

# Minimax Decision-Making for Principal Transformational Leadership Strategies: Optimizing Teachers' Work Engagement through Psychological Capital

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## Abstract

School leaders must choose support strategies while the size and stability of their benefits remain uncertain. This study develops a finite-policy minimax framework for a hypothesized psychological-capital pathway and examines its evidential limits using two public Chinese teacher surveys. The surveys were analysed separately: a leadership and engagement dataset provided 1,033 usable records after one invalid leadership response was excluded, while a two-wave psychological-capital dataset contained 431 teachers. Composite-score regressions, 5,000-resample bootstrap indirect-association intervals, longitudinal change tests, predictive validation, and influence checks provided supporting empirical illustrations. Authentic leadership correlated with engagement ( $r = 0.301$ ), and teacher efficacy correlated with engagement ( $r = 0.645$ ). Three supplied psychological-capital scores declined after multiplicity correction. Neither survey identified the complete transformational-leadership–psychological-capital–engagement pathway. The decision model therefore used disclosed hypothetical gains, costs, and uncertainty bounds rather than estimated intervention effects. Across 48 parameter settings, minimax loss selected the status quo in 38, illustrating the conservatism that follows when zero benefit remains admissible. Minimax regret could select an active policy under the same uncertainty. Elementary optimality conditions explain these differences and specify when additional evidence can change a decision. The framework supports transparent conditional planning; it does not establish that a principal strategy improves engagement through psychological capital.

**Keywords:** minimax optimization; minimax regret; psychological capital; teacher work engagement; school leadership; uncertainty

## 1. Introduction

Teachers' engagement is often determined by the amount of support time and resources a principal chooses to invest before deciding if it will be effective. Average associations give only partial information for this choice. A decision also needs to have a clear intervention, its cost, a range of possible outcomes, and a way to evaluate poor outcomes. If a weak response is taken as the benchmark, a strategy might seem appealing when it has an expected average return, but not when it doesn't. However, a very large uncertainty set can lead to the situation where all the available options are perceived as worse than "no action".

In recent years, psychological capital has been associated with teacher engagement, occupational well-being, and career outcomes in China. Gao et al. (2023) recognize three distinct kinds of psychological-capital profiles of kindergarten teachers. Guo et al. (2022) assessed relationship between engagement and psychological capital and occupational well-being among special education teachers. Ma (2023) investigates the psychological capital in the context of an emotion-regulation pathway. The studies reported above have placed psychological capital in the context of a decision problem for leadership, but they fail to measure the impact of the same group of principal interventions.

A related distinction is raised by evidence of leadership. Zhang et al. (2022) examine preschool principal leadership styles and teacher leadership, with psychological capital as a mediator. The paper by Shao et al. (2025) explores the topics of authentic leadership, school climate, teacher efficacy and engagement. Authentic and transformational leadership are two distinct constructs. Teacher efficacy is a specific professional belief while psychological capital is a broader set of psychological resources. The replacement of the above-mentioned labels would result in the illusion of coverage without measurement.

We are thus faced with the problem of moving from incomplete observational evidence to a transparent conditional decision model in this case. It is not an empirically new pathway of mediation. There are three proposed contributions. First, in a finite-policy loss model, uncertain costs are divided into hypothetical psychological-capital gains, uncertain engagement transmission, implementation attenuation, and costs. Second, simple optimality conditions are provided for the period when minimax loss is conservative and why minimax regret may differ. Third, the information Chinese survey analyses provide and the parameters they do not identify come to light in separate analyses of Chinese surveys.

The title communicates the overall decision problem that the model is intended for. The optimization (in this study) involves the selection of an option in the mathematical model given. Transformational leadership and the psychological-capital pathway appears as a concept application, rather than as observed items on the survey. The empirical part does not provide empirical evidence of any main strategy that can boost engagement by using psychological capital.

## 2. Evidence and Decision Problem

### 2.1. Psychological resources and engagement

Teacher resources evidence is spread over outcomes and populations. Han and Wang (2021) explore the Chinese teachers' self-efficacy, engagement and reflection. Self-efficacy, reflection and resilience are discussed by Heng and Chu (2023). Liu et al. (2023) rank engagement as one of the top three factors after growth mindset, enjoyment, and grit. These take on views of engagement as part of a larger resource system. They are not suggesting that all of the resources can replace the psychological capital in a mediation model.

Career related studies widen the outcome area. Zhao et al. (2022) explore the concepts of calling, burnout, engagement and psychological capital. Hu et al. (2022) consider career commitment, gratitude, and well-being in ethnic areas of China. Zhu et al. (2022) explore psychological capital and its association with commitment and turnover intention for substitute teachers. Their outcomes are related to different choices and outcomes. The complexity of the engagement payoff-function makes it impossible to insert a turnover coefficient without a justified transformation and a corresponding estimate.

Context also requires attention, as organizational context is also significant. In their study, Sarwar et al. (2023) explore the concepts of authentic leadership, support and psychological capital and its relationship with performance. Wang (2024) focuses on the concept of workload, support, engagement, and well-being. Hsieh et al. (2022) separate school-level and teacher-level organizational support in Taiwan and Pöysä et al. (2022) compare leader–follower relationship and engagement profiles in Finland. The latter studies provide information to the literature discussion, while the datasets analyzed are still Chinese. There are limitations in the transfer of numerical results between studies because of the differences in context and level of analysis.

### 2.2. Measurement, identification, and prediction

The more statistical procedures that are added the more of an ill fit the fit will get. Flake and Fried (2020) give a solid account of how choosing measurements without documentation jeopardizes interpretation. The authors of the study (Lundberg et al., 2021) stress the importance of setting a desired amount. In this context the desired intervention amount would be the variation in engagement that a specific principal strategy via psychological capital would create. That number is not listed in either of the surveys that were archived.

Grosz et al. (2020) call for explicit causal assumption. Rohrer et al. (2022) illustrate why the strong identification assumptions held by familiar path models are important. The fitted PLOS products of coefficients are thus considered as "exploratory indirect associations". These are not "as-caused-as" relationships. The sensitivity to specific records is different from omitted-variable bias, which is covered by influence checks (Cinelli & Hazlett, 2020). As accuracy in predicting validation holds, causal identification is not the issue of predictive validation (Bates et al., 2024). The lack of school identifiers further restricts the generalizability to new schools, following Yarkoni (2022)'s discussion of mismatches between verbal claims and statistical targets.

### 2.3. Why minimax and why regret?

Keith and Ahner (2021) differentiate between alternative representations of uncertainty. Lin et al. (2022) survey distributional robust optimization and Duchi and Namkoong (2021) discuss performance protection over distributions. Van Parys et al. (2021) develop a data-to-decision outcome under a well-specified statistical framework. The guarantees don't automatically extend to this application: The surveys don't have observed strategy payoffs or a sample from an intervention-effect distribution.

In the present model, however, a rectangular parameter set with open-ended hypothetical bounds is employed. Its ease of tractability is based on its simple loss structure. The disadvantages of minimax rules and the sensitivity of regret-based recommendations to the choices available are mentioned by Anderson and Zachary (2023). Comparisons between loss and regret, then, are not an extension of the analysis, but it is a part of it. We ask what evidence is needed to make an active policy defensible according to a worst case criterion and how much is that evidence dependent on the criterion itself?

## 3. Materials and Empirical Methods

### 3.1. Public datasets and audit decisions

Table 1 describes the sources and the limits of their coverage. The PLOS survey originates from school teachers in Shandong, China (Shao et al., 2025). Its spreadsheet contains 1,034 records and 50 columns, although the article reports 1,043 participants. The nine-record discrepancy was not resolved. One response, All = 7 at Excel row 1035, falls outside the stated leadership range of 1–5. That cell was treated as missing; requiring all leadership items yielded 1,033 records. The raw file was preserved. One identical complete row was retained because identical responses do not establish a duplicate participant.

**Table 1. Separate public Chinese teacher datasets and empirical coverage.**

| Source | Records used | Measured constructs                                   | Coverage limit                                |
|--------|--------------|-------------------------------------------------------|-----------------------------------------------|
| PLOS   | 1,033        | Authentic leadership; climate; efficacy; engagement   | No transformational leadership or full PsyCap |
| OSF    | 431          | Supplied PC1–PC4; support; career satisfaction, T1/T2 | No leadership or engagement                   |

*Note. PLOS original file  $n=1,034$ ; one invalid leadership response excluded. Samples are not linked.*

The OSF archive associated with Xu et al. (2026) contains 431 records and 79 columns. It includes supplied psychological-capital scores PC1–PC4, organizational support POS1, and career-satisfaction items CS1–CS5 at two waves, together with item-level fields and demographics. The audit found 56 whitespace-only demographic cells and no identical full rows. Numeric scores used in the longitudinal analyses were complete. Full methods and a component map were not available in the supplied archive; PC1–PC4 therefore retain their source labels.

The samples were not pooled, matched, or treated as repeated measurements of a common population. Sample sizes are constrained by the archived sources. No post hoc power calculation is used to claim adequate identification. This distinction follows the broader requirement to justify the role of sample size rather than rely on a conventional threshold (Lakens, 2022).

### 3.2. Scoring and documentation

PLOS composites are the average scores of 14 authentic-leadership items, 10 school-climate items, 12 teacher-efficacy items, and 9 engagement items. The original study reports ranges of 1–5, 1–5, 1–9 and 0–6 respectively. In the engagement columns, the numbers 1–7 are used. The primary analysis leaves the coding of the files unchanged, the difference between the two analyses is a sensitivity analysis and not an assumption that it is a correction. The translation and column order can't be determined from the number of the headers.

Each teacher-efficacy authors' scoring directions includes a method for unweighted means on the standard instrument, and the engagement author's manual includes a method for computing mean total scores. The order of these checks is not to determine the order of items in the archived adaptation. Undocumented sub-scale allocations and reverse scoring were not applied. No scores were re-constructed in the OSF for psychological-capital items. This is to prevent assuming that the instrument used a different grouping than the one that was archived.

The internal consistency (Cronbach alpha) and its bootstrap interval are summarized under the adopted scoring. They lack convergent, discriminant, and longitudinal measurement invariance. For research on climate and efficacy, Zakariya (2020) demonstrates the difference in structural and invariance testing. No validity claim is made here.

### 3.3. Associations, indirect paths, and robustness

Descriptive statistics were computed, and Pearson correlations performed and Fisher-transformation confidence interval computed, along with Spearman correlations. Authentic leadership, school climate, and teacher efficacy were regressed on engagement. Asymptotic normal intervals were given using the heteroskedasticity-consistent (HC3) standard errors. A demographic sensitivity model, gender (source coded), level and type of education, profession and continuous age were employed. All the demography category names were not guessed from the numeric codes.

There are three equations in the exploratory serial model: climate on leadership, efficacy on leadership and climate, and engagement on leadership, climate and efficacy. It has three indirect relationships: products of the relationship between leadership, climate and engagement; products of the relationship between leadership, efficacy and engagement; and products of the relationship between leadership, climate and efficacy and engagement. The 5,000 draws and seed 20261013 were used for Teacher-level Bootstrap resampling. Percentile intervals were given. These equations are not causal, that is, they do not assign an order of decomposition, they only assign an order.

Other checks included 1000-resample alpha intervals, alpha after item removal, variance inflation factors, and residuals, leverage and Cook's distances. Constant responses in all 4 item blocks and Cook's distance  $> 4/n$  were constant responses and not considered to be invalid records. A primary analysis was performed with flagged records. A training-mean baseline was used with five sets of ten-fold validation, performed for predictions based on leadership-only and joint-composite scores. All fittings have been made in training folds. No repeated-fold dispersion was considered to be an independent-sample confidence interval.

The robustness analyses were organized around distinct sources of uncertainty rather than a preferred model. Coding sensitivity assessed dependence on the archived response scale, influence diagnostics examined dependence on individual records, and predictive validation assessed performance for respondents excluded from model fitting. Agreement across these checks would strengthen confidence in the descriptive pattern, while disagreement would identify assumptions requiring closer examination. None of these procedures resolves unmeasured confounding or verifies the conceptual ordering of the proposed mediators. Accordingly, interpretation should distinguish stability of an estimated association from evidence that changing a leadership practice would produce a corresponding change in engagement.

### 3.4. Longitudinal supporting analyses

Change was defined as T2–T1 for the 10 nodes that were supplied. Paired t-tests, within-person standardized change, and 5,000-resample intervals described change. ten parametric comparisons were controlled by Holm adjustment. A

complementary nonparametric sensitivity family was provided by Wilcoxon signed-rank tests and rank-biserial effects. Stability of scores supplied was summarized by test-retest correlations.

The ten nodes in each wave were then standardized in an exploratory regression system and each T2 node regressed on all ten T1 nodes. The data were reported on the 100 coefficients for HC3 errors, Benjamini–Hochberg adjustment, and Holm adjustment. It is an unregularized system, and does not replicate the panel-network algorithm in the source article. Does not detect intra-individual causal processes. Organizational support was not reinterpreted as principal leadership and career satisfaction was not reinterpreted as engagement.

## 4. Minimax Decision Framework

### 4.1. Policies, losses, and uncertainty

Figure 1 separates the conceptual decision pathway from the information supplied by the surveys. Let  $S$  be a finite set of policy intensities. A policy  $s$  has an assumed nonnegative psychological-capital gain  $g_s$  and a nonnegative implementation cost  $c_s$ . The uncertain coefficient  $b$  represents hypothetical transmission of a resource gain to engagement, and  $t$  represents implementation attenuation. The cost weight  $w$  is fixed within a decision instance. The model uses normalized utility units, not raw engagement scale points.

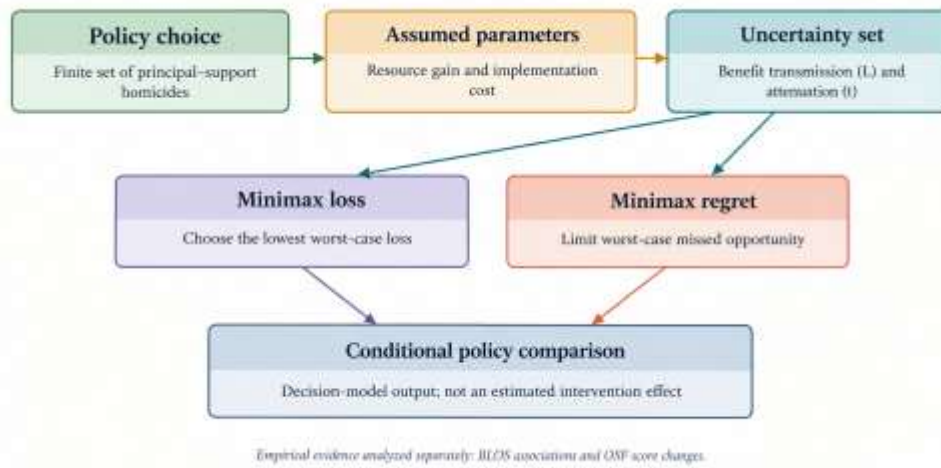


**Figure 1. Conceptual minimax framework and separate survey illustrations. The principal-strategy pathway is a decision-model assumption, not a jointly estimated empirical mechanism.**

The loss and uncertainty set are:

$$L(s; b, t, w) = w c_s - b t g_s. \quad (1)$$

$$U = [\underline{b}, \bar{b}] \times [\underline{t}, 1], \text{ with } 0 \leq \underline{b} \leq \bar{b} \text{ and } 0 \leq \underline{t} \leq 1. \quad (2)$$

The minimax-loss decision is:

$$s^* \in \arg \min_{s \in S} \max_{(b,t) \in U} L(s; b, t, w). \quad (3)$$

The model separates assumptions from estimation. None of  $g_s$ ,  $c_s$ ,  $b$ , or  $t$  was estimated as a leadership-intervention parameter from the surveys. Values are illustrative design choices. They are neither fabricated observations nor calibrated effects. Individual transformational-leadership dimensions cannot be ranked from these inputs.

### 4.2. Elementary optimality conditions

**Proposition 1.** For nonnegative gains and bounds, the worst-case loss of policy  $s$  is  $w c_s - \underline{b} \underline{t} g_s$ .

**Proof.** The product  $b t$  is nondecreasing in each coordinate on  $U$ . Because  $g_s \geq 0$ , loss is nonincreasing in each coordinate. Its maximum is attained at  $(\underline{b}, \underline{t})$ . This remains true when a gain or a lower bound equals zero.

**Corollary 1.** If the status quo has zero gain and cost, every active policy has positive cost, and  $\underline{b} = 0$  or  $\underline{t} = 0$ , the status quo is the unique minimax-loss policy for  $w > 0$ .

**Proof.** The status quo has worst-case loss zero. Every active policy has worst-case loss  $w c_s > 0$  when the lower-bound benefit product is zero. Comparing these values gives the result.

**Corollary 2.** An active policy weakly improves on the status quo under minimax loss if  $\underline{b} \underline{t} g_s \geq w c_s$ . For  $g_s > 0$ , the required lower-bound product is at least  $w c_s / g_s$ .

These statements are direct consequences of the chosen loss and are not presented as new general minimax theorems. Their application value is diagnostic. A recommendation for investment requires a sufficiently positive lower bound on benefit transmission and implementation, not simply a positive average survey correlation.

### 4.3. Regret and finite computation

Regret compares a policy with the best available policy after a scenario is known:

$$R(s; b, t, w) = L(s; b, t, w) - \min_{a \in S} L(a; b, t, w). \quad (4)$$

$$s^* \in \arg \min_{s \in S} \max_{(b,t) \in U} R(s; b, t, w). \quad (5)$$

For this model, set  $z = bt$ . Regret is the maximum over  $a \in S$  of  $w(c_s - c_a) - z(g_s - g_a)$ , a maximum of affine functions of  $z$ . It is therefore convex in  $z$ . The feasible  $z$  interval is  $[bt, \bar{b}]$ . Its maximum is attained at an endpoint; evaluating rectangle corners consequently includes all required endpoint cases. This argument justifies the computation for this loss structure. It would not justify corner enumeration for an arbitrary nonlinear payoff.

#### 4.4. Illustrative parameter grid

Table 2 gives the assumed policies. They are generic intensities and have no empirical mapping to individualized consideration, inspirational motivation, or intellectual stimulation. The lower bound  $\underline{b}$  took values 0, 0.1, 0.2, and 0.4; the upper bound  $\bar{b}$  was 0.8. The lower attenuation bound  $\underline{t}$  took values 0, 0.25, 0.5, and 1. Cost weights were 0.5, 1, and 2. The full factorial design contains 48 settings. Each setting was evaluated under both decision criteria, with ties retained.

**Table 2. Hypothetical policy assumptions in normalized utility units.**

| Policy           | Assumed gain | Assumed cost |
|------------------|--------------|--------------|
| Status quo       | 0.00         | 0.000        |
| Low intensity    | 0.15         | 0.025        |
| Medium intensity | 0.30         | 0.070        |
| High intensity   | 0.50         | 0.150        |

*Note. All values are illustrative assumptions, not measured intervention effects. No strategy-dimension mapping is asserted.*

The base calculation contains 192 worst-case policy losses. The regret calculation evaluates four corners for each setting and policy, giving 768 rows; coincident corners were retained for consistency. Grid frequencies are sensitivity summaries, not probabilities or estimated frequencies of optimal principal behaviour.

## 5. Results

### 5.1. Survey descriptives and associations

Table 3 reports the PLOS composite summaries and alpha intervals. Internal consistency was high, but this does not resolve adaptation and coding issues. Engagement means refer to observed 1–7 coding. Under the hypothetical minus-one shift, the engagement mean would change from 4.784 to 3.784; slopes and correlations would remain unchanged.

**Table 3. PLOS composite descriptives and internal consistency (n = 1,033).**

| Scale | Mean  | SD    | Alpha | 95% alpha CI |
|-------|-------|-------|-------|--------------|
| AL    | 3.691 | 0.917 | 0.953 | 0.946–0.958  |
| SC    | 3.784 | 0.602 | 0.916 | 0.902–0.928  |
| TE    | 6.291 | 1.444 | 0.965 | 0.960–0.969  |
| WE    | 4.784 | 1.222 | 0.956 | 0.950–0.960  |

*Note. AL: authentic leadership; SC: school climate; TE: teacher efficacy; WE: work engagement. Alpha intervals use 1,000 resamples. WE retains observed 1–7 coding; scoring remains partly provisional.*

Authentic leadership correlated with climate at 0.498, efficacy at 0.272, and engagement at 0.301. Climate correlated with engagement at 0.386, whereas efficacy correlated with engagement at 0.645. The leadership–engagement Pearson interval was [0.244, 0.355], and the efficacy–engagement interval was [0.607, 0.679]. Spearman correlations for these two pairs were 0.288 and 0.660. The patterns are consistent with related resource studies, but they concern the measured constructs rather than the title's complete pathway.

### 5.2. Exploratory decomposition and predictive performance

Table 4 reports the bootstrap decomposition. All three indirect-association intervals excluded zero. Their sum was 0.300, while the direct coefficient was 0.101 with an interval spanning zero. The total association was 0.401. These are unstandardized composite-score quantities, not latent structural effects or principal intervention estimates.

**Table 4. Exploratory PLOS indirect-association decomposition.**

| Path              | Estimate | Bootstrap SE | 95% CI       |
|-------------------|----------|--------------|--------------|
| AL → SC → WE      | 0.093    | 0.032        | 0.028–0.154  |
| AL → TE → WE      | 0.092    | 0.034        | 0.029–0.162  |
| AL → SC → TE → WE | 0.116    | 0.021        | 0.078–0.161  |
| Total indirect    | 0.300    | 0.043        | 0.218–0.389  |
| Direct            | 0.101    | 0.054        | -0.002–0.212 |
| Total             | 0.401    | 0.050        | 0.304–0.499  |

*Note. Unstandardized effects; 5,000 percentile bootstrap resamples. These are composite-score associations involving authentic leadership, climate and efficacy, not causal PsyCap mediation.*

In the joint engagement regression, leadership had  $b = 0.101$  (HC3  $p = .063$ ), climate  $b = 0.283$  ( $p = .004$ ), and efficacy  $b = 0.485$  ( $p < .001$ ). R-squared was 0.446. With demographic adjustment, the corresponding coefficients were 0.100, 0.271, and 0.481; R-squared was 0.455. The maximum variance inflation factor was 1.438. Demographic adjustment did not turn the direct leadership association into a conventionally significant result.

The primary sample contained 1,033 records. Removing respondents constant within all four blocks left 1,012; the influence-based sensitivity left 976. These variants are reported in the computational supplement. They should not be used selectively to replace the primary result. The repeated held-out prediction results appear in Table 5. The joint model reduced average error relative to both the leadership-only model and the training-mean baseline. This supports its descriptive predictive usefulness within respondent-level splits; it does not establish transfer to unseen schools or improvement caused by changing a predictor.

**Table 5. Repeated respondent-level predictive validation.**

| Model    | RMSE  | MAE   | Baseline RMSE |
|----------|-------|-------|---------------|
| AL SC TE | 0.910 | 0.660 | 1.220         |
| AL only  | 1.165 | 0.912 | 1.220         |

*Note. Averages across five repetitions of ten folds per model. Baseline predicts the training-fold mean. Folds are not independent; school-level transfer was not assessed.*

### 5.3. Psychological-capital change

Table 6 presents the supplied OSF psychological-capital scores. PC1, PC3, and PC4 declined after Holm adjustment; PC2 did not. Standardized within-person changes were modest. Organizational support showed a mean change of  $-0.016$  and an adjusted  $p$  value of .684. The numerical labels are retained because component meanings were not verified. Figure 2 presents the psychological-capital change intervals without assigning unsupported dimension names.

**Table 6. OSF supplied psychological-capital score changes ( $n = 431$ ).**

| Score | Change | 95% CI       | Paired dz | Holm p |
|-------|--------|--------------|-----------|--------|
| PC1   | -0.182 | -0.260–0.099 | -0.216    | < .001 |
| PC2   | -0.094 | -0.183–0.001 | -0.095    | 0.194  |
| PC3   | -0.161 | -0.252–0.070 | -0.171    | 0.002  |
| PC4   | -0.166 | -0.247–0.088 | -0.193    | < .001 |

*Note. Change =  $T2 - T1$ . Scores retain source labels; dimensions are not assigned. Intervals use 5,000 resamples; Holm correction covers all ten paired tests, not only these four.*

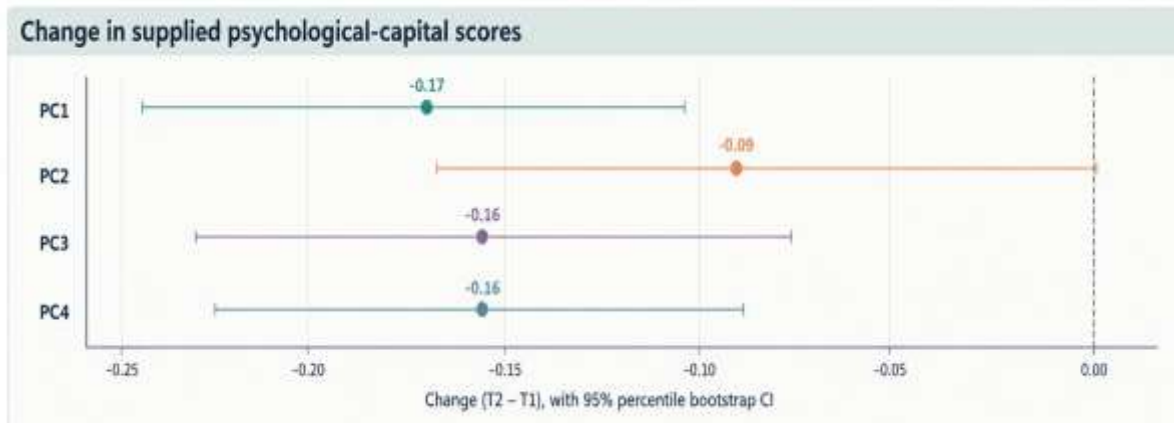


**Figure 2. Changes in supplied OSF psychological-capital scores, with 95% percentile bootstrap intervals.**

**Dimension labels remain unverified; points refer to source PC1–PC4 scores.**

The supplementary nonparametric tests and T1-to-T2 regressions address different statistical questions. Neither supplies an engagement outcome or a principal strategy effect. Changes in the OSF scores cannot therefore be treated as estimates of the hypothetical gains in Table 2.

### 5.4. Worst-case decisions and regret

In the 48 settings, minimax loss chose status quo in 38 settings and low intensity in six while high intensity was chosen only in one. Medium and high intensity were tied in three settings. When the cost weight is equal to one, the difference between loss and regret is shown in Figure 3. The frequent selection of the status quo follows from the specified uncertainty bounds and positive implementation costs; it is not an empirical recommendation against investment in teachers.



**Figure 3. Illustrative minimax-loss and minimax-regret policies at cost weight one. SQ: status quo; Low, Med and High: assumed intensity; M/H: medium/high tie. Cells are deterministic model outputs, not empirical recommendations.**

For a setting with  $\underline{b} = 0$ ,  $\underline{t} = 0$ , and  $w = 1$ ,  $b_L t_L$  minimax loss selected the status quo, whereas minimax regret selected medium intensity. Its maximum regret was 0.080. The active alternative hedges against the opportunity loss of doing nothing when benefit transmission is high. Under minimax loss, however, the zero-benefit scenario makes its cost decisive. Both answers are internally coherent, but they respond to different objectives.

## 6. Discussion

The main finding is that a strong recommendation requires that a particular type of evidence be included in the uncertainty set. Positive survey association does not rule out null intervention benefit. If the decision maker is willing to take a risk that the transmission is zero or that any implementation will lead to an at least complete attenuation, then there is no guarantee that any action taken under an active policy will yield a benefit and the associated costs will be measured in terms of a potential reduction in value from the baseline. The recommendation of status-quo should be viewed as an indicator of lack of substantiating decision evidence, rather than as a substantive opinion about transformational leadership.

The survey responses provide more limited but valuable information. The PLOS data lend support to the associations between authentic leadership, climate, efficacy, and engagement. They also demonstrate that indirect decomposition cannot establish a complete causal account: the direct causal effect is not identified, all variables were measured cross-sectionally, and school-level clustering information was unavailable. The OSF data indicate modest declines in three of the four supplied psychological-capital scores across the two waves, although engagement was not measured. They inspire people to look at the variation in resources but fail to point to the strategy effects required for optimization.

This separation is consistent with the wider literature. Psychological capital is found in the research on engagement, burnout, performance, commitment, and turnover (Guo et al., 2022; Hu et al., 2022; Ma, 2023; Sarwar et al., 2023; Zhao et al., 2022; Zhu et al., 2022). Such results should not be combined into a single benefit measure. Heterogeneity is suggested from this profile-based evidence (Gao et al., 2023; Pöysä et al., 2022) but still not revealed any intervention responses that reflect the subgroups. A school-level support might be different and individual perceptions vary as well (Hsieh et al., 2022), so unobserved school context cannot be simply deemed as a minor technical omission.

The analysis provides a meaningful evidence threshold for minimax modelling. For any policy to be better than the status quo, it needs to offer a lower-bound benefit product, which will encompass the weighted cost-to-gain ratio. This condition determines which unknowns are important to decision. A future study does not require the precise estimation of each possible psychological mechanism. It should focus on gains specific to the policy, fidelity of policy implementation, effects on engagement transmission, and costs close to the policy switches.

For practical planning, the decision framework can help principals specify what must be learned before committing resources. A pilot strategy could document staff participation, implementation time, expenditure, and changes in verified psychological capital and engagement measures. These observations would inform subsequent uncertainty bounds only when the evaluation design supports the intended interpretation. Decision makers should also state whether their priority is limiting expenditure under adverse conditions or reducing missed opportunities under favourable conditions. Recording this objective before comparing policies would make recommendations easier to explain and review. Such an application remains prospective; the current survey analyses do not validate it.

Regret, a second look. It can choose an active strategy over the absolute worst-case scenario, when that criterion is for inactivity. This disparity is no justification for making regret the universal preference. Regret is dependent on which options are incorporated and relates policies to a policy-specific benchmark. The question of whether to focus on avoiding expenditure is more fitting than avoiding missed opportunity is opened up by a substantive account that is necessary to answer the question of the choice between loss and regret (Anderson & Zachary, 2023).

There is just a limited scope for math. The model is of the type nonnegative gains + bilinear benefit term, so endpoint evaluation is adequate. If the effects of adverse intervention, differential benefits by teacher, or uncertainty about costs, or a budget allocation vector were present, the problem would be altered. In the future, distributional robust methods can be a more interesting choice when an appropriate intervention-effect data set is available (Duchi & Namkoong, 2021; Lin et al., 2022; Van Parys et al., 2021). Those guarantees are inappropriate to be applied to unmatched observational surveys.

## 7. Limitations and research priorities

Firstly, there is no evidence of the full transformational-leadership–psychological-capital–engagement chain in both datasets. Second, there is no documented standard scoring, which means that the exact archived adaptation can not be determined. The order of leadership items, reverse-key, engagement coding and OSF component mapping are not fully resolved. One source issue is the PLOS sample discrepancy, and the other issue is an invalid response. High alpha is not enough to answer these questions.

Third, resampling of teacher records assumes independence of teacher records. School-level dependence is not explained by intervals and held-out errors, because these data were not available. Fourth, it was not tested for OSF time interval and measurement invariance. Within-person and between-person variation are confounded by temporal associations. Fifth, the minimax parameters are hypothetical – none of the policy gain, costs, or transmission coefficient are calibrated from the surveys. The counts of the cells of the selected grid are not inferential in nature.

An empirical defensible extension would be based on an existing or newly collected joint dataset of teacher(s) that would include verified instrument mappings, school identifiers, strategy exposure, fidelity measures and cost information. Interpretation would be strengthened if there were a variation of randomized or credible quasi-experimental strategies. Until then, the current framework is just a planning representation and not a validated recommendation system.

## 8. Conclusions

This study helps to understand when minimax decision-making can be used to assist principals in determining leadership strategies to enhance teachers' work engagement. Empirical evidence on the measured psychological resources, leadership-related factors and engagement is presented in both separate Chinese teacher surveys, but neither of these datasets includes all stages of the pathway between the factors of transformational leadership and psychological capital and work engagement. Therefore, the empirical results are not sufficient to confirm that the psychological capital dimension of engagement is the result of principal interventions. Instead, the study creates a clear decision-making process based on clearly articulated hypothetical policy gains, uncertainty of implementation, and costs. The results show that with zero benefit as a possible outcome, and positive cost of the policies, it is the status quo which is optimal under the minimax-loss criterion. Minimax regret can, however, support an active policy by taking into account the opportunity cost of not seeing desirable results. The contrasting decisions are provided to show how recommendations vary according to the decision criterion and uncertainty assumptions used. The framework also establishes criteria for the evidence needed to support more defensible leadership decisions, specifically benefits of interventions, fidelity of implementation, transmission of engagement, and costs. Integrated teacher-level datasets with instruments of measurement with known reliability and appropriate designs of interventions should be employed for future research to estimate these parameters. The overall study does not yield a definitive recommendation for leadership based on an empirical study but rather a framework for making decisions about leadership in light of the conditions of the study. Also, the study does not show how teachers' engagement improves as a result of transformational leadership strategies.

## Data and code availability

The PLOS data are available through <https://doi.org/10.1371/journal.pone.0320839.s001>. The OSF repository is [https://osf.io/tuaws/?view\\_only=cf316099765e4c1fa03547ffe814fde2](https://osf.io/tuaws/?view_only=cf316099765e4c1fa03547ffe814fde2). The accompanying 293-zip-results package contains executed scripts, numerical outputs, source hashes, and rerun instructions. Analyses were executed locally; the accompanying notebook permits rerunning on Google Colab. The raw datasets are not pooled or redistributed in the manuscript supplement.

## Research ethics

The work involved secondary analysis of publicly shared survey records and no new participant recruitment or contact. Original study governance remains the responsibility of the source studies. The analysis reports aggregate statistics and does not attempt participant identification. No new ethics approval number or exemption is asserted.

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