)

Variable	Data Source	Unit	Mean	SD	Between SD	Within SD	Median	25% Q.	75% Q.	Min	Max
No. of employees in financial intermediation, real estate,renting and business activities ind./100	APO (on labour data)	1,000 people	11.38	21.01	18.10	11.62	2.959	0.4847	8.891	0.0114	120.95
Share of total workforce employed in financialintermediation, real estate, renting and business activities ind.	Own cal. based on APO	Share	0.0179	0.0238	0.0201	0.0138	0.0093	0.0054	0.0213	0.0007	0.1430
GDP of community, social and personal services ind./10	APO (on nat.acc.)	1Bio$	14.69	45.95	28.67	36.64	2.982	0.3639	10.06	4.2E-6	484.8
Share of community, social and personalservices ind. GDP in total GDP	Own cal. based on APO	Share	0.1496	0.0813	0.0747	0.0373	0.1359	0.0904	0.1931	7.4E-7	0.4976
No. of employees in community, socialand personal services ind./1000	APO (on labour data)	1,000 people	11.46	29.81	29.81	16.93	2.131	.5712	6.261	5.0E-4	226.1
Share of total workforce employed in community,social and personal services ind.	Own cal. based on APO	Share	0.1185	0.0558	0.0439	0.0362	0.1114	0.0810	0.1481	3.3E-6	0.3012
Capital input (capital service)	APO	Index	0.2770	0.4724	0.3097	0.3654	0.0015	0.0004	0.3390	5.0E-4	2.765
IT-realted capital input (capital service)	APO	Index	0.2898	0.8221	0.3327	0.7565	0.0014	0.0001	0.0561	6.7E-7	5.839
Non IT-realted capital input (capital service)	APO	Index	0.2790	0.4674	0.3125	0.3567	0.0015	0.0005	0.3540	5.3E-4	2.647
Number of workers (persons/jobs)/1,000	APO (on Asia-QALI)	1,000 persons	89.02	178.0	178.8	42.85	22.42	7.132	47.86	0.4549	774.5
Hours worked (total employment)/10,000	APO (on Asia-QALI)	1mio hours	18.77	37.28	37.27	9.557	5.257	1.393	10.20	0.0931	168.3
Quality-adjusted labour input	APO (on Asia-QALI)	Index	0.6704	0.3415	0.0598	0.3366	0.6268	0.3774	0.9385	0.0890	1.835
Population/10,000	APO (on pop.c. and nat.acc.)	1,000 people	19.45	35.90	36.16	8.199	5.121	1.701	12.73	0.1248	141.2
Share of male population	APO (on pop.c. and nat.acc.)	Share	0.5040	0.0129	0.0107	0.0078	0.5036	0.4979	0.5147	0.4524	0.5336
Share of female population	APO (on pop.c. and nat.acc.)	Share	0.4959	0.0130	0.0107	0.0078	0.4964	0.4852	0.5021	0.4664	0.5476

Table 10 (continued)

Variable	Data Source	Unit	Mean	SD	Be-tween SD	Within SD	Median	25% Q.	75% Q.	Min	Max
Share of young population (0-14)	APO (on pop.c. and nat.acc.)	Share	0.1527	0.1721	0.1717	0.0451	0.0004	0.0003	0.3263	0.0001	0.4498
Share of adult population (15-64)	APO (on pop.c. and nat.acc.)	Share	0.6031	0.0628	0.0378	0.0509	0.5887	0.5512	0.6561	0.4787	0.7452
Share of old population (>65)	APO (on pop.c. and nat.acc.)	Share	0.0479	0.0194	0.0119	0.0156	0.0408	0.0363	0.0532	0.0237	0.1573
Share of urban population	APO (on pop.c. and nat.acc.)	Share	0.3180	0.1709	0.1545	0.0831	0.2766	0.2061	0.4053	0.0374	0.8194
Share of rural population	APO (on pop.c. and nat.acc.)	Share	0.6820	0.1709	0.1545	0.0831	0.7233	0.5946	0.7939	0.1806	0.9626
GDP deflator	APO (on nat.acc.)	Index	0.3508	0.3395	0.1006	0.3253	0.2352	0.0457	0.6256	3.0E-6	1.733
Private consumption/10	APO (on nat.accc and WB PPP)	1bio$	51.71	116.6	82.25	85.33	13.17	3.083	50.88	0.0662	997.7
Government consumption/10	APO (on nat.accc and WB PPP)	1bio$	15.76	45.44	35.75	29.50	1.886	0.4088	8.823	0.0659	365.1
Gross fixed capital formation/10	APO (on nat.accc and WB PPP)	1bio$	29.03	101.7	61.11	82.72	4.070	0.7419	15.62	0.0301	990.5
Net increase in stocks	APO (on nat.accc and WB PPP)	1bio$	19.74	47.89	35.26	32.72	3.953	0.4752	17.16	-90.15	370.4
Exports of goods and services lessimports of goods and services/10	APO (on nat.accc and WB PPP)	1bio$	19.90	49.98	28.71	41.56	3.828	0.9481	14.12	0.0037	423.1

For indexed variables, 2010 is the reference year (2010 = 1.0)

B. Multicollinearity Diagnostics

To address the possibility of multicollinearity among the thirteen IMF structural conditions, we report pairwise correlations of the condition counts in their raw form (Table 11) and after within-country (fixed-effects) demeaning (Table 12), together with variance inflation factors (VIFs) from the full OLS specification including all controls and country fixed effects (Table 13).

The strongest raw correlations involve State-Owned Enterprise Reform (SOE) with Revenues and Tax Policy (RTP, $r = 0.62$), Social Policy (SP, $r = 0.58$), Fiscal Policy (FP, $r = 0.55$) and Privatisation (PRI, $r = 0.51$), and between Fiscal Policy and Financial Sector conditions (FIN-FP, $r = 0.56$). This pattern reflects the well-documented tendency of IMF programmes to bundle related structural reforms (Kentikelenis & Stubbs, 2023) enterprise restructuring with the fiscal and tax instruments that finance it, and financial-sector reforms with the fiscal policy adjustments that accompany them. No condition pair attains $|r| \geq 0.65$ in either the raw or the within-country correlation matrix; the maximum within-country correlation is $r = 0.55$ (SOE-SP). All thirteen variance inflation factors are below 5 (range 1.54-4.12; mean 2.36), well under the conservative threshold of 5 and the standard threshold of 10. Multicollinearity among the structural condition variables therefore does not pose a credible threat to identification.

Table 11 Raw pairwise correlations among the thirteen IMF structural conditions

	DEB	ENV	EXT	FIN	FP	INS	LAB	RES	POV	PRI	RTP	SOE	SP
DEB	1.00												
ENV	0.13*	1.00											
EXT	0.16*	0.11*	1.00										
FIN	0.36*	0.15*	0.44*	1.00									
FP	0.27*	0.15*	0.44*	0.56*	1.00								
INS	0.17*	0.29*	0.25*	0.37*	0.25*	1.00							
LAB	0.06	0.40*	0.32*	0.27*	0.37*	0.17*	1.00						
RES	−0.02	−0.01	0.26*	0.10*	0.40*	0.02	0.12*	1.00					
POV	0.19*	0.30*	0.21*	0.16*	0.24*	0.27*	0.14*	0.12*	1.00				
PRI	0.12*	0.09*	0.32*	0.45*	0.36*	0.46*	0.42*	0.14*	0.08	1.00			
RTP	0.26*	0.23*	0.54*	0.45*	0.55*	0.29*	0.34*	0.31*	0.55*	0.29*	1.00		
SOE	0.35*	0.03	0.49*	0.52*	0.55*	0.42*	0.21*	0.20*	0.49*	0.51*	0.62*	1.00	
SP	0.32*	0.08	0.32*	0.31*	0.29*	0.26*	0.10*	0.02	0.30*	0.35*	0.46*	0.58*	1.00

* denotes significance at the 5% level

Table 12 Pairwise correlations among the thirteen IMF structural conditions after within-country (fixed-effects) demeaning

	DEB	ENV	EXT	FIN	FP	INS	LAB	RES	POV	PRI	RTP	SOE	SP
DEB	1.00												
ENV	0.13*	1.00											
EXT	0.12*	0.12*	1.00										
FIN	0.34*	0.16*	0.43*	1.00									
FP	0.22*	0.15*	0.41*	0.54*	1.00								
INS	0.14*	0.30*	0.21*	0.34*	0.20*	1.00							
LAB	0.04	0.37*	0.33*	0.27*	0.37*	0.16*	1.00						
RES	−0.04	−0.01	0.24*	0.08	0.38*	−0.01	0.12*	1.00					
POV	0.15*	0.32*	0.12*	0.12*	0.17*	0.21*	0.14*	0.08	1.00				
PRI	0.09*	0.10*	0.30*	0.43*	0.33*	0.44*	0.42*	0.12*	0.04	1.00			
RTP	0.20*	0.26*	0.48*	0.44*	0.51*	0.23*	0.36*	0.28*	0.48*	0.26*	1.00		
SOE	0.32*	0.06	0.43*	0.51*	0.51*	0.38*	0.23*	0.16*	0.41*	0.51*	0.54*	1.00	
SP	0.28*	0.09*	0.28*	0.26*	0.23*	0.22*	0.10*	−0.01	0.24*	0.32*	0.42*	0.55*	1.00

* denotes significance at the 5% level

Table 13 Variance inflation factors of the thirteen structural-condition variables from the full OLS specification (with country fixed effects via dummies and all controls listed in Appendix A)

Condition	VIF	1/VIF
DEB	1.66	0.60
ENV	2.15	0.46
EXT	2.07	0.48
FIN	2.78	0.36
FP	2.76	0.36
INS	1.74	0.57
LAB	2.00	0.50
RES	1.54	0.65
POV	2.13	0.47
PRI	2.38	0.42
RTP	3.33	0.30
SOE	4.12	0.24
SP	2.07	0.48
Mean (13 conditions)	2.36	

All thirteen VIFs are below the conservative threshold of 5

C. Identification Diagnostics: Heteroskedasticity-Based IV

For the heteroskedasticity-based estimator (Lewbel, 2012), Table 14 reports, for each of the thirteen conditions and four outcomes, the first-stage F statistic on the excluded (Lewbel-generated) instruments and Shea's partial R^2. The per-regressor F is the relevant diagnostic with several endogenous regressors, since the joint Kleibergen-Paap statistic can mask weak identification of one regressor when others are well identified (Sanderson & Windmeijer, 2016). All 52 first-stage F statistics lie well above the Staiger and Stock (1997) threshold of 10, from 19.64 (PRI in services) to 387.85 (ENV in services), and the partial R^2 values are correspondingly high (0.73-0.98); instrument exogeneity is supported by the non-rejection of Hansen's J in all four outcomes (p between 0.23 and 0.51).

Table 14 Per-endogenous-regressor identification diagnostics for the Lewbel (2012) estimator, by outcome

	TFP		Agriculture		Manufacturing		Services	
	F	part. R^2	F	part. R^2	F	part. R^2	F	part. R^2
DEB	35.27	0.816	45.63	0.902	42.51	0.914	56.36	0.950
ENV	59.88	0.887	226.79	0.949	244.89	0.952	387.85	0.979
EXT	26.08	0.796	34.26	0.883	29.93	0.895	37.61	0.914
FIN	28.03	0.732	63.82	0.902	66.72	0.927	52.05	0.911
FP	36.98	0.809	52.61	0.927	53.31	0.939	35.76	0.872
INS	57.19	0.871	50.35	0.916	51.96	0.928	64.09	0.959
LAB	42.54	0.818	56.83	0.893	50.87	0.904	49.97	0.922
RES	80.32	0.906	100.82	0.936	110.79	0.950	177.36	0.946
POV	52.09	0.893	73.05	0.939	64.01	0.947	105.85	0.963
PRI	20.96	0.726	30.77	0.851	30.92	0.883	19.64	0.851
RTP	22.77	0.732	54.16	0.921	48.61	0.925	51.80	0.916
SOE	35.96	0.801	35.38	0.881	32.45	0.898	29.27	0.892
SP	39.32	0.823	47.36	0.889	41.12	0.906	69.22	0.911

."F" is the first-stage F statistic for the excluded instruments in the first-stage regression of each endogenous condition. "part. R^2" is Shea's partial R^2

The sparser conditions (ENV, POV, RES, SP) show the largest first-stage F statistics. This is mechanical: Lewbel instruments are products of demeaned regressors and first-stage residuals, and the rare non-zero values of a sparse condition concentrate the heteroskedasticity the instruments exploit. It does not imply substantive power to detect productivity effects of these conditions, which remains limited by the few non-zero observations and is quantified in Appendix E.

D. Identification Diagnostics: Panel Copula

The panel copula estimator of Haschka (2022) requires non-normal endogenous regressors, symmetric errors, and Gaussian regressor-error dependence. The symmetry requirement is met as residual skewness beneath Tables 5, 6, 7, 8 is below 0.03 throughout, and the Gaussian-dependence requirement is documented to be robust to violation (see simulation-based evidence in, Park and Gupta (2012); Haschka (2022). The identifying non-normality of the regressors is examined in Table 15: after within-country demeaning, skewness ranges from 3.71 to 12.23 and excess kurtosis from 22.6 to 176.4, univariate normality is rejected for every condition (Shapiro-Wilk and Jarque-Bera $p < .001$), and joint normality of the thirteen-vector is rejected by Mardia's skewness and kurtosis tests ($p < .001$). The non-normality assumption is thus satisfied by a wide margin.

One feature warrants comment: the conditions are integer counts with a large mass at zero (81-99% zeros), whereas the copula identification result of Park and Gupta

Table 15 Marginal and joint normality diagnostics for the thirteen IMF structural conditions, after within-country demeaning to match the panel fixed-effects transformation

Condition	% zero	Distinct values	Skewness	Excess kurt.	Shapiro p	JB p
DEB	97.0	3	6.11	41.3	<0.001	<0.001
ENV	98.3	3	9.23	104.4	<0.001	<0.001
EXT	86.2	11	4.39	25.4	<0.001	<0.001
FIN	81.2	17	4.21	26.1	<0.001	<0.001
FP	86.7	9	4.35	29.6	<0.001	<0.001
INS	94.5	7	7.72	79.9	<0.001	<0.001
LAB	95.5	6	6.82	56.1	<0.001	<0.001
RES	98.8	4	12.23	176.4	<0.001	<0.001
POV	97.7	4	7.19	70.4	<0.001	<0.001
PRI	93.2	6	5.39	36.9	<0.001	<0.001
RTP	83.5	14	3.71	22.6	<0.001	<0.001
SOE	88.5	11	4.29	30.8	<0.001	<0.001
SP	97.3	5	7.75	75.8	<0.001	<0.001
Joint multivariate normality of the 13-condition vector:						
Mardia skewness statistic (χ^2, df= 455): 82, 016					p <0.001	
Mardia kurtosis statistic (z): 946.9					p <0.001	

"% zero" is the share of country-year observations with no condition imposed. "Distinct values" is the number of distinct integer values the condition takes in the panel. "Excess kurt." is kurtosis minus three. "Shapiro p" is the p-value of the Shapiro-Wilk test of univariate normality; "JB p" is the p-value of the Jarque-Bera test. Mardia skewness and kurtosis statistics jointly test multivariate normality of the 13-vector. All Shapiro-Wilk and Jarque-Bera p-values are below 10^{-30}.

(2012) is derived for continuous regressors. Park and Gupta (2012) show by simulation that the estimator remains unbiased for discrete regressors, including integer counts and Poisson and negative-binomial variables; our data are sparser than their simulated range, so we do not claim their evidence covers our case exactly. Reassurance comes instead from the close agreement of the copula estimates with the OLS and Lewbel (2012) estimates across all four outcomes, the latter well identified by the diagnostics in Appendix C, an independent indication that the copula results are not artefacts of the discrete-regressor approximation.

E. Power Analysis: Minimum Detectable Effects

This appendix reports the minimum detectable effect (MDE) for each of the thirteen conditions in each of the four outcomes. The MDE is the smallest true coefficient detectable at two-sided $\alpha = 0.05$ with power $1 - \beta = 0.80$; for a coefficient with standard error $\widehat{\mathrm{SE}}$ it equals $(z_{1-\alpha/2} + z_{1-\beta})\,\widehat{\mathrm{SE}} \approx 2.802\,\widehat{\mathrm{SE}}$. We compute MDEs from the OLS standard errors; those implied by the heteroskedasticity-based and copula estimators are within 10% throughout. Because the dependent variables are rescaled to $[-100, 100]$ (Tables 5, 6, 7 and 8), the MDEs in Table 16 are directly comparable to the estimated coefficients.

Detection power varies sharply across conditions. For the dense conditions FIN (113 non-zero country-years), RTP (99), EXT (83) and FP (80), the MDEs lie between 1.0 and 4.1 in all sectors, so moderate effects are detectable. For the five sparse conditions RES (7), ENV (10), POV (14), SP (16) and DEB (18), the sectoral MDEs are large, reaching 16.4 (RES, agriculture), 17.7 (POV, services) and 13.1 (ENV, manufacturing), and exceed the magnitude of typical structural-reform effects in the literature. A non-significant coefficient on a sparse condition therefore cannot be read as evidence of no effect; accordingly, the discussion in Sect. "Empirical evidence and the policy recommendations in Sect. "Conclusion do not treat such nulls as zeros and

Table 16 Minimum detectable effects (MDE) by condition and outcome, in the rescaled units of Tables 5, 6, 7 and 8, computed at two-sided $\alpha = 0.05$ and power $1 - \beta = 0.80$ from the OLS standard error of each coefficient

Condition	Non-zero country-years	MDE-TFP	MDE-Agri	MDE-Manuf	MDE-Services
DEB	18	2.25	5.45	12.19	10.91
ENV	10	1.75	9.46	13.13	12.74
EXT	83	0.31	3.82	3.29	3.29
FIN	113	0.25	1.05	2.54	2.46
FP	80	0.37	2.81	2.63	4.08
INS	33	0.52	2.34	4.40	4.75
LAB	27	1.33	5.58	9.22	10.49
RES	7	2.53	16.44	11.90	8.33
POV	14	0.92	4.85	11.57	17.71
PRI	41	0.72	5.72	7.92	3.64
RTP	99	0.38	1.41	3.42	2.31
SOE	69	0.40	4.07	4.06	5.08
SP	16	1.53	9.28	7.17	6.13

"Non-zero country-years" is the count of observations in which the condition was imposed ($1 \leq$ value); 600 country-years are in the panel

Table 17 Robustness of conventionally significant findings to Benjamini-Hochberg FDR adjustment of the joint 156-test family

Hypothesis	Sign	$\hat{\beta}$ range	q_{OLS}	q_{HetIV}	q_{Copula}	Survives
Surviving joint $q \leq 0.05$ in all three estimators						
DEB → Services	−	$[-31.3, -12.7]$	0.010	0.042	<0.001	3/3
RTP → Services	−	$[-3.80, -3.58]$	0.0003	0.005	0.005	3/3
Surviving joint $q \leq 0.10$ in all three estimators						
SOE → Manuf.	+	$[+4.25, +8.76]$	0.033	0.074	0.045	3/3
PRI → Manuf.	−	$[-8.91, -7.53]$	0.050	0.083	0.050	3/3
Surviving joint $q \leq 0.05$ in at least one estimator						
FP → Agri.	−	$[-4.73, -2.66]$	0.010	0.101	0.039	2/3
RES → Services	+	$[+8.34, +12.58]$	0.039	0.584	<0.001	2/3
SP → Services	+	$[+7.57, +13.27]$	0.010	0.132	0.039	2/3
ENV → Manuf.	+	$[+3.95, +13.05]$	0.049	0.233	0.170	1/3
SP → Manuf.	+	$[+9.91, +12.54]$	<0.001	0.111	0.146	1/3
LAB → Services	+	$[+7.21, +8.29]$	0.140	0.050	0.039	1/3
Significant in ≥ 1estimator (consistent sign) but not surviving FDR in any						
POV → Manuf.	−	$[-18.16, -9.63]$	0.110	0.220	0.050	0/3
POV → TFP	+	$[+0.76, +1.47]$	0.107	0.584	0.378	0/3
FIN → Agri.	+	$[+0.60, +0.82]$	0.146	0.261	0.170	0/3
FP → Manuf.	+	$[+1.99, +2.64]$	0.153	0.523	0.715	0/3
SOE → TFP	−	$[-0.37, -0.25]$	0.328	0.234	0.544	0/3
FP → TFP	−	$[-0.39, -0.24]$	0.266	0.378	0.584	0/3
EXT → Agri.	−	$[-1.54, -0.31]$	0.675	0.360	0.906	0/3

"Sign" indicates the sign of the point estimate, consistent across the three estimators in every row. "$\hat{\beta}$ range" gives the minimum and maximum point estimate across OLS, the Lewbel (2012) estimator and the Haschka (2022) estimator. "Survives" is the number of estimators in which the hypothesis attains $q \leq 0.05$, except in the second block, where it is the number attaining $q \leq 0.10$. All four central findings clear $q \leq 0.10$ in all three estimators, and the first two clear $q \leq 0.05$ in all three. The final block collects pairs significant in at least one estimator under conventional inference whose q-value exceeds 0.05 in every estimator; of these, only POV→Manufacturing attains $q \leq 0.10$ (in the copula)

restrict prescriptions to condition-outcome pairs that are both adequately powered and consistent across the three estimators.

F. Robustness to Multiple-Testing Adjustment

The four results tables in Sect. "Empirical evidence report $13 \times 4 \times 3 = 156$ contemporaneous coefficient tests. To gauge how the principal results withstand multiple testing, we compute the false-discovery-rate q-value of Benjamini and Hochberg (1995) for each: ordering the $m = 156$ two-sided p-values as $p_{(1)} \leq \cdots \leq p_{(m)}$, the q-value of the k-th smallest is $q_{(k)} = \min_{j \geq k} m\, p_{(j)}/j$. Table 17 reports, for every condition-outcome pair significant in at least one estimator with a sign consistent across the three estimators, its FDR q-value in each estimator; two further pairs significant in a single estimator only (PRI on agriculture, RTP on manufacturing) carry opposite signs across estimators and are omitted as unstable.

The four central findings survive: DEB→Services, RTP→Services, SOE→Manufacturing and PRI→Manufacturing all clear $q \leq 0.10$ in all three estimators, with consistent signs, and the two services effects clear $q \leq 0.05$ in all three.

Two features make this a demanding test in our setting. The joint family treats the three estimators as independent tests of distinct hypotheses, whereas they are three estimates of the same parameter, so each finding is in effect counted three times against the family size, an overcounting that inflates the q-values. And the procedure controls the false-discovery rate under the assumption that almost all tested hypotheses are true nulls (Benjamini & Hochberg, 1995; Storey, 2002); here the thirteen categories are the ones the literature treats as policy-relevant, so that assumption

Table 18 Pre-treatment balance of selected lagged macroeconomic and structural covariates by treatment status ($T_{it} = 1$ if any IMF structural condition imposed in country *i* year *t*)

	Before reweighting			After entropy balancing		
Variable	Mean (T=1)	Mean (T=0)	Std. diff.	Mean (T=1)	WgtMean (T=0)	Std. diff.
$L.$ log TFP	1.846	1.986	−0.182	1.846	1.857	−0.000
$L.$ log population	10.471	11.043	−0.351	10.471	10.472	−0.000
$L.$ share urban	0.314	0.339	−0.145	0.314	0.314	−0.000
$L.$ log GDP deflator	−1.553	−1.794	0.144	−1.553	−1.554	0.001
$L.$ log private cons.	4.502	5.062	−0.309	4.502	4.502	−0.000
$L.$ log govt cons.	2.661	3.389	−0.394	2.661	2.662	−0.000
$L.$ log gross fixed inv.	3.251	3.955	−0.353	3.251	3.251	−0.000
$L.$ log exports	3.161	3.832	−0.352	3.161	3.161	−0.000
$L.$ share agri. GDP	0.255	0.236	0.162	0.255	0.255	−0.000
$L.$ share manuf. GDP	0.133	0.154	−0.273	0.133	0.133	−0.000

The right-hand block reports balance after the Hainmueller (2012) entropy-balancing procedure reweights $T = 0$ observations to match $T = 1$ first moments on the listed covariates. Standardised differences are computed as $(m_1 - m_0)/\sqrt{(s_1^2 + s_0^2)/2}$. Before reweighting, six of the ten standardised differences exceed the conservative Imbens and Rubin (2015) concern threshold of |0.25|; after entropy balancing the maximum absolute standardised difference is below 0.002, i.e. first-moment balance is achieved by construction

understates the share of genuine effects and pushes the adjustment toward more conservative, familywise behaviour. The reported q-values are therefore best read as an upper bound on the false-discovery probability, and the survival of the four findings under this stricter-than-necessary standard is the substantive point.

G. Selection on Observables: Balance and Reweighted Estimates

Countries enter IMF programmes endogenously, and treatment intensity may be correlated with prior macroeconomic conditions that also affect productivity. Country fixed effects absorb time-invariant cross-country differences but not time-varying selection on observables. We address the latter in two steps: a pre-treatment balance comparison, and re-estimation of the four main specifications after Hainmueller (2012) entropy-balancing reweighting, which forces the weighted means of the untreated country-years to match those of the treated.

We define treatment at the country-year level, that is, $T_{it} = 1$ if any of the thirteen conditions was imposed on country i in year t, and $T_{it} = 0$ otherwise, which holds for 140 of the 600 country-years (23.3%). Balance is assessed on ten lagged covariates that the conditionality literature (Kentikelenis & Stubbs, 2023; Vreeland, 2003) treats as predictors of programme entry: lagged log TFP, log population, urban share, log GDP deflator, log private and government consumption, log gross fixed investment, log exports, and the agricultural and manufacturing GDP shares. Table 18 reports the comparison.

Table 19 OLS estimates of condition effects, original unweighted (left panel, identical to Tables 5, 6, 7 and 8) and after Hainmueller (2012) entropy-balancing reweighting on the ten pre-treatment covariates listed in Table 18 (right panel)

	Original OLS (unweighted)				Entropy-balanced OLS (reweighted)			
	TFP	Agri	Manuf	Services	TFP	Agri	Manuf	Services
DEB	0.395	−2.119	−0.805	−13.51***	0.269	−3.083	−1.253	−12.87***
ENV	−0.332	2.935	13.05**	−4.195	−0.265	3.399	15.02***	−1.787
EXT	0.068	−1.329	−0.068	0.139	0.088	−1.287*	0.485	0.422
FIN	0.072	0.822**	−0.475	0.319	0.033	0.948***	−0.089	0.221
FP	−0.242*	−3.461***	2.009*	0.941	−0.258**	−3.051***	1.710	−0.271
INS	0.069	−0.098	2.319	1.217	0.032	−0.039	2.319	0.064
LAB	0.181	2.804	0.632	8.290**	0.167	3.171**	−1.044	6.709***
RES	0.263	4.050	4.109	8.677**	.504	3.464	1.896	9.875*
POV	0.781**	−0.935	−9.627**	6.342	0.839*	0.239	−9.882**	5.526
PRI	0.219	−2.753	−7.662**	0.212	−0.030	−3.070*	−5.325*	0.879
RTP	−0.149	−0.428	−1.925	−3.684***	−0.143*	−0.931	−1.728*	−3.303***
SOE	−0.246	0.539	4.450**	1.212	−0.102	0.785	4.331***	1.640
SP	−0.468	2.044	12.54***	7.566***	−0.408	2.737	8.426*	5.065
N	600	400	343	331	600	400	343	331
$\sum w$	—	—	—	—	280.0	200.9	180.4	164.0

Significance: *: 10%, **: 5%, ***: 1%. Full specifications include the controls listed in Appendix A and country fixed effects. $\sum w$ is the sum of weights and indicates the effective weighted sample size

Before reweighting, six of the ten standardised differences exceed the conventional |0.25| threshold; entropy balancing reduces all ten below 0.002 in absolute value. Table 19 compares the unweighted OLS estimates with the reweighted estimates. The four central findings retain their sign and conventional significance:

- DEB→Services: -13.51^{***} unweighted, -12.87^{***} reweighted;
- RTP→Services: -3.68^{***}, -3.30^{***};
- SOE→Manufacturing: $+4.45^{**}$, $+4.33^{***}$ (strengthened to the 1% level);
- PRI→Manufacturing: -7.66^{**}, -5.33^{*} (significant at 10%, magnitude reduced).Three of the four are unchanged or strengthened; PRI→Manufacturing weakens to the 10% level but retains its sign and a sizeable negative magnitude. The core pattern, that is debt and tax conditionality reducing services productivity, state-enterprise reform raising and privatisation lowering manufacturing productivity is unaffected by the reweighting.

Among the remaining, more tentative associations, several strengthen (ENV→Manufacturing and LAB→Services to the 1% level, FIN→Agriculture), and FP→Agriculture holds at the 1% level. Those that weaken, RES→Services, SP→Manufacturing and SP→Services, the last losing significance, were each significant in only one or two estimators to begin with. Reweighting thus leaves the well-supported findings intact and unsettles only the associations that were already marginal.

H. Robustness Checks

To assess the robustness of our main findings we conduct three additional checks: estimation on income-tier subsamples, an alternative specification based on aggregate conditionality, and a leading-condition placebo. We focus on the four findings reported in Sect. "Robustness to multiple-testing adjustment, i.e., the negative effects of debt-restructuring (DEB) and revenues-and-tax (RTP) conditionality on services productivity, and the positive and negative effects of state-enterprise reform (SOE) and privatisation (PRI) on manufacturing productivity. Results are reported in Tables 20, 21 and 22.

H.1 Subsamples by Income Tier

We split the panel at the pooled-panel median of GDP per capita, classifying country-years below the median as low-income and those at or above it as middle-income; this yields 246 low-income and 354 middle-income observations (twelve and fifteen countries, respectively, three of them middle-income throughout). Social-policy conditions (SP) do not occur in any low-income country-year and are omitted from that subsample. Estimates for all three estimators are reported in Table 20.

In the middle-income subsample, where the conditioned reforms are concentrated, the three findings located there reproduce the main results: DEB→Services, SOE→Manufacturing and PRI→Manufacturing retain their sign and conventional significance in all three estimators, with magnitudes somewhat larger than in the pooled

specification (DEB→Services between -18.3 and -35.5; SOE→Manufacturing between $+5.7$ and $+13.0$; PRI→Manufacturing between -12.6 and -21.7). RTP→Services, the smallest of the four effects, is negative in five of the six subsample-estimator combinations and significant in the low-income OLS and middle-income heteroskedasticity-based specifications; its modest magnitude makes it the most sensitive to the loss of precision when the sample is split.

In the low-income subsample (246 observations, where the internal-instrument identification is weaker and the estimates correspondingly less precise), the findings are supported where the data permit: DEB→Services and PRI→Manufacturing are negative and significant in the identification-robust heteroskedasticity-based estimator (-18.40 and -16.05), RTP→Services is negative and significant in OLS, and SOE→Manufacturing is positive in all three estimators. The remaining low-income estimates are imprecise, as expected with the reduced sample.

The remaining associations are consistent with the main analysis: the negative fiscal-policy effect on agriculture and the positive financial-sector effect on agriculture keep their sign in both subsamples and attain significance in the middle-income heteroskedasticity-based estimator, while the contemporaneous TFP coefficients remain individually insignificant in both splits, in line with the absence of an aggregate productivity effect.

H.2 Aggregate Conditionality and Interaction with Per-Capita Income

As an alternative to the thirteen separate counts we use their sum, $\mathrm{scstot}_{it} = \sum_k c_{it}^k$, and, to test whether any aggregate effect varies with development, a specification adding log GDP per capita and its interaction with the aggregate. Country fixed effects absorb the level of income, so the interaction is identified by within-country variation in income over the period. Table 21 reports both specifications (Panel A without, Panel B with the interaction).

In Panel A the summed measure is insignificant for all three sectoral outcomes in every estimator and only marginally negative for total factor productivity. This is what the disaggregated analysis implies: because condition categories move different sectors in opposite directions: debt and tax conditionality lower services productivity, state-enterprise reform raises and privatisation lowers manufacturing productivity, they offset under summation, so the aggregate measure conceals exactly the heterogeneity the paper documents.

In Panel B the aggregate effect on agriculture rises with income (positive and significant in all three estimators, OLS $+0.747^{***}$), so aggregate conditionality is less detrimental to agriculture at higher income. For manufacturing and services, the outcomes that carry the four central findings: the interaction is insignificant in every estimator, so those findings are not driven by income-dependent heterogeneity at the aggregate level. For total factor productivity the interaction is significant only in the copula estimator and is read with corresponding caution.

Table 20 Subsample regressions by income tier

	Pooled			Low income			Middle income		
	OLS	HetIV	Copula	OLS	HetIV	Copula	OLS	HetIV	Copula
Total factor productivity									
DEB	0.395	0.348	0.691	0.476	0.551	−7.669	0.512	0.217	10.20**
ENV	−0.332	−0.724	−.590	0.529	0.048	8.556***	4.477**	3.660**	1.838
EXT	0.068	0.081	0.064	0.009	0.110	0.871*	0.095	0.070	1.059
FIN	0.072	0.084	0.120	0.030	0.082	0.384	0.070	0.086	−0.260
FP	−0.242*	−0.258	−0.395	−0.026	−0.095	−0.502	−0.316*	−0.423***	−0.949
INS	0.070	0.056	0.388	0.524	0.387	6.272**	0.020	0.091	0.288
LAB	0.181	0.147	−0.602	0.402*	0.369	−0.512	−0.167	−0.195	−0.096
RES	0.263	0.343	0.057	1.468*	1.686*	4.767	−1.141	−0.810	1.230
POV	0.781**	0.764	1.472	−1.697*	−1.753*	−20.17***	1.215*	1.147*	3.380
PRI	0.219	0.295	0.023	−0.504	−0.474	2.992	0.098	0.011	1.747
RTP	−0.149	−0.142	−0.285	−0.104	−0.203	−0.215	0.091	0.107	−0.406
SOE	−0.246	−0.256*	−0.366	0.012	0.032	−0.760	−0.521**	−.432**	−1.862**
SP	−0.468	−0.437	0.501	−0.620	−0.159	6.628	−0.848	−0.764	−3.591
Agriculture									
DEB	−2.119	−2.903	2.670	−19.78	.016	−39.65	−3.077	−3.740	10.90
ENV	2.935	4.949	−2.381	11.21	−8.656	−6.626	15.96	0.138	−4.905
EXT	−1.329	−1.543*	−0.306	−3.866	−5.384***	1.613	0.061	0.381	0.347
FIN	0.822**	0.754*	0.599*	−3.090	0.012	−6.373	1.126**	1.129***	−0.149
FP	−3.461***	−2.658**	−4.734**	13.17	1.848	16.67	−1.809	−2.226*	−2.792
INS	−0.098	−0.650	−0.994	−15.69	−2.654	−1.129	−0.340	−0.092	−1.441
LAB	2.804	1.358	3.326	6.106	13.98***	18.02*	5.761	3.903	1.522
RES	4.050	2.157	6.032	−53.85	−4.559	−17.61	−2.237	0.128	−1.218
POV	−0.935	−1.518	4.773	36.58**	34.64***	22.03	3.457	1.961	−7.653
PRI	−2.753	−3.201	0.501*	−20.41	−24.25***	−15.24	−0.536	−0.627	3.497
RTP	−0.428	0.301	−0.078	5.267	0.726	3.920	−.385	0.095	0.606
SOE	0.539	0.941	−2.184	3.481	11.15***	−9.368	−1.505	−0.492	−1.558

Table 20 (continued)

	Pooled			Low income			Middle income		
	OLS	HetIV	Copula	OLS	HetIV	Copula	OLS	HetIV	Copula
SP	2.044	2.196	1.856	—	—	—	−3.142	−3.655	−4.875
Manufacturing									
DEB	−0.805	−0.844	−15.46	−28.24	−21.20*	−25.73	−1.641	−2.410	8.479
ENV	13.05**	11.00*	3.946*	15.32	21.28**	49.94*	−9.815	1.844	−16.89
EXT	−0.068	0.212	−1.120	1.200	1.589	−2.848	−1.963	−2.654	4.742
FIN	−0.475	−0.585	−0.213	−1.321	−1.434	1.890	−0.362	−0.101	−1.578
FP	2.009*	1.994	2.641	3.896	8.517	−8.689	−2.717	−2.259	−0.065
INS	2.319	3.119	2.184	−4.315	−9.076	−29.52	3.517	3.986*	9.288*
LAB	0.632	2.588	1.519	5.988	−3.929	4.379	17.93**	19.84***	31.07**
RES	4.109	2.339	−1.252	47.51	−12.05	24.38	15.77	14.52	−24.52
POV	−9.627**	−10.95*	−18.16**	35.53	29.39**	52.62	−19.45**	−20.39***	−43.70**
PRI	**−7.662****	**−7.533****	**−8.906****	−31.02	−16.05*	−11.46	**−12.55****	**−13.87*****	**−21.68***
RTP	−1.925	−2.313**	0.424	−0.802	−0.963	−0.576	−1.862	−2.182	−3.096
SOE	**4.450****	**4.254****	**8.759****	7.589	4.208	15.89	**6.246****	**5.696****	**13.00*****
SP	12.54***	10.86**	9.905*	—	—	—	13.30	13.81**	30.07
Services									
DEB	**−13.51*****	**−12.66*****	**−31.27*****	10.14	**−18.40*****	−1.283	**−19.92*****	**−18.25*****	**−35.46****
ENV	−4.195	−6.396	−5.068	−11.58	−2.140	8.993	−15.83	−16.86	95.84
EXT	0.139	0.420	−1.990	1.106	−0.798	−0.569	0.620	−0.153	−5.311
FIN	0.319	0.179	0.753	−0.839	−2.850***	0.005	1.132	1.345	−0.127
FP	0.941	0.412	1.507	1.543	4.701	0.460	−3.614	−4.422	−0.674
INS	1.217	2.121	−1.363	6.484	3.016	5.144	3.006	3.831	−4.132
LAB	8.290**	7.207***	7.562***	3.578	7.690***	7.114	0.365	9.729	−57.20
RES	8.677**	8.339	12.58***	4.732	24.18**	38.16	15.12	17.82	24.63
POV	6.342	5.886	4.060	−11.93	−0.175	−15.77	7.448	6.803	31.20
PRI	0.212	−0.221	2.324	6.638	−4.230	−0.908	−5.264	−6.690	1.427

Table 20 (continued)

	Pooled			Low income			Middle income		
	OLS	HetIV	Copula	OLS	HetIV	Copula	OLS	HetIV	Copula
RTP	**−3.684*****	**−3.582*****	**−3.800*****	−5.542*	−1.967	−3.035	−1.549	−2.855*	0.343
SOE	1.212	0.704	0.008	−0.870	−0.221	12.55	1.051	1.269	−2.730
SP	7.566***	10.63**	13.27***	—	—	—	3.401	6.731	2.121
N	600	600	600	246	246	246	354	354	354

Country-years with GDP per capita below the pooled-panel median are classified as low income; those at or above the median as middle income. Each cell reports the coefficient on the corresponding condition variable from the four main specifications, by estimator. Significance: *: 10%, **: 5%, ***: 1%. Coefficients on the four central findings of the paper (DEB→Services, RTP→Services, SOE→Manufacturing, PRI→Manufacturing) are in bold. "—" indicates that the corresponding condition was omitted from the specification due to no variation in the relevant subsample (SP in the low-income subsample). The N column shows the number of country-year observations in each estimator-sample combination

Table 21 Aggregate-conditionality specification with and without per-capita-income interaction

	OLS	Heteroskedasticity-based	Copula
Panel A. Aggregate conditionality only			
TFP scstot	-0.0345^{*} (0.0178)	-0.0328^{*} (0.0176)	-0.0596 (0.0669)
Agri scstot	-0.2231 (0.1475)	-0.0999 (0.1085)	-0.2876 (0.2574)
Manuf scstot	$+0.2179$ (0.2015)	$+0.1406$ (0.1681)	$+0.4956$ (0.3570)
Serv scstot	$+0.0049$ (0.2058)	-0.0208 (0.2049)	$+0.2642$ (0.3354)
Panel B. Aggregate conditionality with per-capita-income interaction			
TFP scstot_c	-0.0115 (0.0130)	-0.0334^{*} (0.0176)	-0.0867 (0.0617)
TFP $\text{scstot}_c \times$ ln gdppc	—	-0.0498 (0.0316)	-0.2636^{***} (0.0654)
Agri scstot_c	-0.2787^{*} (0.1484)	-0.1458 (0.1245)	-0.1123 (0.2532)
Agri $\text{scstot}_c \times$ ln gdppc	$+0.7468^{***}$ (0.2700)	$+0.6228^{***}$ (0.2245)	$+0.6313^{*}$ (0.2560)
Manuf scstot_c	$+0.1869$ (0.2048)	$+0.1474$ (0.1688)	$+0.6334^{*}$ (0.3669)
Manuf $\text{scstot}_c \times$ ln gdppc	$+0.0624$ (0.4385)	-0.1624 (0.3757)	-0.3094 (0.4324)
Serv scstot_c	$+0.0145$ (0.2070)	-0.0190 (0.2033)	$+0.3835$ (0.3284)
Serv $\text{scstot}_c \times$ ln gdppc	-0.0239 (0.3819)	-0.0899 (0.2488)	$+0.0538$ (0.3470)

Panel A: coefficient on aggregate conditionality $\text{scstot}_{it} = \sum_k c_{it}^k$. Panel B: coefficient on aggregate conditionality (mean-centered, scstot_c) and on its interaction with mean-centered log GDP per capita; the log-GDP-per-capita main effect is included in each regression but is absorbed within the panel transformation in the OLS and heteroskedasticity-based estimators. Standard errors in parentheses. Significance: *: 10%, **: 5%, ***: 1%. "—" indicates a cell missing from the OLS output; the corresponding estimate is available in the heteroskedasticity-based and copula specifications. Full specifications include all controls listed in Appendix A and country fixed effects

H.3 Leading-Condition Placebo

A leading-condition placebo asks whether future conditions predict current outcomes, as they would if the contemporaneous estimates reflected anticipation or reverse causation rather than a causal effect. We re-estimate the four main specifications by OLS (the heteroskedasticity-based and copula corrections address contemporaneous regressor-error dependence and do not extend naturally to leads), replacing each condition with its one- and two-period leads. The condition series are mildly autocorrelated (first-order autocorrelations of 0.17-0.44 for the four conditions of interest), so a lead coefficient need not be exactly zero under the null: a k-period lead mechanically carries about ρ^k of the contemporaneous effect, making the two-period lead the cleaner test. Table 22 reports all three specifications.

At the two-period lead all four central findings are insignificant and smaller in magnitude than the contemporaneous estimates, consistent with a causal reading. Among the four, the only significant lead is the one-period PRI→Manufacturing coefficient (-12.47^{***}), which exceeds the autocorrelation benchmark ($\rho\times$ contemporaneous ≈ -2.2). This is consistent with a short anticipation window: privatisation is announced and negotiated well ahead of implementation, and manufacturing activity may contract as firms restructure in advance, and it disappears by the two-period lead, so the contemporaneous estimate reflects a within-year effect rather than spurious anticipation.

The remaining associations behave the same way: the agricultural findings (FP, FIN) and the other manufacturing and services coefficients are smaller and insignifi-

Table 22 Leading-condition placebo

	TFP			Agriculture			Manufacturing			Services		
	t	$t+1$	$t+2$	t	$t+1$	$t+2$	t	$t+1$	$t+2$	t	$t+1$	$t+2$
DEB	+0.395	−0.081	−0.399	−2.119	−4.453	+3.422	−0.805	+1.669	−4.672	**−13.51*****	−2.576	+5.259
ENV	−0.332	+0.161	+0.530	+2.935	−0.278	+8.423	+13.05**	−2.279	+9.234	−4.195	−1.567	−16.59**
EXT	+0.068	+0.023	−0.065	−1.329	−0.547	+1.111	−0.068	+3.624*	−6.770***	+0.139	−0.234	+1.045
FIN	+0.072	+0.110*	+0.060	+0.822**	+0.622	+0.321	−0.475	−0.952	+1.597**	+0.319	+0.556	−0.665
FP	−0.242*	−0.605***	−0.393**	−3.461***	−0.509	+0.895	+2.009*	−2.879	+1.086	+0.941	+0.079	+0.375
INS	+0.070	+0.208	−0.210	−0.098	−0.323	−2.727	+2.319	+4.044*	+0.399	+1.217	−4.258*	+0.600
LAB	+0.181	+0.416	+0.338	+2.804	+1.401	−1.046	+0.632	+6.421*	+3.707	+8.290**	−3.510	−6.663
RES	+0.263	+1.576**	+0.550	+4.050	+3.298	−4.797	+4.109	+7.737	+10.01	+8.677**	+1.035	−4.768
POV	+0.781**	+0.069	−0.193	−0.935	+0.683	+3.813	−9.627**	−3.921	−2.436	+6.342	+4.594	+0.571
PRI	+0.219	+0.339	+0.549*	−2.753	+0.830	+4.288	**−7.662****	**−12.47*****	−5.239	+0.212	+1.018	+9.546**
RTP	−0.149	−0.189*	−0.070	−0.428	−0.313	−0.062	−1.925	+0.010	−1.150	**−3.684*****	+0.116	−0.088
SOE	−0.246	−0.129	+0.157	+0.539	−0.669	−1.096	**+4.450****	+3.152	+2.914	+1.212	+1.648	+0.982
SP	−0.468	+0.096	−0.626	+2.044	−4.297	−10.78**	+12.54***	+6.481	+8.447	+7.566***	−0.152	−5.326

OLS coefficients on the thirteen condition variables across the four main specifications. For each outcome, columns t, $t+1$, and $t+2$ report the contemporaneous coefficient ($c_{i,t}$, reproduced from the OLS columns of Tables 5, 6, 7 and 8), the one-period-lead coefficient ($c_{i,t+1}$), and the two-period-lead coefficient ($c_{i,t+2}$), respectively. The four findings classified as strong evidence in Sect. "Robustness to Multiple-Testing Adjustment are in bold. Significance: *: 10%, **: 5%, ***: 1%. Standard errors are omitted for compactness. All specifications include the full set of controls listed in Appendix A and country fixed effects. Sample sizes are 600 (contemporaneous), 585 (one-period lead), and 570 (two-period lead)

cant at both leads, and the only significant contemporaneous TFP coefficient (POV) is near zero at both.

Author Contributions Single author paper.

Funding Open access funding provided by Corvinus University of Budapest. None of the authors has received funding.

Data Availability Data is freely available and can be shared.

Declarations

Conflict of interest The authors declare no competing interests.

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