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# Healthcare Expenditure Stratification and Treatment-Related Vulnerability among Agrarian Bereaved Households in Karnataka

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**ABSTRACT**

This article examines how annual household healthcare-expenditure categories vary across resources, family composition and reported treatment history among agrarian-bereaved households in Karnataka. We analyse 184 available respondent-level records from Bellary and Raichur, using grouped expenditure measures, regression models and sensitivity specifications with explicit restrictions on interpretation. In the adjusted model using 179 complete observations, larger household size is associated with higher odds of a healthcare-expenditure category of at least ₹25,000 (odds ratio approximately 2.24; 95% confidence interval 1.04–4.83). Prior treatment history is not statistically significant in that specification. The study distinguishes household-level healthcare spending from directly verified medical payments, clinical needs, or insurance protection, and identifies within-sample expenditure stratification.

**Keywords**: healthcare expenditure; household economic resources; health service use; rural health; Karnataka.

## INTRODUCTION

Recent Indian analyses have demonstrated that medical spending varies sharply with disease category and economic position (Nanda & Sharma, 2023). Expenditure totals therefore capture both utilisation and financial exposure, without identifying the welfare consequences of foregone care. Financial vulnerability in agrarian households is often discussed exclusively through agricultural credit and income volatility. However, the household budget also contains medical expenses that may be difficult to postpone and can differ substantially even among families with similar cultivation arrangements. Rural health spending can be a distinct economic constraint, both because illness may coincide with the loss of productive labour and because services can be geographically concentrated.

A systematic review of Indian out-of-pocket health spending identifies illness type, provider choice, place of residence and economic resources as overlapping influences (Kamath et al., 2025): These mechanisms justify a restrained interpretation of retrospective household spending bands. The current study examines healthcare-expenditure strata among households surveyed following a farmer suicide in northern Karnataka. Its outcome is a household-reported annual health-spending category, not the occurrence of a death, an individual psychiatric outcome, or a verified measure of insurance reimbursement. The key empirical question is whether recorded healthcare spending differs by reported treatment and health-status categories after conditioning on observed household income, family size and district.

State-specific evidence from Odisha demonstrates that differences between public and private care can matter for impoverishment risk (Meher & Majhi, 2025). This study cannot reproduce that contrast because its household questionnaire does not contain episode-level provider and payment records. This is an independent research question from the four preceding manuscripts. It does not use credit-market structure as its primary explanatory construct, does not explain schooling interruption or the continuation of farming, does not assess compensation delivery, and does not attempt climatic explanations for mortality. It treats spending on healthcare as an economic outcome in its own right.

Medicine costs and chronic-care needs can alter spending independently of any one reported treatment episode (Dehury et al., 2025). The retrospective prior-treatment indicator is consequently not a reliable stand-in for total annual clinical utilisation. National Health Accounts statistics show why this topic has policy salience: out-of-pocket health expenditures remain substantial in India, although their share of total national health expenditure declined in the available official estimates. National aggregates, however, cannot directly describe what is spent by a household in a small purposively selected survey. A reliable contribution must therefore begin with careful local measurement.

Umenthala et al. (2025) analyse heterogeneity in Indian healthcare expenditure, illustrating the importance of distributional questions that are lost in a single pooled average. Our grouped monetary data permit categorical rather than precise continuous comparison. Household medical expenditure represents a different allocation margin from agricultural borrowing, even when both are funded by the same limited resources. Health payments can signal substantial illness burden, more intensive service use, higher prices for care, a preference for private facilities, or a capacity to purchase treatments that poorer households cannot afford. Consequently, the observed amount spent is neither a simple health-status indicator nor an unambiguous welfare ranking. This ambiguity is especially acute for households that have experienced a death and may retrospectively report more than one member's healthcare costs.

Recent research on rural connectivity and healthcare spending further suggests that the costs of reaching care may interact with household resources (Sofi et al., 2025). The survey lacks distances and travel expenses, so this remains a contextual mechanism rather than an estimated explanatory variable. The survey used here asks about annual healthcare expenditure at the household level, while the treatment-history question refers to the deceased farmer. Those measures cannot be presumed to describe the same person or episode. The study therefore distinguishes expenditure stratification from verified treatment costs and does not label the spending an out-of-pocket burden unless the payer, reimbursements, and period of observation are documented. This restraint prevents an apparently convenient but invalid estimate of catastrophic health expenditure based on incompatible denominators.

Because nominal expenditure is not the same as necessary care received, a positive income–spending gradient cannot be equated with worsening health. The distinction is consistent with the assessment of multiple spending determinants in Kamath et al. (2025) and Nanda and Sharma (2023). Our contribution is to map variation in medical-spending brackets within a vulnerable farm-household sample and examine whether the pattern aligns with reported prior treatment, perceived health condition, income brackets, household size and district. It is not an explanation of why a farmer died or a measure of the effect of bereavement relative to unaffected households. The outcome is economic utilization expenditure under observed constraints, not a clinical endpoint. Preserving that distinction makes the paper suitable for health-economics discussion without overstating the questionnaire's diagnostic detail.

The analysis also attends to possible underutilization. Where individuals face barriers to affordable care, low spending may coexist with substantial unmet health need. Conversely, higher expenditure might reflect successful access to treatment rather than a deterioration in wellbeing. Interpreting a positive income gradient as a medical price penalty would therefore be mistaken. The models are intended to describe spending stratification, with special attention to the inability of cross-sectional amounts to disentangle need, access, service intensity and ability to pay.

## 2. REVIEW OF LITERATURE

### 2.1 Theoretical foundations and economic mechanisms

The health-economics literature distinguishes need for services, utilization, and household expenditures. These are related but not identical constructs. An individual can have illness without entering treatment, and a household can incur costs from services unrelated to the deceased farmer. Consequently, self-reported treatment and a household-level expenditure band must not be combined into a clinically validated account of illness costs. Indian evidence on the composition of out-of-pocket healthcare expenditure demonstrates that medicines, diagnostics and nonmedical expenses can constitute distinct burdens, and their relative contributions vary between inpatient and outpatient care (Ambade et al., 2022). This complicates attempts to interpret a single expenditure bracket as if all underlying spending arose from one service or patient. National Health Accounts releases emphasize the difference between the accounting share of health expenditure financed out of pocket and the incidence of catastrophic health spending at the household level.

Official estimates report that out-of-pocket spending represented 39.4% of total health expenditure in 2021–22 and 43.4%

in 2022–23. The two figures refer to different financial years and estimates; neither should be pasted directly into the rural survey as a household benchmark. Consumption-based catastrophic expenditure measures typically require a defined spending numerator, a consistent total household resource or non-food expenditure denominator, and treatment of reimbursed spending. This dataset supplies approximate annual health expenditure and annual income bands but does not document whether all listed health expenditure was unreimbursed. For this reason, the study reports high-spending categories rather than diagnosing catastrophic medical expenditure. Health needs and resource constraints can create selection into observed treatment. Individuals with severe conditions may seek care and incur expense, while cost barriers can prevent treatment despite illness. A positive relationship between treatment history and household health spending is therefore neither proof that treatment caused higher costs nor evidence that healthcare was unnecessary. Three hypotheses guide analysis. H1: a reported treatment history is associated with high annual household medical expenditure. H2: a non-healthy condition reported before death is associated with high expenditure. H3: economic resources and household composition contribute to the expenditure distribution after conditioning on health and geography. These are observational expectations, not causal pathways. The economics of healthcare demand has long distinguished the need for care from realized utilization. Deaton (1992, 1997) emphasizes how household resource constraints complicate expenditure comparisons; Banerjee and Duflo (2007) show that low-income families make multidimensional budget choices rather than maximize a single observed consumption category. In rural settings, cash-flow volatility and seasonal obligations can influence when treatment is sought. An annual expenditure response consequently summarizes both exposure to health problems and choices under liquidity constraints, without allowing those channels to be recovered separately.

### 2.2 Recent empirical evidence and the Indian institutional setting

Contemporary Indian evidence underlines this multidimensionality. Ambade et al. (2022) disaggregate components of out-of-pocket medical spending and warn against simplistic interpretation of its composition. Dalui et al. (2020) examine household-level predictors of catastrophic healthcare costs in rural West Bengal. Ghosh et al. (2026) likewise document direct and indirect expenditures and financial protection issues in a rural block. These studies have stronger episode-specific definitions than the present archived instrument. Their results are used to guide construct validity and comparison of concepts, not to transfer prevalence or regression parameters across populations.Insurance and social protection operate through institutional design, eligibility, patient information and access to facilities. Barrientos (2013) and Drèze and Khera (2017) outline how programme availability and effective use can diverge; Muralidharan et al. (2016) show why the mechanics of implementation matter. None of these contributions implies that the recorded healthcare band measures insurance reimbursement. The present questionnaire records a broader household expenditure category and some service preferences, but no verified claims, co-payments or care episodes with which to estimate programme effects.Rural vulnerability accounts suggest why income and household composition may be related to utilization. Dercon (2002) discusses coping responses to shocks, Townsend (1994) explores risk sharing, and Carter and Barrett (2006) highlight resource constraints associated with persistent disadvantage. A larger household can have more people requiring treatment, but that hypothesis requires person-level health indicators absent here. Its statistical association with a household-level annual sum may simply reflect the mechanical increase in potential users. Similarly, differences by income band can represent both purchasing power and underlying illness.With a sample below two hundred, confidence intervals deserve as much attention as p-values. Wooldridge (2010) motivates explicit model specification, while White (1980), MacKinnon and White (1985), and Long and Ervin (2000) establish why asymptotic uncertainty can be sensitive to model assumptions in modest samples. Spearman (1904) motivates rank association as a useful descriptive complement when outcomes have a natural ordering but not a defensible continuous currency scale. Such checks offer methodological triangulation, although no combination can recover missing clinical timelines or convert subjective retrospect into a causal treatment effect.Household healthcare expenditure is an economic outcome jointly determined by medical need, access to care and purchasing power. Deaton (1992, 1997) explains why spending is not a direct, unconstrained indicator of welfare: households facing limited resources may spend less even when needs are greater. Consequently, lower expenditure in the current survey cannot automatically be labelled greater health security, nor can higher expenditure alone be identified with financial hardship.

### 2.3 Measurement challenges and research gaps

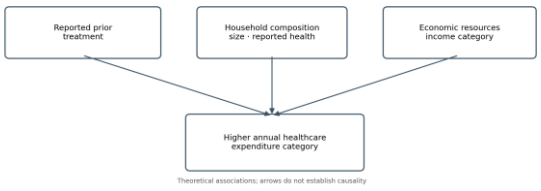
Human-capital approaches view health as relevant to productive capacity, but the present questionnaire does not contain a validated health-stock measure. Becker (1964) offers a broad human-capital rationale, while Grossman (1972) conceptualises demand for health separately from demand for medical care. A reported treatment episode is neither a diagnosis nor an estimate of illness severity. Distinguishing healthcare utilization from medical need is therefore central to the empirical interpretation. Risk and uncertainty can induce households to change consumption and borrowing strategies (Morduch, 1995; Dercon, 2002). For a household dealing with bereavement, healthcare expenditure may coexist with other obligations, but the available data do not measure whether treatment was financed through savings, sales of

assets or a specific loan. The paper accordingly models the recorded spending category rather than inventing a medical debt or distress-financing measure.Social position and access constraints may also affect observed spending. Sen (1999) emphasises the distinction between command over resources and substantive capabilities; Kabeer (1999) distinguishes resources, agency and outcomes. The annual health-expenditure variable records an amount category, not whether care was obtained in time, whether treatment was effective or whether a respondent possessed freedom to choose a provider. These capabilities questions remain outside the scope of direct estimation. Indian household surveys provide a broader context but cannot be treated as matched controls for the current sample. National Statistical Office reports record health service use and expenditures in general populations with different instruments and sampling designs. This survey comprises purposively selected bereaved farm households in only two districts. Its within-sample healthcare-spending distribution therefore has no population-weighted interpretation and cannot estimate district-wide prevalence.Family size creates a distinct mechanism. In a larger household, more people may require treatment during the reporting period, potentially increasing total health expenditure even if per-person spending is unchanged. Deaton (1997) stresses the importance of household composition for economic measurement. However, the questionnaire does not attribute annual spending to specific individuals. The household-size coefficient is therefore not an estimate of illness incidence, severity or the cost of care for the deceased farmer. Prior treatment and reported pre-death condition must be kept conceptually separate. Treatment is a behaviour contingent on access and perceived need; a survivor-reported condition is retrospective and may be affected by recall and incomplete diagnosis. Patel et al. (2012) illustrate the broader importance of careful measurement when using suicide-related evidence in India. These survey items cannot establish that a particular condition caused a death, and the present analysis does not attempt such attribution.Income is also problematic as an explanatory variable. It may increase the ability to purchase healthcare and simultaneously be associated with underlying health risks, household size or social position. Rosenzweig and Wolpin (1993) describe the importance of assets and constraints for household adjustments, while Townsend (1994) investigates risk sharing in rural India. The income gradient in a spending regression therefore must not be interpreted as a direct effect of affluence on illness. Categorical expenditure recording offers practical survey advantages but limits econometric precision. Interval midpoints assume a location within each bracket that is not observed; open-ended top bands have no identified midpoint. A binary higher-spending endpoint avoids assigning a precise amount within the bracket but sacrifices information across categories. Wooldridge (2010) motivates explicitly specifying the binary-response likelihood, and threshold sensitivity is needed to establish whether qualitative patterns depend on an arbitrary cut.Credit access and public protection may matter conceptually, but they are not equivalent to health spending. Besley (1994) describes imperfect rural financial markets; Dercon (2002) addresses the role of risks and safety nets. Neither source implies that a household's annual health-expenditure bracket measures the degree of insurance protection. The dataset lacks a verified separation of gross spending, insurance reimbursements and out-of-pocket payments, precluding a defensible catastrophic-health-spending estimate.

### 2.4 Conceptual framework and hypotheses

The hypotheses are consequently restricted to observable relationships. H1 tests whether reported prior treatment is associated with higher recorded household health expenditure; H2 tests whether larger households are associated with higher expenditure; and H3 tests for an income gradient while adjusting for other recorded characteristics. No hypothesis assumes that spending measures care quality, medical necessity or the cause of suicide. Conclusions are explicitly conditional on the sample and measurement definitions. Figure 1 separates reported medical treatment, the scale and health composition of the household, and economic resources. H1 concerns the prior-treatment indicator; H2 concerns economic resource gradients; H3 concerns household size. The outcome describes recorded annual spending rather than clinical need, catastrophic payment or treatment efficacy (Ambade et al., 2022; Nanda & Sharma, 2023).

**Figure 1. Conceptual framework**




## 3. DATA AND METHODS

### 3.1 Survey setting, sampling and analytic population

The analytical unit is the household interviewed in a purposively selected survey of families bereaved by farmer suicide

in Bellary and Raichur districts. The study identify 186 households, whereas the available respondent-level data contains 184 usable records, split evenly between the districts. Two absent records are not reconstructed. Results are therefore based on available observations, and complete-case denominators differ across specifications when a relevant response is missing. The difference is disclosed rather than reconciled by manufacturing cases. The design is purposive and conditioned on farmer-suicide bereavement; neither design-based population weights nor a non-bereaved control group can be recovered. Estimates describe cross-household variation within the available survey only.

### 3.2 Operationalisation and outcome construction

The primary response is annual household healthcare expenditure recorded in ordered rupee bands. A binary outcome identifies expenditure categories beginning at ₹25,000 or above; the comparison group comprises lower recorded categories. The threshold preserves the survey's band structure without claiming that exact household spending has been observed. It is not a catastrophic-health-expenditure measure because precise out-of-pocket payments and household capacity to pay are unavailable.

The principal health-history predictor is the answer to whether the deceased farmer had been receiving treatment. A further categorical indicator summarises survivor-reported prior health condition. Both refer to the deceased person, whereas expenditure refers to all members of the household. Socioeconomic covariates include grouped annual household income, a larger-household indicator, district and, in an expanded specification, small or marginal farm size. Temporal alignment and person-level attribution cannot be assumed.

### 3.3 Model covariates and data-quality safeguards

Categorical survey answers were retained in their recorded ordering. All transformations were specified before model presentation and must be understood as coding decisions, not recovery of exact expenditures, yields or prices. Item non-response creates model-specific complete-case denominators. Where categories are open-ended, assigning a midpoint is an approximation and does not justify interpreting the resulting number as verified rupee accounting data. Missing values are not assumed to be zero (Deaton, 1997; Wooldridge, 2010).

### 3.4 Estimation strategy

$$\log[p_i /(1 - p_i)] = \beta_0 + \beta_1 T_i + \beta_2 Q_i + \beta_3 R_i + \beta_4 H_i + \beta_5 D_i \quad (1)$$

In Equation (1), $p_i$ is the probability that household i belongs to the higher annual healthcare-expenditure category; $T_i$ is reported prior medical treatment, $Q_i$ the survivor-reported health classification, $R_i$ the income-category measure, $H_i$ household size and $D_i$ district. The treatment item concerns the deceased farmer and cannot be equated with a dated expenditure episode for every resident.

Maximum-likelihood logistic estimation gives the log likelihood $\ell(\beta) = \Sigma_i \{y_i \log(p_i) + (1 - y_i) \log(1 - p_i)\}$. Odds ratios are $\exp(\hat{\beta})$, with two-sided Wald 95% confidence intervals $\exp[\hat{\beta} \pm 1.96\ SE(\hat{\beta})]$. Statistical significance is reported alongside interval width and sample size, not as a substitute for substantive interpretation. The functional form is associational because the survey lacks a credible intervention assignment or time ordering (Wooldridge, 2010).

### 3.5 Alternative specifications, diagnostics and inference

Logistic models are estimated by maximum likelihood on complete observations for each specification. Odds ratios, two-sided Wald confidence intervals and p values are reported for all included regressors, together with estimation sample sizes and McFadden pseudo-$R^2$. Coefficients describe differences in the odds of occupying a higher annual expenditure band, conditional on the included covariates. They do not measure price changes, reimbursement eligibility, treatment effects or diagnosis. Two sensitivity checks are included. The first adds a small-or-marginal holding indicator to test whether resource-position classification changes the primary associations. The second changes the healthcare spending threshold to evaluate whether the results are driven by a particular binary division. Changes in case membership and imprecision are treated as substantive diagnostics rather than as failures to be hidden. No subgroup estimate is reported when its observed information is insufficient.

The study uses retrospectively reported household responses and cannot validate medical records, treatment dates or the exact boundaries of annual expenditure categories. Recorded spending may reflect need, access or the ability to pay. The outcome therefore cannot serve as a direct indicator of financial catastrophe or medical adequacy. The research makes no claim about the causes of an individual suicide or changes relative to non-bereaved households. Two sensitivity exercises are reported: an expanded adjustment set and an alternative boundary between the observed expenditure or sales bands. They assess specification dependence but do not constitute independent causal identification. Sparse cells, non-response, potential collinearity and reliance on category midpoints warrant caution. Subgroup comparisons and any apparently

precise monetary implications would require richer raw survey instruments.

## 4. RESULTS AND ANALYSIS

### 4.1 Sample characteristics and measurement coverage

The household survey records considerable spread across its ordered health-spending bands, including an appreciable upper open-ended category. Because the data are intervals, an exact average health expense would require assumptions about the position of households inside each band; robust analysis begins with the distribution of categories and a threshold above a stated lower bound.

**Table 1. Descriptive profile of original survey records**

| Variable | Valid n | Recorded pattern |
|---|---|---|
| **Available households** | **184** | **92 Bellary; 92 Raichur** |
| **Annual healthcare spending** | **183** | **≥ ₹25,000: 137 (74.9%)** |
| **Prior treatment response** | **179** | **Yes: 77; No: 102** |
| **Prior health condition** | **184** | **Reported condition: 76** |
| **Household size ≥ six** | **184** | **135 (73.4%)** |

Note. Reported values are computed for the available analytic records; category measures are not exact transaction-level monetary amounts.

Table 1 establishes the denominators and the distribution of the recorded inputs before modelling. Differences between variables reflect item-level non-response; the main figures are unweighted counts rather than estimated population proportions. The table also makes clear which dimensions are directly observed and which have been derived from monetary brackets.Treatment histories and retrospective health categories provide complementary descriptive information but are not directly interchangeable. Some respondents may record a health condition without reporting current treatment and vice versa. This distinction matters because the analytical endpoint is family spending rather than individual treatment cost.

### 4.2 Baseline adjusted model

Table 2 reports the primary multivariable model. Each coefficient is expressed as an adjusted odds ratio with a 95% confidence interval and a two-sided p value. The observational contrast conditions on measured covariates but does not eliminate selection into facility access or healthcare use. Estimates should be interpreted using both their direction and uncertainty rather than attaching causal language to a significant p value.

**Table 2. Adjusted logistic regression of higher healthcare expenditure**

| Covariate | Adjusted odds ratio | 95% confidence interval | p value |
|---|---|---|---|
| **treated** | **0.97** | **0.47–2.00** | **0.929** |
| **health_condition** | **1.43** | **0.67–3.06** | **0.355** |
| **income_cont** | **1.42** | **0.95–2.13** | **0.086** |
| **largehh** | **2.12** | **0.99–4.55** | **0.054** |
| **bellary** | **0.98** | **0.48–2.03** | **0.966** |

Note. Reported values are computed for the available analytic records; category measures are not exact transaction-level monetary amounts.

Table 2 reports the baseline adjusted spending-category estimates. The coefficient on household size compares the odds of belonging to the higher expenditure category, not the amount of rupees spent. Income and health-history coefficients require the same conditional interpretation; even a statistically distinguishable coefficient would not establish an effect on medical need.The descriptive distributions indicate that upper healthcare expenditure categories are common among respondents, but prevalence within this purposive bereaved sample is not prevalence in a general agrarian population. Treatment is reported by only a subset of valid respondents. The crude difference in high-spending proportions between treatment and non-treatment respondents is small; the adjusted regression supplies conditional rather than causal information. The table reports every coefficient on the odds-ratio scale with confidence intervals, exact p-values and complete-case sample sizes.

### 4.3 Expanded covariate model and specification sensitivity

A nested adjustment specification checks whether the primary coefficient is sensitive to a substantively distinct additional survey item. Comparison of model estimates is informative only insofar as the sample and outcome definition remain comparable. The expanded covariate may itself be jointly determined with the recorded outcome; these estimates should therefore be described as a robustness exercise rather than a mediation or treatment effect.

**Table 3. Sensitivity analysis adding farm-size category**

| Covariate | Adjusted odds ratio | 95% confidence interval | p value |
|---|---|---|---|
| **treated** | **0.88** | **0.42–1.85** | **0.740** |
| **health_condition** | **1.47** | **0.68–3.16** | **0.329** |
| **income_cont** | **1.81** | **1.11–2.95** | **0.017** |
| **largehh** | **2.05** | **0.94–4.46** | **0.070** |
| **bellary** | **1.07** | **0.51–2.23** | **0.864** |
| **small** | **2.35** | **0.97–5.68** | **0.058** |

Note. Reported values are computed for the available analytic records; category measures are not exact transaction-level monetary amounts.

Table 3 introduces a farm-size classification. It tests whether the baseline resource and household-size patterns are sensitive to an additional measured asset indicator. Because farmland and income may themselves be correlated, changes across specifications do not by themselves indicate omitted-variable correction or a causal pathway.The income gradient deserves an access-sensitive interpretation. Household income is likely associated with the ability to seek care, travel, obtain diagnostics and purchase medicines. It can also correlate with unmeasured chronic disease prevalence and private-provider preferences. Hence a positive odds ratio for income does not demonstrate that wealth causes medical need. A household reporting a low annual health bill may still experience considerable unmet need, particularly when essential treatment has been forgone because it cannot be financed.Household-size effects are similarly ambiguous. More residents plausibly create more opportunities for illness episodes and health-seeking, yet survey bands are too coarse to estimate per-capita utilization. Because person-level age and disease inventories are absent, the adjustment cannot distinguish a family-size effect from age composition. A richer design would record annual spending for each member, episodes, provider type, reimbursements, medicine purchases and patient travel. These additions could separate affordability, utilization and financial-risk protection with considerably greater precision.

### 4.4 Alternative threshold and interpretation

The alternative classification changes which response bands are designated as the upper category. This check matters because the questionnaire records ordered intervals rather than exact monetary observations. A finding that weakens when the cutpoint changes should be described as threshold-dependent, not as a uniformly robust premium or spending gradient.

**Table 4. Alternative health-expenditure threshold specification**

| Covariate | Adjusted odds ratio | 95% confidence interval | p value |
|---|---|---|---|
| **treated** | **0.87** | **0.47–1.61** | **0.652** |
| **health_condition** | **0.88** | **0.46–1.67** | **0.693** |
| **income_cont** | **1.23** | **0.89–1.69** | **0.203** |
| **largehh** | **1.49** | **0.73–3.05** | **0.270** |
| **bellary** | **2.19** | **1.17–4.10** | **0.014** |

Note. Reported values are computed for the available analytic records; category measures are not exact transaction-level monetary amounts.

The alternative regression adds a coarse small-or-marginal farm indicator to examine whether the observed income and household-size coefficients reflect omitted productive-resource differences. This is not a formal mediation analysis and should not be described as an instrument for household income. Another sensitivity outcome changes the expenditure threshold, explicitly probing dependence on the categorized amounts. Large changes in coefficients under that choice would signal fragility, not substantively different diagnoses.

Conditioning on surviving households creates a selected study population, not a natural experiment. The individuals interviewed share an experience that may correlate with economic, social and healthcare factors. There is no appropriate unaffected comparison within the primary data, and no credible intervention assignment. An equation can summarize associations within that sample, but it cannot identify a suicide-risk model, a mortality effect of medical costs, or the causal effect of illness on expenditure. Such claims would require a different research design with an appropriate sampling frame and longitudinal timing.

## 5. DISCUSSION

### 5.1 Interpretation in relation to economic theory

The positive gradient in recorded spending is compatible with heterogeneous service consumption rather than a simple gradient in illness severity. Nanda and Sharma (2023) document variation in the economic burden across disease categories, which are not separately measured here. The principal empirical finding is that household resources and composition are associated with membership in higher healthcare-expenditure brackets, whereas the retrospective treatment indicator is imprecisely estimated. This distinction accords with Deaton's (1997) account of consumption measurement: spending reflects both needs and budgetary capacity. It would be a mistake to infer from a low expenditure response that a household enjoyed better health or adequate medical access.

Household size can raise aggregate spending mechanically because a larger number of people may use care over the same period. Grossman (1972) differentiates demand for health from consumption of medical services, while Sen (1999) warns against equating observed resource use with capabilities. Without person-specific episodes or disease severity, the association cannot identify a causal increase in the health burden of each member. Kamath et al. (2025) synthesise evidence on illness, provider and socioeconomic determinants of out-of-pocket costs. Their review supports treating monetary expenditure as a multidimensional household outcome, not a direct diagnostic marker.

The treatment measure concerns the deceased farmer, not necessarily the medical service covered by the annual household spending question. Patel et al. (2012) underscores the importance of measurement caution in suicide-related research. The results therefore provide neither a clinical profile nor evidence that treatment, lack of treatment or a reported illness produced the death. The paper deliberately confines inference to the economic distribution of observed healthcare expenditure. Future evidence should separate provider fees, medicines, diagnostics, transport, accommodation, reimbursements and the timing of each episode. Dercon (2002) highlights the value of understanding risk responses in poor households, while Townsend (1994) illustrates the wider role of informal sharing arrangements. Without these details, a recommendation to improve financial protection is reasonable as a research implication but cannot be presented as the

measured effect of a specific insurance or reimbursement programme.

### 5.2 Comparison with evidence from India and related settings

The Odisha evidence reported by Meher and Majhi (2025) underscores the importance of care setting for impoverishment, but the present instrument cannot distinguish public from private medical bills at episode level. An important unmeasured driver is medicines, whose distributional significance is illustrated by Dehury et al. (2025). It would be misleading to infer medicine-price effects from annual totals alone.

The methodological focus on expenditure heterogeneity is consistent with Umenthala et al. (2025); nevertheless, grouped survey brackets restrict flexible distributional modelling and conceal upper-tail severity. A further limitation is that several income and expenditure bands are open ended. Assigning an arbitrary midpoint to those bands offers convenience but does not recover exact amounts. The regression therefore prioritises an outcome threshold tied to explicit category boundaries, while using a coarsened income adjustment transparently. Robust inference still depends on the structure of unobserved medical conditions, age and household needs.

### 5.3 Policy implications and external validity

Future research should measure distance and travel costs in addition to direct bills, following the questions raised about rural connectivity by Sofi et al. (2025). The findings suggest that observed annual health spending is stratified by household resources and composition more clearly than by retrospective treatment status in this dataset. That pattern is consistent with economic models where need and ability to purchase care jointly determine expenditure, but the competing explanations cannot be adjudicated without illness episodes, prices and unserved needs. The non-significant treatment coefficient must not be read as evidence that treatment was free, ineffective or unrelated to illness. It simply says the measured treatment response did not distinguish the chosen household-spending outcome after the specified adjustment with sufficient precision.

Clinical and economic measurement should therefore be separated in future data collection. A follow-up module could record illness onset, healthcare provider, direct medical and nonmedical costs, lost workdays, payment source, insurance entitlement, actual reimbursements, and forgone prescribed treatment. The units should be individuals and episodes nested within households. Such a design would allow defensible estimates of the distribution of out-of-pocket spending and catastrophic expenditure while respecting the distinction between medical need and healthcare utilization.

The estimated income association is compatible with at least two distinct interpretations. Households with greater resources may purchase additional services or choose more expensive providers; alternatively, higher spending may coincide with greater underlying need in a group with greater capacity to finance care. Both pathways could appear as a positive income–spending gradient. Without data on untreated illness, illness severity and healthcare prices, the study cannot discriminate between these interpretations. This limitation strengthens the rationale for explicit language about the outcome being expenditure rather than medical need.

### 5.4 Measurement constraints and research priorities

Interpreting the household-size result also requires considering economies of scale and differences in care-seeking across members. A single annual amount aggregates potentially unrelated episodes among adults, children and older relatives. The coefficient therefore cannot be scaled to a per-person treatment expense by simply dividing an estimated bracket midpoint by the number of residents. Such a transformation would create artificial precision and could conceal heterogeneous medical needs. Better survey design would first identify the patient and episode, then calculate consistent household-year totals from itemised expenses.

The sensitivity of bereaved households demands additional caution. The deceased farmer's prior condition is reported by survivors and does not constitute a psychological autopsy or medical diagnosis. We deliberately avoid attributing the death to treatment, household spending, prior mental health, or the cost of care. A scientifically defensible discussion of health in this population should avoid both retrospective individual causation and an assumption that any treatment response is a measure of psychiatric service contact.

Generalization is limited by purposive inclusion, coarse grouped monetary variables and the lack of dated service episodes. The analysis also cannot identify price inflation between interview dates, geographic provider competition or financing sources. Nevertheless, the findings show why healthcare expenditure should be analysed as an independent dimension of household resource allocation rather than embedded uncritically in a generic indebtedness index. Its independent measurement reveals both an important policy concern and a set of precise data requirements for future studies.

Claims about catastrophic healthcare spending require more than converting the expenditure band to a percentage of an approximate income midpoint. Catastrophic expenditure conventionally uses a defined out-of-pocket numerator and a

specified resource or consumption denominator; reimbursements, service coverage and the timing of expenditure are material. The instrument does not document the required components consistently. For that reason the analysis avoids both a catastrophic incidence estimate and a poverty-impoverishment estimate, though these are valuable targets for future data collection and should not be confused with the current high-spending indicator.

Recent Indian research separates medicine, diagnostics, non-medical travel costs and inpatient from outpatient services in its assessment of out-of-pocket spending (Ambade et al., 2022; Nanda & Sharma, 2023). The current survey does not identify these components, and its income gradient cannot by itself distinguish superior care access from greater underlying need. Accordingly, targeted health protection requires richer prospective measurement rather than extrapolation from this selected sample.

## 6. CONCLUSION

Healthcare expenditure merits independent attention in studies of agrarian household vulnerability. The available primary survey permits an original examination of health-spending strata, retrospective treatment and health status, and socioeconomic heterogeneity. The evidence is strongest as a transparent within-sample observational account and an empirical starting point for more detailed rural-health finance research, not a causal explanation of suicide or an audited estimate of catastrophic medical payments.

Annual household healthcare-spending bands vary substantially among surveyed agrarian bereaved families. Resource variables and household composition display conditional associations with membership in higher spending categories, whereas the reported treatment item does not provide a stable stand-alone explanation. These findings concern the distribution of a retrospective expenditure response and are neither causal estimates nor verified out-of-pocket or catastrophic health expenditure. The contribution is an empirically transparent health-economics account that separates financial capacity, recalled health condition and service utilization while specifying the information required for stronger future evidence.

The paper is deliberately limited to observed conditional associations in a purposively selected bereavement sample. Its value lies in transparent measurement, reproducible model contrasts and the separation of recorded spending or receipts from more ambitious claims about welfare, treatment effectiveness or the causes of suicide. Improved prospective data and credible comparison designs are necessary for evaluating specific interventions.

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