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ORIGINAL ARTICLE

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Innovation context and technology traits explain heterogeneity across studies of agricultural technology adoption: A meta-analysis

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Abstract

Despite a wealth of case-specific insights from agricultural adoption studies, we lack systematic evidence on which technology characteristics matter for adoption across different innovation contexts. We synthesise the results of 304 quantitative farm-level adoption studies for a wide range of agricultural innovations across more than 60 countries using multi-level meta regression. Our results show that land, capital and knowhow are generally more important when an innovation uses the respective factor intensively, but this effect is reduced in contexts where the factor is abundant. Our findings have implications for the design of rural development and agricultural extension programmes. Both should consider the interplay of geographic context and innovation characteristics to develop more effective sustainable intensification strategies.

KEYWORDS

cross-country analysis, induced innovation, meta-analysis, technology adoption determinants

JEL CLASSIFICATION

C21, D22, O13, O33, Q12, Q16

1 | INTRODUCTION

Innovations in agricultural production are essential to achieve global food security, affordable and healthy diets, and more sustainable use of natural resources (Herrero et al., 2020; Rockström et al., 2017; von Braun et al., 2021). We conceptualise innovations as technologies and practices that change production factor composition or increase factor productivity. In developing countries, agricultural innovation has often resulted in positive impacts on productivity and food security (Gollin et al., 2018; Ogundari & Bolarinwa, 2018; Stewart et al., 2015), although heterogeneous social and ecological impacts have been reported (Pingali, 2012). Along with other innovations under the umbrella of digitalisation and smart farming, remote sensors and robots performing autonomous operations in crop and livestock production are expected to power the next agricultural revolution (Barrett & Rose, 2020; Lowenberg-DeBoer, 2015; Torero, 2021). Therefore, a better understanding of the underlying diffusion patterns is needed to inform future rural development policy and agricultural extension strategies. Designing such strategies for adoption is challenging, especially in agriculture, because adoption depends on a wide range of interacting factors, such as biophysical context, farm structure, decision-maker characteristics, technology attributes and institutions. The literature on the determinants of agricultural innovation is correspondingly rich in both theoretical and empirical studies from all over the world (Feder et al., 1985; Foster & Rosenzweig, 2010; Knowler & Bradshaw, 2007; Mwangi & Kariuki, 2015; Prokopy et al., 2019). Review studies have so far struggled to produce consistent evidence on the direction and magnitude of adoption determinants (Knowler & Bradshaw, 2007), or have done so with limited generalisability in terms of geography and types of innovation (Baumgart-Getz et al., 2012; Prokopy et al., 2019; Ruzzante et al., 2021; Shang et al., 2021).

We propose a theoretical framing that explicitly considers interactions between innovation traits and geographic contexts. This enables us to derive somewhat more generalisable insights than prior studies based on a meta-regression approach. Using a new comprehensive global data set of adoption studies and correlated hierarchical effects meta-regression analyses, we exploit variation across space and time to investigate how production contexts influence innovation adoption drivers depending on innovation traits.

We contribute to policy design and technology development. First, we estimate the magnitude of various farm-level innovation adoption determinants over a global range of contexts. Second, we quantify how innovation traits and key geographic context factors affect the relative importance of adoption determinants. This knowledge can inform R&D initiatives and policy-makers in the design of locally adapted technologies and corresponding dissemination strategies that account for heterogeneous innovation contexts.

Section 2 motivates our theoretical framing. Section 3 describes the identification and information extraction from primary studies, and documents our empirical framework and secondary data. Section 4 presents our meta-regression results. In Section 5, we discuss policy implications and limitations of our study before we conclude with avenues for future research.

2 | CONCEPTUAL FRAMEWORK

Rather than looking at groups of similar innovations separately as other studies have done, we use some of the inherent economic innovation traits across innovation groups to derive more general, theory-informed insights into patterns of adoption. This is justified by prior reviews suggesting that innovations can be categorised meaningfully to relate their adoption determinants to specific traits (Arslan et al., 2022; Blair et al., 2021; Fliegel & Kivlin, 1966; Rubas, 2004). We expand global coverage by including OECD countries and a wider range of innovation traits, thereby adding to the meta-analysis by Ruzzante et al. (2021) who adopted a similar approach.

The induced innovation hypothesis (IIH) suggests that innovation is driven by the quest to use relatively more expensive production factors more efficiently (Hicks, 1932). The set of potential factor-augmenting technologies has been conceptualised as the innovation possibility frontier by early microeconomic theorists (Ahmad, 1966; Binswanger, 1974a; Funk, 2002). The first prominent empirical application by Hayami and Ruttan (1971) and related empirical work in agriculture found partial support for the hypothesis (Binswanger, 1974b; Cowan et al., 2015; Goldman, 1993). Based on improved methods and national datasets in the 1990s, several studies cast doubt on the general validity of the IIH (Liu & Shumway, 2009; Olmstead & Rhode, 1993). Clearly, a comprehensive understanding of innovation processes requires a broader theoretical approach linking micro-level, including behavioural, perspectives with system theories (Edler & Fagerberg, 2017). Still, the induced innovation rationale remains popular as a conceptual framework to motivate thinking about innovation processes in bio-based sectors (Asche & Smith, 2018; Stark et al., 2022).

As with most microeconomic optimisation problems, the IIH can be formulated either in terms of highest gain (profit maximisation) or in terms of least resistance (cost minimisation). As such, technological change is usually factor-augmenting. A rational decision maker facing the choice between innovations augmenting different factors along the innovation possibility frontier (IPF) will choose the innovation that augments the most expensive factor, as it maximises output (Funk, 2002). Similarly, one could argue that a rational decision maker would choose the technology along the IPF that minimises use of the more expensive factors.

In addition to the production factors (land, labour and capital) that are commonly used in the literature (Blair et al., 2021; Pardey et al., 2010) we also consider knowhow as a proxy of management skills (Dawson & Lingard, 1982; Huffman, 2020). We make four related propositions:

Proposition 1 *The extent to which the farm size determines the adoption of land-intensive innovations is moderated by relative land-abundancy.*

Proposition 2 *The extent to which labour availability determines the adoption of labour-intensive innovations is moderated by relative labour-abundancy.*

Proposition 3 *The extent to which capital availability determines the adoption of capital-intensive innovations is moderated by relative capital-abundancy.*

Proposition 4 *The extent to which knowhow determines the adoption of knowhow-intensive innovations is moderated by relative knowhow-abundancy.*

Framing the IIH in terms of factor intensities rather than relative factor prices allows us to use globally available data and a theoretically motivated classification of innovation options in our empirical approach below. Accordingly, we do not claim to test IIH directly—rather we seek to provide complementary economic evidence to explain adoption patterns of agricultural technologies at a global scale.

3 | MATERIALS AND METHODS

3.1 | Primary data collection

We closely followed the guidelines for meta-analyses in economics by Havránek et al. (2020). A database containing agricultural innovation adoption determinants from prior studies was created in five steps (see Text S1 and S2). First, we gathered and assessed the eligibility of 1423 adoption studies from the reference lists of prior reviews (Table S1). Second, we followed Grames et al. (2019) and used text mining on the eligible studies to derive a data-driven systematic search string before we retrieved a total of 27,043 peer-reviewed articles from three literature databases, namely Web of Knowledge, EBSCOhost and AgEcon. Third, with the support of automation tools to prioritise relevant abstracts and titles, we screened all unique records according to the eligibility criteria presented in Table S2. Fourth, we extracted and coded the results of 534 randomly selected[1] primary studies along with meta data into a detailed spread-

[1]We expect that not all relevant studies were identified by our approach, but do not expect that non-identified studies differ systematically from the identified ones.

sheet, following Stanley and Doucouliagos (2012) and Floress et al. (2019). We thus base our analysis on a convenience sample of the innovation adoption literature similar to prior reviews (Oca Munguia & Llewellyn, 2020; Ruzzante et al., 2021). Apart from the estimated adoption coefficients and their precision estimates, we collected sample characteristics such as sample size, mean and standard deviation of independent variables, distribution of adopters/non-adopters, information about empirical specifications (e.g., logit, probit), and dependent variable characteristics (e.g., scale and innovation description). Fifth, we categorised all innovations and adoption determinants and expanded the approach of Floress et al. (2019) by including detailed information on measurement units, for example whether farm size was defined as total farm size or area cultivated, measured in hectares, acres or a (non-) linear transformation of the same. An extended PRISMA diagram (Page et al., 2020) with the number of studies that were excluded at each stage of the screening process along with the filtering process of comparable effect sizes is shown in Figure 1.

3.2 | Effect sizes

The primary data for this study are estimated log odds ratios of adoption determinants, which can be used in meta-analysis without further standardisation (Cooper et al., 2009; Stanley & Doucouliagos, 2012). As a measure of precision, this study used the variance of the log odds ratio, calculated from the standard errors, t-statistics, p-values or p-significance thresholds (typically coded as stars) depending on availability. Although we recognise that the majority of our observed effects is neither causal nor unbiased, we assume these estimates to be unbiased on average based on the central limit theorem applying to large samples (see Text S3 for further discussion).

Meta-regression relies on the condition that observations (effects) are measured in a homogeneous manner. We thus carefully ensure the comparability of adoption determinants by using a fine-grained categorisation procedure and rigorous filtering. A total of 32,079 beta coefficients of agricultural innovation adoption determinants were extracted from 524 unique studies (see SI, Full list of included studies, online). Out of these, 22,137 were eligible based on the reported

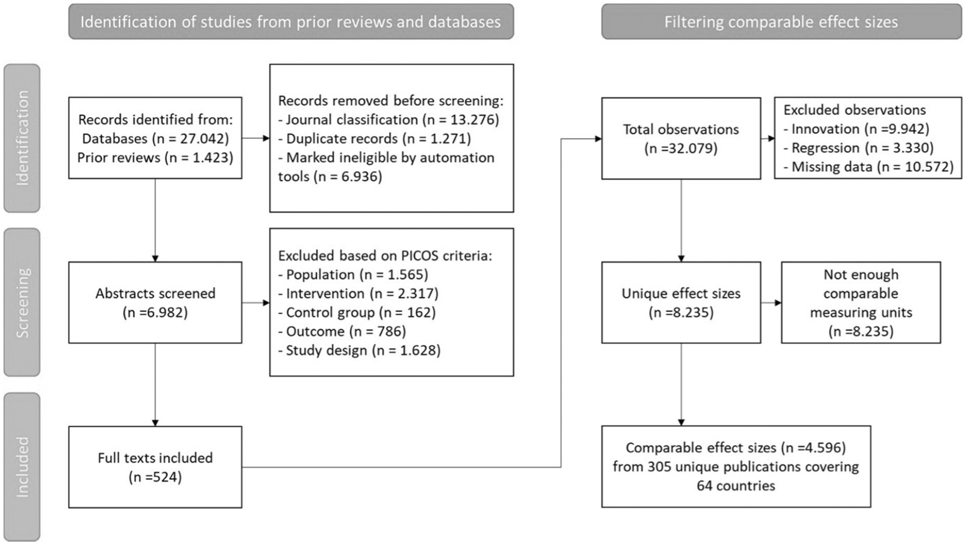

FIGURE 1 Extended PRISMA diagram of included studies and effect sizes

outcome (Text S6; Table S3) and categorised into 42 categories of adoption determinants (Text S5; Table S4). For comparability, only studies using logit or probit estimation methods are included in this analysis, further reducing the number of observations to 18,807. These restrictions could introduce a bias against lumpy innovations or those that can be partially or dynamically adopted, because their adoption is typically not studied as a binary outcome (Doss, 2006; Pannell & Claassen, 2020). We found no drastic differences between the frequency of innovations before and after filtering (Figure S4), but recognise that this check fails to account for studies that did not meet our PICOS criteria. Since some primary studies did not report test statistics or only effect estimates that could not be converted to log odds ratios, comparable effect size estimates and their variance could be calculated for 8235 observations. To ensure comparability, effect sizes within each category of adoption determinants were grouped into the respective measurement units whenever we could obtain sufficiently detailed information. For this analysis, we only used measures of adoption determinants that were used by at least five different studies.

3.3 | Empirical framework

3.3.1 | Aggregation of dependent effect sizes

Meta-analysis without moderators is used to estimate a weighted mean for each adoption determinant, where the weights are inversely related to the variance. We used the estimated log odds ratios as the outcome measure and employed multilevel random effects models with robust variance estimation (RVE). Doing so requires us to deal with non-independent effects and correlated sampling errors. The correlated and hierarchical effects (CHE) model described by Pustejovsky and Tipton (2021) addresses these types of dependencies and can be written as follows for the average effect:

$$y_{ij} = \beta_0 + u_j + v_{ij} + e_{ij} \tag{1}$$

where y_{ij} is the i^{th} effect size (innovation) in study j ($i = 1 \ldots m$, $j = 1 \ldots k$), β_0 is the average population effect, u_j are study-level random effect with variance σ_1^2 (between study variance), v_{ij} are observation-level random effects with variation σ_2^2, and e_{ij} are the known sampling variances of the respective effect sizes with variance s_j^2 and Cov(eij, eij) = ρs_j^2 where we assume a constant correlation[2] among estimates from the same study of $\rho = 0.5$. The unknown variance components σ_1^2 and σ_2^2 are estimated using the Restricted Maximum Likelihood estimator (Viechtbauer, 2005).

3.3.2 | Induced innovation: Meta-regression framework

We use interaction terms between the country- and time-specific factor endowments and innovation-specific factor intensities to test the propositions outlined in Section 2. Table 1 provides an overview of the dependent variables (adoption determinants), the factor intensities assigned as binary variables to each innovation and the proxies for factor abundance used in the analysis. For the binary trait-indicators, we developed a coding scheme with predefined criteria to assign factor intensities. Four trained analysts independently assigned all innovation traits to all innovations based on the coding scheme, reaching a final inter-coder agreement of 96% (see SI, Text S6, for further details). The selection of context indicators was informed by pragmatic criteria of comparability and availability across countries; we discuss the implications below. We

[2]The simplifying assumption of having the same constant correlation of outcomes within studies was taken because with the available data we were not able to model heteroscedastic variances.

TABLE 1 Definition of dependent and independent variables

Dependent variables		Independent variables	
Adoption determinant	**Scale and measuring units**	**Innovation factor intensity (traits)**	**Geographic factor abundance (context)**
Land	Continuous variables: hectares or acres of total farm size or area under cultivation	T1: Land intensity (i.e., 1 for contour farming, buffer strips, agroforestry, conservation practices, organic farming, 0 for all other)	C1: Land-abundance: (1) Log of hectares of cropland equivalent per worker (Fuglie, 2012)
Labour	Continuous variables: number of women, men, adults or household members	T2: Labour intensity (i.e., 1 for permanent cover, contour farming, buffer strips, agroforestry, conservation practices, fertiliser, non-chemical pest control, nutrient intensity optimisation, organic farming, soil analysis, 0 for all other)	C2: Labour-abundance: (1) Share of workforce employed in agriculture (ILO, 2021)
Capital	Binary variables: access or use of formal credit	T3: Capital intensity (i.e., 1 for buffer strips, agroforestry, fertiliser, non-chemical pest control, chemical pest control, soil analysis, mechanisation, precision farming analysis support, precision farming interventions, improved seeds, GMOs, crop insurance, 0 for all other)	C3: Capital-abundance: (1) Log of agricultural machinery stock per hectare of cropland equivalents (Fuglie, 2012)
Knowhow	Binary variables: access and use of traditional extension services	T4: Knowhow intensity (i.e., 1 for permanent cover, agroforestry, reduced tillage, conservation practices, non-chemical pest control, nutrient intensity optimisation, chemical pest control, organic farming, soil analysis, analysis support for precision farming, contract farming, crop insurance, 0 for all other)	C4: Knowhow-abundance: (1) Education index (Smits & Permanyer, 2019)

Source: Dependent variables (adoption determinants) and their measuring units were extracted from primary studies. Innovation traits were assigned to each innovation category based on predefined criteria. Country-level indicators of factor abundance were obtained for the year of data collection of the primary study. The indicator listed under (1) are our primary set of context variables.

consider quantity ratios adequate because they provide an intuitive proxy of relative scarcity, reflecting the material conditions of production, while being less sensitive to agricultural policies than price ratios, in the short term.

Empirically, interaction terms along with a set of control variables were added to the CHE model so that the extended model can be written as

$$y_{ij} = \beta_0 + \beta_1 T_{ij} + \beta_2 C_{ij} + \beta_3 C_{ij} \times T_{ij} + \beta_4 M_{ij} + u_j + v_{ij} + e_{ij} \tag{2}$$

where β_0 is an intercept, β_1 the estimated coefficient for the factor intensity dummy T, β_2 the coefficients for country-year specific factor abundancies C, β_3 the coefficient of the interaction between factor abundance and factor intensity, β_4 the coefficients for additional control moderators M; $u_j\, v_{ij}$ and e_{ij} are defined as in Equation (1). The aggregated effect was considered economically meaningful when its estimated 95% confidence interval did not include zero. To better interpret the magnitude, the aggregated log odds ratios were transformed to odds ratios. Results from estimating Equation 2 must be interpreted with care given that context factors (C) may be endogenous to technology adoption. For example, if mechanisation reduces labour requirements, fewer workers per hectare of cropland are needed. We address this issue by adopting measures of context factors that were taken before levels of adoption were measured in the studies that enter our meta-analyses. Moreover, these studies largely focus on innovations at subnational scale in relatively early stages of dissemination, which are unlikely to affect context factors measured at national scales. That said, we do not claim to have found a strategy that rigorously identifies the causal effect of context factors on adoption factors, but expect to find plausible correlations. We show correlations and geographic distribution of each context factor in the Figures S5 and S6, respectively. Summary statistics of all independent variables and adoption determinants are reported in Tables S5 and S6, respectively.

3.3.3 | Robustness checks and publication bias assessment

We checked the robustness of our estimates by consecutively adding sets of control variables These were: (1) other innovation traits; (2) context indicators; (3) study- and regression characteristics; and (4) dummies indicating whether the primary study controlled for other selected adoption drivers such as assets, education or income. Our main results are based on the full set of control variables. In the random effects model, true population effects may differ even in the absence of sampling error. We therefore tested within each outcome, whether the effect sizes belong to different populations by testing the significance of the Q statistic using a χ^2 distribution (Hedges & Olkin, 1985). We tested for the presence of publication bias using Egger's regression test with a significance threshold of $p = 0.10$ (Egger et al., 1997; Sterne & Egger, 2005). Results of the main regressions with moderators after excluding influential observations are reported as robustness checks. Potentially influential observations were identified using Cook's distances larger than four standard deviations. As a constant correlation between estimates from the same study, we assumed a value of 0.5 and conducted a sensitivity analysis by varying this value between 0 and 1. We also tested whether results could be driven by studies that provide multiple estimates of the adoption of the same innovation (i.e., different model specifications) by considering only one average estimate per study (Gleser & Olkin, 1994). Finally, we tested whether results were sensitive to the choice of context-indicator by using an alternative set of context variables given in the SI.

The analysis was conducted using the metafor package (Viechtbauer, 2010) and clubSandwich package (Pustejovsky, 2020) for R (R Core Team, 2020). Further information including summary statistics, variable descriptions, robustness checks, publication bias assessment, and a full list of included studies are provided in the Supplementary Material.

4 | RESULTS

This study synthesises a total of 4596 estimated beta coefficients of innovation adoption determinants. They originate from 305 unique publications, of which 257 report results for a specific region, 46 at the country level, and 2 across countries. Figure 2 illustrates the geographic distribution of studies and innovations. Most studies focus on the United States, suggesting a potential language selection bias because we incorporated part of the data from Floress et al. (2019). In terms of number of observations, sub-Saharan countries, predominantly Ethiopia, were strongly represented in the adoption literature, whereas Latin America, Europe, and Oceania are under-represented in our dataset (see Table S6).

4.1 | Effect size aggregation

Figures 3 and 4 show the average odds ratios for comparable categories of binary and continuous adoption determinants respectively along with their robust 95% confidence intervals for all measuring units with at least 20 observations. The columns on the right indicate the number of effect

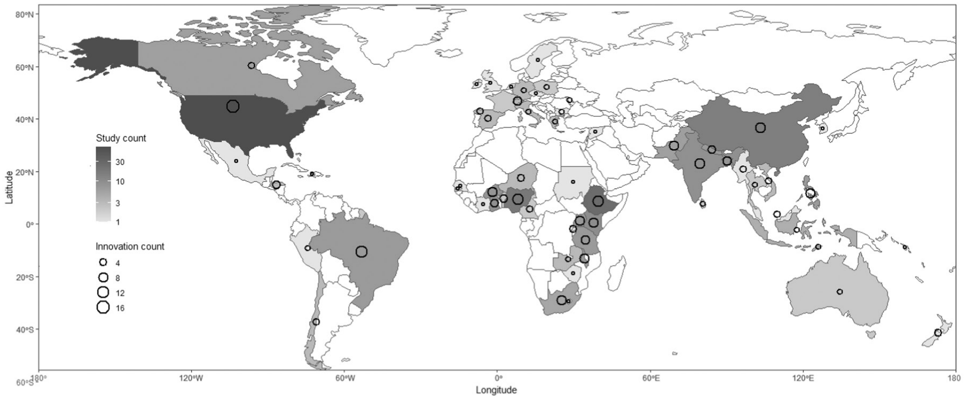

FIGURE 2 Geographic distribution of comparable studies and innovation. The colour of the country indicates the number of adoption studies in the respective country, while the size of the circles indicates for how many different innovations in a given country adoption determinants were estimated. In the USA, for example, we found 68 studies reporting adoption determinants for 16 different innovations.

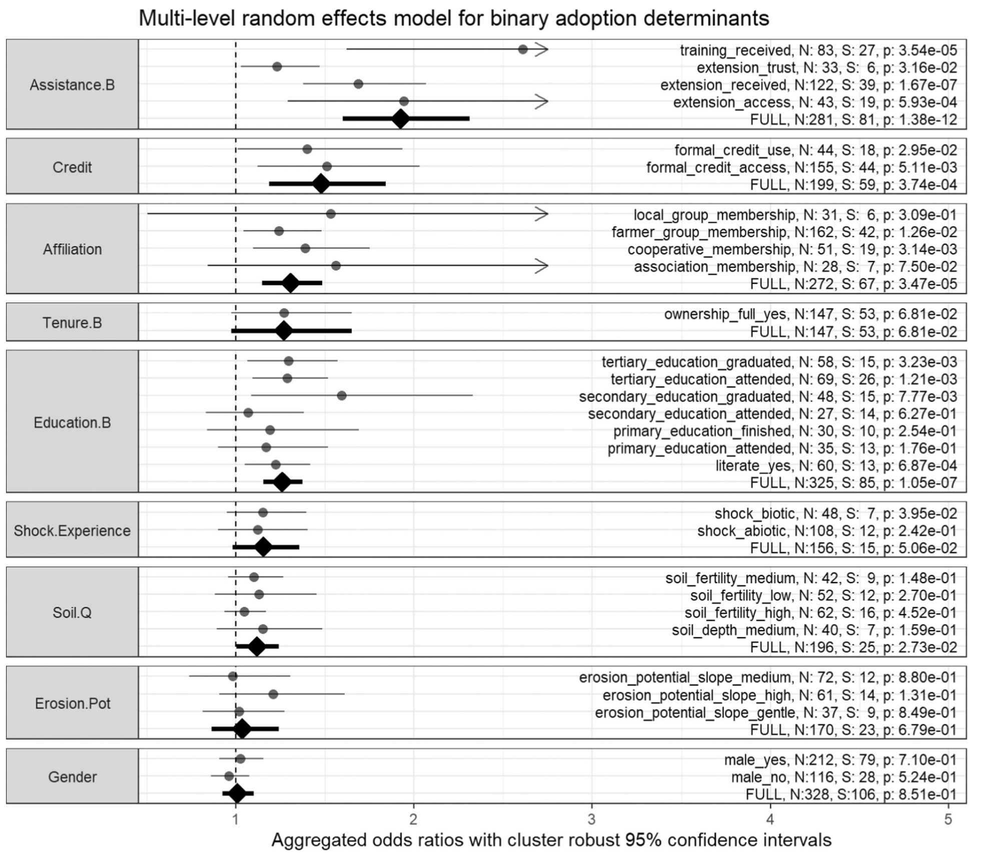

FIGURE 3 Weighted mean odds ratios (OR) for binary adoption determinants grouped by measuring unit. Within each category (grey boxes on left side) we report separate regressions for each measuring unit (written on right side). The diamond-shaped estimates (FULL) combine all measuring units with the respective category. N, number of observations, S, number of studies, p, Satterthwaite *p*-value of OR being equal to one. Dotted line indicates zero effect, namely odds of adoption equal odds of non-adoption. For exact numerical representation of estimates and additional model statistics see Table S7

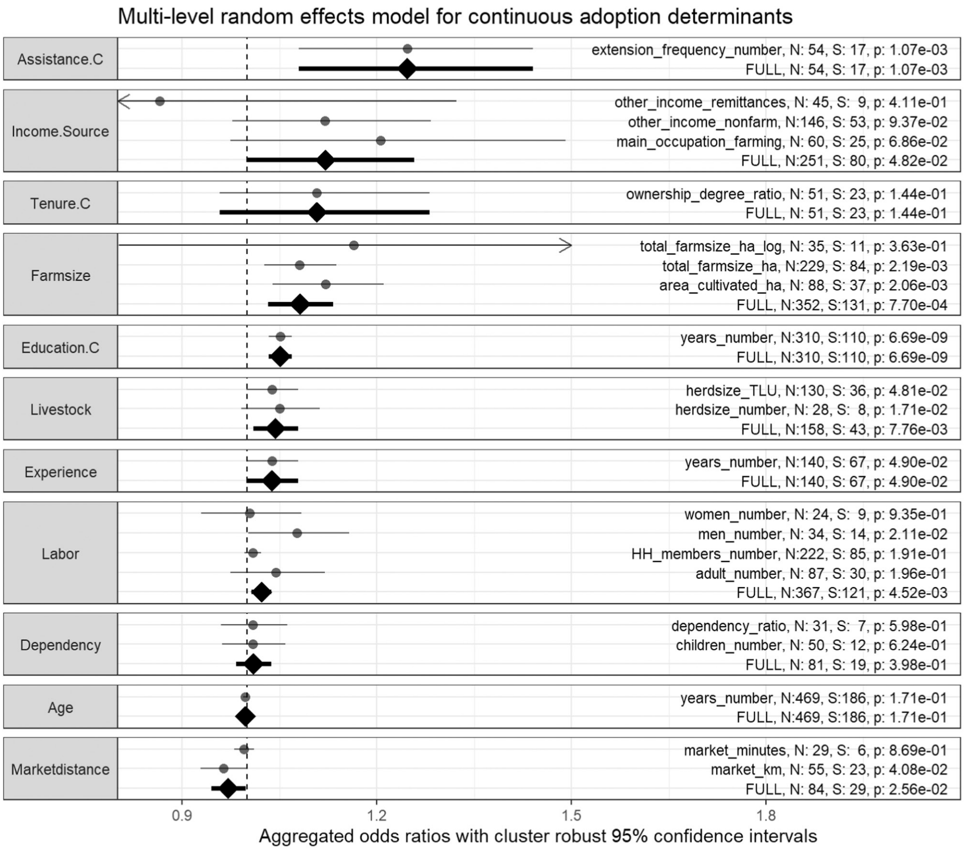

FIGURE 4 Weighted mean odds ratios for continuous adoption determinants grouped by measuring unit. Within each category (grey boxes on left side) we report separate regressions for each measuring unit (written on right side). The diamond-shaped estimates (FULL) combine all measuring units with the respective category. N, number of observations, S, number of studies, p, Satterthwaite *p*-value of OR being equal to one. Dotted line indicates zero effect, namely odds of adoption equal odds of non-adoption. For exact numerical representation of estimates and additional model statistics see Table S7

sizes used for the estimate (N), the number of studies from which these effects were extracted (S) and the *p*-value indicating whether the estimated intercept significantly differed from zero. Odds ratios can be interpreted as changes in the odds of adopting the innovation against the reference of one, all else being equal. For example, binary variables indicating that extension services were received have an average odds ratio of 1.69, which translates to an increase of 69% (95% CI: 37.6–106.7%) in the odds of adoption. Similarly, binary variables indicating access to and use of formal credit were grouped together in the FULL model specification, resulting in an average increase in the odds of adoption by 48% (95% CI: 18.7–84.1%).

The only adoption determinants that are consistently (i.e., for all measuring units) and significantly (i.e., $p < 0.1$) different from zero were Assistance (binary and cont.), Credit (binary), Tenure (binary), Education (cont.), Experience, Livestock, while farm size—although generally positive and significant—exhibited substantial variation when measured in log hectare unit. These findings are more conclusive than those of previous vote-count analyses (Knowler & Bradshaw, 2007; Shang et al., 2021) Other commonly used determinants such as gender and age were not found to significantly differ from zero on average; the latter is contrary to findings by Baumgart-Getz et al. (2012), who focused on North America. Market distance was the only measure negatively associated with adoption. In general, our findings are in line with those by Ruzzante et al. (2021), as expected. Binary measures tended to have larger magnitudes than related continuous measures. For example, having graduated from a university increases the odds of adoption by 29% (95% CI: 6.6%–57.2%), while one additional year of education has an effect of 5% (95% CI: 3.3%–6.9%). At the same time, variables measured on a continuous scale tended to have a lower variance. Binary measures must be interpreted with caution avoiding conclusions about relative magnitudes, because we could not control for the reference categories since this would drastically reduce the sample size. If tertiary school attainment is compared to secondary school attainment, one can expect a lower magnitude than when it is compared to another baseline category, for example, having received no primary education, which may be the case in developing countries. Hence, our estimates of categorical adoption determinants should be interpreted as upper-bound estimates. Notably, even within the relatively fine-grained outcome measures, all estimates still have significant residual heterogeneity ($p<0.01$) (reported in Table S7). We thus proceed to meta-regression analysis and assess whether moderators can explain this heterogeneity.

4.2 | Revisiting the induced innovation hypothesis

The meta-regression results presented in Table 2 show the interaction effects between innovation specific factor intensity and country specific factor abundance for the four adoption categories land, labour, capital and knowhow. The innovation traits T1–T4 are assigned the value of one if the innovations use the respective factor intensively, and zero otherwise, while the context factors C1–C4 are continuous measures (see Table 1 for details).

We found significant ($p<0.1$) negative interaction terms for the proxies of land, capital and knowhow, that is, farm size, credit access/use and extension access. A negative interaction term indicates that a higher value of the context indicator is associated with a lower odds ratio of adopting innovations that use the interacted factor intensively. We did not find a significant interaction effect on labour, but note that the estimated between-study heterogeneity in true effects (σ_1^2) was close to zero, so that potential moderators that could explain this variation would have an economically rather insignificant magnitude. The omnibus moderator tests were marginally significant for farm size (p = 0.08) and extension (p = 0.08).

4.3 | Robustness

Regarding the aggregated effects (Figures 3 and 4), Egger's regression test indicated no evidence for publication bias. However, the Q-statistic indicated significant heterogeneity in the true effects for all estimated average effects, which we attribute to differences in innovation, sample and study characteristics (Table S7). Hence, even though the average odds ratio is significantly greater than one, the distribution of true effects estimated by the random effects model may include effects smaller than one.

The QE-test for residual heterogeneity in the regression reported in Table 2 remained highly significant after the inclusion of all moderators. The moderators included in this analysis thus only explain a part of the variation in true effects. The interaction effects for farm size and credit shown in Table 2 remain stable after the exclusion of potentially influential studies identified via Cook's distance (Table S10). As shown in Table S8 and Table S9 both estimates and their p-values are sensitive to the assumed within-study correlation of estimates; at an extreme hypothetical intra-study correlation of 1 the effects are only marginally significant. Yet, the sensitivity analysis supports our belief that the within-study correlation of effects plays an important role and should be modelled accordingly. Our interaction effect estimates are robust to a variety of model specifications and when considering only one estimate per innovation per study (Tables S12–S16). An alternative set of context variables produced similar magnitudes and directions of

TABLE 2 Interaction effects of factor intensity and factor abundance for land, labour, capital and knowhow

	Farmsize	Labour	Credit	Assistance.B
Innovation traits				
T1: land-intensive	0.24***	0.00	0.00	0.21
	(0.07)	(0.02)	(0.12)	(0.17)
T2: labour-intensive	−0.10**	0.03	0.06	−0.12
	(0.07)	(0.06)	(0.12)	(0.11)
T3: capital-intensive	0.10*	−0.02	−0.48**	0.87***
	(0.06)	(0.03)	(0.25)	(0.34)
T4: knowhow-intensive	−0.03	0.02	−0.15	1.47***
	(0.04)	(0.02)	(0.11)	(0.54)
Context indicators				
C1: land-abundance	0.03	−0.06**	−0.25	−0.01
	(0.04)	(0.04)	(0.35)	(0.15)
C2: labour-abundance	0.00	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.02)	(0.01)
C3: capital-abundance	0.00	−0.00	0.08	0.08
	(0.02)	(0.01)	(0.09)	(0.09)
C4: knowhow-abundance	−0.05	−0.14	1.18	0.24
	(0.25)	(0.11)	(2.36)	(1.16)
Interaction terms				
T1 × C1	−0.07***			
	(0.03)			
T2 × C2		−0.00		
		(0.00)		
T3 × C3			−0.12***	
			(0.05)	
T4 × C4				−1.79*
				(1.06)
Constant	0.11	0.26**	0.99	0.35
	(0.24)	(0.12)	(2.13)	(1.36)
Regression type	Yes	Yes	Yes	Yes
Measurement units	Yes	Yes	Yes	Yes
Model specification	Yes	Yes	Yes	Yes
sigma2 1	0.04	0.00	1.06	0.18
sigma2 2	0.06	0.01	0.14	0.28
cochran.qe	5470.97	4085.72	1660.44	1154.95
p value cochran.qe	0	0	0.00	0.00
cochran.qm	41.89	25.75	21.03	31.78
p value cochran.qm	0.01	0.26	0.40	0.08
df.residual	343	338	172	165

(Continues)

TABLE 2 (Continued)

	Farmsize	Labour	Credit	Assistance.B
logLik	−187.32	96.70	−190.79	−191.84
deviance	374.63	−193.41	381.59	383.68
AIC	428.63	−143.41	427.59	433.68
BIC	532.25	−47.83	499.98	511.33
AICc	433.43	−139.24	435.04	443.04
Observations	368	361	193	188

Note: Column labels indicate dependent variable, that is, adoption determinant as specified in Table 1. Innovation traits (T1–T4) refer to binary variables of factor intensity, while context indicators (C1–C4) indicate factor abundance for land, labour, capital and knowhow, respectively (see Table 1 for details). A set of control dummies accounts for model specifications in primary studies: regression type (logit, probit), scale of dependent variable (binary and multivariate), observation level (plot or farm), spatial level (regional or national); model specification dummies for whether the original model controlled for farm size, labour, credit, assets, income and education or not, and in case of farm size whether a squared term was included in the primary regression. Distribution of measurement units for each regression is shown in Table S5. Brackets contain robust standard errors clustered at the study-level. The sigmas refer to estimated variation components between studies (σ_1^2) and within study (σ_2^2). Cochrane test for residual heterogeneity (QE) and its significance (QEp) as well as omnibus moderator test statistic (QM) and its significance (QMp) are reported.

*** $p<0.01$; ** $p<0.05$; * $p<0.1$.

estimated interaction effects (Table S11), but the coefficients were no longer significant. The original set of context indicators was chosen to minimise the number of missing observations; for the alternative context variables much fewer observations are available, which explains the reduced statistical power.

5 | DISCUSSION

We found large and significant positive average effects for binary adoption determinants related to assistance, credit access and group affiliation. We also found statistically significant positive average effects, albeit an order of magnitude smaller, for continuously measured determinants related to years of formal education, experience and livestock herd size. These findings are broadly in line with prior meta-analyses. Ruzzante et al. (2021), for example, reported similar effect directions, with only minor differences in absolute magnitudes. They used a different effect size measure (the partial correlation coefficient) and included both binary and continuous adoption measures, which may partially explain the difference.

We further found no evidence for a uniform effect of age or gender on adoption. We found mostly positive associations for labour endowment, farm size, risk preferences, and tenure status as adoption determinants, although some measuring units did not indicate a significant average impact. Importantly, we show that some of these factors matter more or less under a selected set of contextual conditions that reflect factor abundance and corresponding technology traits. Since direction and magnitudes of adoption determinants have been extensively discussed in earlier reviews (see Section 1 and Table S1), we focus here on the results of our moderation analysis. Regarding the interaction effect of innovation factor intensity and context factor abundance, we found that our propositions (Section 2), based on induced innovation, can explain some of the variation in true effects across countries and innovations.

5.1 | Land

Consistent with proposition P1, our results suggest that the extent to which land availability at farm level determines the adoption of land-intensive innovations decreases with increasing land abundance. Our interpretation of land (farm size) as an adoption determinant deviates from Ruzzante et al. (2021) in that we do not interpret the positive sign as sufficient evidence for increasing returns to scale of the innovation. Prior studies have emphasised the role of fixed transaction costs involved in changing the production system. Often a critical scale of operation is needed to overcome an innovation threshold (Foster & Rosenzweig, 2017). Relying on fixed transaction costs as an impact channel for farm size is therefore consistent with the idea that the positive effect of farm size on adoption reflects economies of scale. Differences in the farm size estimate would then be attributable to different marginal cost of implementing the innovation. However, the fact that larger farms are more likely to adopt innovations may also relate to alternative mechanisms such as affluence, risk-affinity, management style or bargaining power—which are likely endogenously linked to farm size.

Ruzzante et al. conjecture based on their findings that NRM technologies may be implemented in a capital-intensive fashion in the USA, whereas labour-intensive implementation would dominate in developing countries. Indeed, within-innovation differences in factor intensities could explain heterogeneity in adoption determinants at various scales. Since our data allows us to empirically test for these relationships, we focus on the between-innovation variation in factor intensities by assuming each innovation to be homogeneous with respect to factor intensities. Our theoretical framework based on induced innovation (IIH) provides a mechanistic interpretation of the relationship between factor abundance and factor intensity and for the macro-scale interpretation, it does not matter whether the variation occurs within or between innovations.

5.2 | Labour

We did not find a statistically significant interaction effect for the factor labour. This may be due to a lack of statistical power to explain very small variations (see Figure 4). Furthermore, household size related variables are typically included in adoption studies as a proxy for farm labour usage in the presence of imperfect labour markets. Under functioning labour markets, the size of the household is not expected to have any influence on the farm labour usage, since labour supply and labour demand of the farm household are separable (Benjamin, 1992). Instead of low wages as a reason for farm labour being less of a driver for innovation, labour supply may actually be low due to imperfect markets, even though the country is labour abundant. Thus, the findings may point towards a discrepancy between labour abundance and actual labour supply. However, our data did not allow for tests with other farm labour indicators or an assessment of the role of seasonal fluctuations in labour availability.

5.3 | Capital

Consistent with proposition P3, we find that a change in access to formal credit has a relatively smaller effect on the adoption decision in capital abundant contexts. This must be interpreted with caution because our observations are limited to non-OECD countries, which is not surprising given that capital markets work comparatively well in OECD countries and credit access is thus virtually never considered. This is in line with the sizeable impact of access to capital on US agricultural productivity during the first half of the twentieth century, when rural capital markets were less developed (Hutchins, 2022). Data limitations did not allow us to test the effect on adoption determinants such as debt-asset ratio, which is more commonly measured in capital-abundant OECD countries. To corroborate our findings with respect to capital, we report additional moderation analyses for tenure status, livestock and gender in Table 3. Following Arslan et al. (2020), we also used livestock (herd size measured in tropical livestock units or total

TABLE 3 Additional regression results for adoption determinants related to capital

	Tenure.B	Tenure.B	Livestock	Livestock	Gender	Gender
Innovation traits						
T1: land-intensive	0.08	0.08	−0.00	−0.01	−0.07	−0.06
	(0.12)	(0.14)	(0.04)	(0.03)	(0.18)	(0.18)
T2: labour-intensive	0.10	0.10	0.04	0.04*	−0.24*	−0.25**
	(0.15)	(0.16)	(0.03)	(0.03)	(0.15)	(0.15)
T3: capital-intensive	−0.09	−0.10	0.04	0.18***	−0.28*	−0.39*
	(0.23)	(0.31)	(0.03)	(0.05)	(0.12)	(0.17)
T4: knowhow-intensive	0.21	0.21	0.00	0.01	0.05	0.05
	(0.19)	(0.22)	(0.03)	(0.03)	(0.14)	(0.14)
Context indicators						
C1: land-abundance	0.01	0.01	0.04	0.03	0.01	0.00
	(0.31)	(0.31)	(0.04)	(0.04)	(0.09)	(0.09)
C2: labour-abundance	−0.02	−0.02	−0.00	−0.00	0.00	0.01
	(0.02)	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)
C3: capital-abundance	0.17	0.17	0.05***	0.04**	−0.04	−0.02
	(0.12)	(0.13)	(0.02)	(0.02)	(0.05)	(0.06)
C4: knowhow-abundance	−4.01*	−4.00*	−0.38	−0.36	0.53	0.49
	(1.55)	(1.55)	(0.21)	(0.24)	(0.95)	(0.94)
Interaction terms						
T3 × C3		−0.00		0.04***		−0.04
		(0.11)		(0.01)		(0.05)
Constant	4.94***	4.94***	0.22	0.23	0.27	0.35
	(2.72)	(2.71)	(0.17)	(0.18)	(0.99)	(0.98)
Regression type	Yes	Yes	Yes	Yes	Yes	Yes
Measurement units	Yes	Yes	Yes	Yes	Yes	Yes
Model specification	Yes	Yes	Yes	Yes	Yes	Yes
sigma2.1	0.89	0.89	0.00	0.00	0.00	0.00
sigma2.2	0.15	0.15	0.01	0.01	0.48	0.48
cochran.qe	1415.33	1411.90	1674.14	1637.12	4274.09	4268.68
p value cochran.qe	0.00	0.00	0.00	0.00	0	0
cochran.qm	32.06	31.89	32.98	43.73	30.39	30.92
p value cochran.qm	0.04	0.06	0.03	0.00	0.08	0.10
df.residual	126	125	139	138	299	298
logLik	−135.55	−135.11	102.01	106.56	−373.77	−372.77
deviance	271.11	270.23	−204.02	−213.12	747.54	745.55
AIC	317.11	318.23	−158.02	−165.12	795.54	795.55
BIC	382.34	386.11	−90.53	−94.86	884.35	887.97

(Continues)

TABLE 3 (Continued)

	Tenure.B	Tenure.B	Livestock	Livestock	Gender	Gender
AICc	327.93	330.23	−148.42	−154.50	799.92	800.33
Observations	147	147	160	160	321	321

Note: Innovation traits (T1–T4) refer to binary variables indicating land intensive, labour intensive, capital intensive, and knowhow intensive, respectively (see Table 1 for details). A set of control dummies accounts for model specifications in primary studies: regression type (logit, probit), scale of dependent variable (binary and multivariate), whether the original model controlled for other independent variable categories or not, observation level (plot or farm), spatial level (regional or national). Brackets contain cluster robust standard errors. The sigmas refer to estimated variation components between studies (σ_1^2) and within study (σ_2^2). Cochrane test for residual heterogeneity (QE) and its significance (QEp) as well as omnibus moderator test statistic (QM) and its significance (QMp) are reported. ***$p<0.01$; **$p<0.05$; *$p<0.1$.

heads) as a proxy of capital because of its function as collateral.[3] We tested whether the effect of this capital proxy is moderated by capital-intensity and abundance (Column 4 in Table 3) and found a positive interaction effect indicating that livestock is relatively more important for adopting capital-intensive innovations in capital-abundant settings. This result may seem at odds with the IIH. But, livestock often serves as a collateral and its effect is then indirect and mediated mainly by the availability of (in-)formal capital markets, which tend to be more developed in capital-abundant settings. In addition, we report results for tenure status (1 = being full owner, 0 otherwise) and gender (1 = male, 0 = female). Similar to livestock, the effect of tenure status is positively influenced by capital abundance in the study context independent of capital intensity of the innovation. That is, being a full owner and having more livestock is more important where capital is available (and land can be employed as collateral on capital markets).

Interestingly, none of the context or trait-related variables were significantly associated with the estimated coefficients for gender as an adoption determinant. Although insignificant, we cautiously interpret the negative point estimate in the gender regression as a sign that identifying as male may be more important for the adoption of innovations in capital scarce contexts. This is intuitive considering the gender-based differences in access to capital and underlines the continued need for inclusive (especially gender-sensitive) financial institutions.

5.4 | Knowhow

The large magnitude of the assistance effect (Figure 3) highlights that agricultural extension plays an important role in the innovation adoption process, even though a lack of accountability, performance gaps and distributional shortcomings were highlighted in the literature on agricultural knowledge systems (Anderson et al., 2006; Norton & Alwang, 2020). Although we cannot rule out that some primary studies labelled 'extension' as relating to any type of professional in-person knowledge transfer, we assume that extension predominantly relates to the more traditional 'Train & Visit' approaches. Such approaches were subject to criticism in favour of more bottom-up approaches (Chambers, 1998; Scoones et al., 2009; Scoones & Thompson, 1994), but our estimates show that they effectively enhance the adoption of agricultural innovations. Clearly, bottom-up approaches could have been even more effective in doing so and may have beneficial effects beyond promoting technology adoption.

We interpret the results in Table 2 (column 4) in favour of our proposition P4, namely that extension as a proxy of knowhow positively influenced the adoption of knowhow-intensive innovations especially in knowhow scarce contexts. Our finding contradicts Ruzzante et al. (2021), who found that the education level in the same context was negatively associated with the effect

[3]For formal credit, livestock has been argued to be a poor collateral for being prone to theft and disease (Binswanger & McIntire, 1987), but some microcredit institutions do accept it nowadays (Chapoto & Aboagye, 2017).

extension has on the adoption of improved seeds (not knowhow-intensive), but positively associated with the effect it has on the adoption of natural resource management (knowhow-intensive). This is a surprise because we based our calculation on the same education index. In addition, we found that extension was more effective for the adoption of capital- and knowhow-intensive innovations opposed to land and labour-intensive ones. We speculate that our finding can be explained by factor mobility: capital and knowhow tend to move more freely than land and family labour (Binswanger & McIntire, 1987), making it easier for extension agents to successfully advocate capital and knowhow-intensive innovations. The finding may also indicate effectiveness of the facilitating role knowledge networks such as extension agents or associations play in promoting access to rural credit (Balana & Oyeyemi, 2022; Carrer et al., 2020; Linh et al., 2019).

Two factors limit our confidence in the coefficient of the corresponding interaction term. First, we did not find consistent results using alternative proxies of knowhow, namely experience and formal education (Table S17). The estimates of the interaction terms are also inconsistent across model specifications, outcomes and particularly sensitive to the choice of alternative context indicators. As with labour, this may have to do with the difficulty of decomposing very limited heterogeneity in true effects. Second, the relative focus on non-OECD countries in our sample, once again, limit generalisability to the global level. In OECD countries, assistance is often measured by the presence of (self-paid) advisors rather than publicly financed extension agents. Including observations with this assistance proxy in the regressions led to inconsistent results. We excluded these observations due to the difference in their total heterogeneity (Figure 3: rows 1, 3 and 4); the corresponding dummy would have been correlated with the error term and thus biased our estimates.

5.5 | Limitations

Our meta-analysis was constrained by the diversity of empirical strategies, (partially) unreported results, and notably by a lack of consistency in the measurement of commonly used adoption determinants. Overcoming comparability related issues by rigorously filtering out non-comparable observations and controlling for the exact measurement units increased the geographical imbalance in our final dataset. Our categorisation of innovations and consecutive assignment of factor intensities did not account for potential heterogeneity of factor intensities, especially when endogenously influenced by the geographic context (see also Section 3.3.2). In addition, there may be a pre-existing geographic bias in terms of the innovations (and thus factor intensities) under study and thus covered in the literature. The same holds true for measurement scales and units. For example, capital was commonly measured as access to credit in developing countries and as debt-asset ratio in industrial countries.

Finally, the available context indicators may not be optimal for testing theory-based propositions. In addition, our indicators did not capture distributional asymmetries of context factors within countries, although we included the Gini-coefficient as a control (Tables S13–16, column 7). Other proxies such as factor price ratios could be more intuitive in the context of induced innovation and would facilitate interpretability for policy-makers, but to the best of our knowledge such data do not exist with global coverage.

5.6 | Future directions

Abstracting from specific innovations in terms of innovation traits merits closer attention both in meta-analyses and primary studies, because they may facilitate transferability of research findings. Our results point towards potential transferability of past research findings by abstracting traits and applying known (or assumed) combinations thereof to future innovations. The factor intensities employed in this study represent only a subset of distinct innovation traits and other dimensions should be considered more systematically. For the case of agricultural robots, for example, capital and knowhow intensity may be useful traits, but they should be complemented by inherent impacts (e.g., environmental footprint), attributes relevant for social learning diffusion processes (e.g., observability) as well as differentiation between labour-augmenting and labour-replacing innovation (Marinoudi et al., 2019). For the ex-ante diffusion assessment of new technologies, a trait-based uncertainty reduction of adoption determinants could provide important insights. Interacting innovation characteristics and the affinity of innovators towards these characteristics has been proposed as a mediation mechanism in the ADOPT model by Kuehne et al. (2011, 2017). Our estimated ranges of odds ratios for the most commonly used categories of adoption determinants may serve as credible input ranges in agent-based models for modelling diffusion patterns of digital agricultural information (Shang et al., 2021). Finally, many environmental indicators (e.g., potential productivity or vulnerability to climate change impacts) as well as socioeconomic context indicators (e.g., population density, land prices, access to digital infrastructure) are available on subnational scales. Therefore, a promising avenue for policy-oriented future research would be a more regional analysis of certain subsets in terms of innovation and/or context to better understand the role of within-country production context variation.

Previously identified challenges for generalisation include the various definitions of adoption, measures of adoption determinants, and representations of temporal adoption dynamics (Doss, 2006). Our findings therefore suggest that established minimum standards for agricultural adoption studies are needed to extract further generalised lessons from this important subfield in agricultural economics. Below we provide a non-exhaustive list of practical recommendations towards this goal, complementing previous attempts to create reporting guidelines for adoption studies. At a minimum, authors of adoption studies should:

1. Report all estimated effects in tabular format along with a measure of their sampling error independent of their significance.
2. Indicate model specifications and variables that were used in the regression, but omitted in the results table to save space.
3. Indicate any (non-)linear transformation of variables. Independent variables should be measured in or converted to the International System of Units (i.e., hectares, tons, years), where applicable.
4. Report the number of observations for each regression. Especially for data structures with multiple observations per individual (e.g., panel data, multiple plots), the unit of observation should be clearly indicated.
5. Provide summary statistics in tabular format and include at least the mean and standard deviation (proportion for binary variables) of all (in-)dependent variables for the entire sample as well as for different subgroups (e.g., adopters and non-adopters).
6. Provide a description of (a) the study area(s), (b) the innovation(s) considered, such as claimed advantages, historical adoption levels, and (c) the sample characteristics in terms of market orientation (e.g., subsistence vs. commercial), product specialisation (e.g., rice farmers, mixed livestock farmers etc.).
7. Make preferential use of continuous independent variables as such and not recode them into categorical or ordinal scales.

Of course, following such guidelines is subordinate to a rigorous research design that contributes to better understand behaviour, as well as the role of gender, innovation characteristics and digitalisation in agriculture (Pannell & Claassen, 2020).

6 | POLICY IMPLICATIONS

Innovation in agricultural production remains one of the most important strategic pillars in the transformation towards sustainable food systems, as pointed out during the UN Food Systems

Summit (von Braun et al., 2021). Despite the large number of existing adoption studies worldwide, we still poorly understand why apparently beneficial agricultural technologies suffer from low or stagnating uptake. Here we have systematically taken stock of the existing, mostly context-specific, empirical evidence and found that agricultural knowledge and extension systems as well as alleviation of credit constraints may deserve more attention than they currently receive, especially in the developing world. In particular, our findings warrant more emphasis on the design of policies and interventions that improve technical knowledge, skills and capital access.

For example, agricultural extension programmes could boost the uptake of new technologies by aligning dissemination strategies with regionally heterogeneous target group characteristics and agricultural factor scarcities. As digital technologies become increasingly available to farmers worldwide, the importance of technical knowledge and skills as adoption determinants will grow. Digital literacy thus also has to feature more prominently in the curricula of rural training and education programmes.

Agricultural extension is also often the vehicle for rural credit programmes. Our findings suggest considerable synergies from packaging agricultural extension and rural credit lines, such that they coherently promote technologies with attributes that address region and farm-specific output and input market constraints. Considering spatially heterogeneous endowments and access to production factors across prospective user groups may also allow technology developers to better tailor future innovations to local needs.

Beyond market-related factor scarcities, environmental policies are likely to play an increasingly important role as drivers of innovation in the transformation towards sustainable and climate-resilient agriculture (Ambec et al., 2011). For example, conservation policies that limit access to land in a context of land abundance, were shown to induce agricultural intensification (Koch et al., 2019). Similarly, smart environmental regulation could increase the attractiveness of eco-efficient technologies, such as weeding robots, if contextual variability in the abundance of other production factors were properly taken into account. A successful digital transformation thus implies increased and interdisciplinary collaboration between new and traditional stakeholders of agricultural knowledge systems in order to avoid innovation system failure (Hermans et al., 2015) .

Finally, the temporal dynamics of context factors imply that forward-looking policy design must be informed by a structural understanding of the embeddedness of production systems. This, at the same time, warrants great caution in the transfer of research findings across space and time, because differences in geographic context and accelerating climate change impacts clearly influence sample-specific findings of adoption studies. If we want to leverage innovation to overcome food system challenges, policy must take into account their context-specific (dis-)advantages and recognise macro-structural barriers of innovation diffusion.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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