

When Does Network Restructuring Improve Organizational Search? A Computational Study of Dynamic Influence Networks on NK Landscapes

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Abstract

Organizations often restructure their internal communication networks expecting enhanced search performance. However, the conditions under which restructuring helps remain poorly understood in existing organizational studies. To investigate, this study introduced a computational framework grounded in the NK landscape model to examine how dynamic restructuring of influence networks affects organizational search. The framework compared four restructuring rules (passive reset, passive dyad switching, agent-initiated rewiring, and designer-initiated phase-based restructuring) against static baselines across six levels of problem complexity. Using 6000 simulation runs, the study found that restructuring has limited effects at moderate complexity, but designer-initiated restructuring significantly improves search performance at the highest complexity level ($K = 11$, $p = 0.002$, Cohen's $d = 0.31$). No other restructuring rules produced significant improvement. These results challenge the assumption that restructuring universally benefits organizational search and suggest that its value is narrowly bounded by problem complexity.

Keywords: *NK model; network restructuring; exploration-exploitation; organizational search; complex adaptive systems; agent-based simulation*

1. Introduction

The tension between exploration and exploitation is one of the most fundamental challenges in organizational adaptation (March, 1991). Organizations must exploit existing knowledge to improve current performance while simultaneously exploring new possibilities to ensure long-term adaptability (O'Reilly & Tushman, 2008; Clauss et al., 2020). Levinthal and March (1993) warned that adaptive processes tend to refine exploitation more rapidly than exploration, causing systems to be effective in the short run but self-destructive in long run. Resolving this tension is therefore not a matter of choosing one mode over the other, but of designing organizational structures that sustain both (Zahoor et al., 2023).

The NK fitness landscape model (Kauffman, 1993; Kauffman & Levin, 1987) has become a foundational computational tool for studying this tension. By representing an organization's problem space as a landscape of interdependent decisions, where the parameter K controls the

ruggedness of the terrain. The model allows researchers to examine how search behavior and organizational design interact with problem complexity (*Gavetti & Levinthal, 2000; Csaszar, 2018*). Simulation method is well suited to this domain as they permit controlled experimentation with structural variables that are difficult to manipulate empirically (*Harrison et al., 2007; Wu & Sekiguchi, 2023*).

Within this tradition, *Lazer and Friedman (2007)* demonstrated that the structure of an organization's communication network fundamentally shapes search performance. Dense network accelerates exploitation by rapidly diffusing superior solutions but suppress premature convergence on local optima. Sparse network preserves solution diversity but cannot diffuse improvements effectively, leading to higher discovery but low average performance. This trade-off has been replicated and extended in later works which showed the distribution of diversity within a network, not just its overall density. It helped in determining whether the system can escape local optima and reach global optima (*Barkoczi & Galesic, 2016*). Based on the existing evidence, no static network structure resolves the exploration-exploitation tension (*Faroque et al., 2020; Sun et al., 2023*).

This raises a natural question: if no static structure is optimal, can organizations benefit by dynamically restructuring their influence networks over time? The idea is intuitively appealing. Managers frequently reorganize teams, departments, and reporting lines with a belief that structural changes improve performance. Empirical research on corporate restructuring has produced mixed and contingent results. For instance, a study by *Girod and Whittington (2017)* found that pervasive restructuring generally improves performance in stable environments but harms dynamic ones, while incremental reconfiguration shows the opposite pattern. *Bowman (et al., 1999)* similarly asked “when does restructuring improve economic performance?” Similarly, *Umar (2023)* examined literature on corporate restructuring potential in enhancing organizational performance with definitive claims. Prior literature indicated that the answer of improvement depends on context-specific factors that remain underexplored. These empirical ambiguities persist only because the causal mechanisms linking structural change to search performance are difficult to isolate in field settings.

Computational research has begun to address this gap, though evidence is fragmented. One of the early works by *Siggelkow and Levinthal (2003)* showed that temporary decentralization (sequentially dividing and reintegrating an organization) outperforms both permanent centralization and permanent decentralization when decision interdependencies are pervasive.

This logic suggests that the timing of structural change matters as much as direction. *Wall (2015)* extended this by showing that inducing temporal change to organizational dynamics and temporal changes to organizational setup during search can increase performance. However, the mode of change and problem complexity play a key role in determining magnitude of benefits. Recently, *Blanco-Fernández (et al., 2024)* examined dynamic team composition in an NK framework and found that benefits of periodically changing a team's composition depend highly on coordination and task complexity. *Songhori and Garcia-Diaz (2018)* modelled evolving networks where agents endogenously create and serve ties through reinforcement learning, showing that the interaction of individual learning and structural adaptation produces performance differences that are contingent on aspirations levels and landscape complexity.

Despite these advances, literature lacks a systematic comparison of restructuring rules (passive vs active, centralized vs decentralized) tested against a common static baseline across a wide range of problem complexities. Existing studies mostly examine one restructuring mechanism in isolation, under specific parameter settings, and at a single complexity level. This makes it difficult to determine whether the benefits of restructuring are general or contingent, if contingent on what.

The study addressed this gap by introducing a computational framework that modelled dynamic influence network restructuring on NK landscapes. Our framework implements four restructuring rules spanning a passive-active dimension: passive random reset, passive dyad switching, agent-initiated rewiring, and designer-initiated restructuring. We tested these rules against static baselines across six levels of complexity ($K \in \{0, 2, 4, 6, 8, 11\}$) with 50 replications per condition, yielding 6,000 simulation runs.

The study contributes to computational organizational literature in two ways. First, it introduced a reusable computational framework for studying dynamic network restructuring, extending the static network paradigm of *Lazer and Friedman (2007)*. Second, the empirical evidence of the study extends the argument benefits of restructuring are contingent on problem complexity.

2. Literature Review

2.1. Exploration, Exploitation, and the Ambidexterity Problem

The distinction between exploration and exploitation is among the most influential concepts in organizational theory. *March (1991)* characterized the two activities by the temporal and spatial distribution of their returns. Exploration includes search, variation, risk-taking, experimentation, and play attributes that yield uncertain returns due to uncertainty, distance in time, and unevenly distribution across organization (*Lavie et al., 2010; Argote et al., 2020*). Exploitation, on the other hand, deals with refinement, choice, production, efficiency, and implementation where the yields are certain, proximate, and localized (*Papachroni and Heracleous, 2020*). Organizations often choose what is safe for now over what is new because improving existing ideas are faster than finding better ones (*March, 1991; O'Reilly and Tushman, 2004; Bruder, 2025*). The result is the competency trap. It makes an organization increasingly efficient at practices that are increasingly obsolete which *Levinthal and March (1981)* highlighted in their work on myopia of adaptative learning. The system that engages in exploitation to the exclusion of exploration is likely to find itself in trap with suboptimal stable equilibria.

The recognition that organizations must sustain both activities to survive long-run environmental change produced the literature on organizational ambidexterity. The term traces to the argument, rooted in the classical distinction between mechanistic and organic structures (*Burns & Stalker, 1961*) that organizations facing shifting demands require dual structures (one for routine performance and one for innovation). *Tushman & O'Reilly, (1996)* laid the modern-day research on organizational design argued that firms must host 'multiple contradictory structures, processes, and cultures within the same firm' to succeed at both evolutionary and revolutionary changes. Three routes to this capacity have since been elaborated. First, structural ambidexterity separates exploratory and exploitative units, each internally coherent and bridged by senior management (*Benner and Tushman, 2003; O'Reilly & Tushman, 2013*). Second, contextual ambidexterity relocates the balance to the individual level, relying on organizational contexts that let employees allocate their own effort between alignment and adaptation (*Gibson & Birkinshaw, 2004; Ossenbrink et al., 2019*). Third, temporal (sequential) ambidexterity alternates between the two activities over time rather than pursuing both at once through time-paced rhythms of change (*Brown & Eisenhardt, 1997; Wang et al., 2018*) or deliberate toggling between organizing modes selected to match the search problem at hand

(Nickerson & Zenger, 2002, Yi et al., 2016). Comparing the three types of ambidexterity, temporal or dynamic one has scarce empirical evidence than structural ones (O'Reilly & Tushman, 2013).

The empirical case for ambidexterity is substantial. For instance, He and Wong (2004) found that manufacturing firms pursuing both exploration and exploitation found to be outperforming than those emphasizing either of the two. Similarly, Jancenelle, (2019) 5-year longitudinal study with S&P firms found that firms deliver highest level of performance with balanced ambidexterity. However, the association with performance is curvilinear in an unstable environment. Numerous studies have found a positive association of ambidexterity and firm performance (Lavie et al., 2010; O'Reilly & Tushman, 2013; Dranev et al., 2018). The concept has grown nonetheless grown contested. For instance, Gupta (et al., 2006) questioned whether exploration and exploitation are ends of a single continuum or orthogonal dimensions organizations can pursue simultaneously. Organizational design scholars have struggled to impose order on a construct that now spans multiple levels of analysis and an expanding set of mechanisms (Rojas-Córdova et al., 2022, Srikanth & Ungureanu, 2024).

Across these developments, one question remains persistently underspecified: How do specific organizational structures enable organizations to balance both exploration and exploitation? The ambidexterity literature names categories of solutions: separate units, supportive contexts, alternating phases but rarely models the mechanism by which the arrangement of communication among members produces, and might resolve, the underlying trade-off. Formalizing that mechanism is the contribution of computational tradition examined in this study by characterizing the problem environments in which search unfolds.

2.2. The NK Model as a Theory of Problem Complexity

The NK model introduced in evolutionary biology by Kauffman and Levin (1987) represents a decision problem as landscape over N binary choices in which each choices' payoff contribution depends on K other choices. When $K=0$, the landscape is smooth and single-peaked. As K increases toward $N-1$ the number of local optima grows rapidly and the landscape becomes rugged, so that incremental (local) search becomes increasingly trapped on inferior peaks (Kauffman, 1993). Levinthal (1997) adapted this framework into organizational design research to explain why organizations adapting through local experimentations so often cover suboptimal configurations and showed how the difficulty of adaptation rises with interdependence.

Subsequent works on the NK model as general-purpose laboratory for organization and strategy research. *Rivkin (2000)* used this model to explain why complex strategies are difficult to imitate. *Gavetti and Levinthal (2000)* contrasted cognitive and experimental search. *Rivkin and Siggelkow (2003)* showed that design elements encouraging broad search raise the marginal value of elements providing stability. *Knudsen and Levinthal (2007)* demonstrated the value of separating generations of alternatives from their evaluation. *Csaszar (2013)* mapped organizational structure onto exploration-exploitation frontier. *Baumann (et al., 2018)* counts 71 NK simulation studies from 1997 published in leading management journals and categorized into six themes (learning modes, problem decomposition, cognitive representations, temporal dynamics, distributed search, and search under competition). Across all six themes, complexity (K) is pivotal manipulated variable in majority of reviewed articles. The framework's enduring appeal lies precisely in this control. Problem complexity can be manipulated in a manner that empirical settings cannot isolate (*Harrison et al., 2007; Billinger et al., 2014; Baumann et al., 2018*).

Importantly, the NK model has also migrated from the computer to the laboratory. This experiment puts real people into complex decision scenarios to see if they use the strategy predicted by computer models: making small, steady improvements mixed with the occasional random risks. The paradigm was pioneered by *Billinger (et al., 2014)* who found that success narrows human search to the neighborhood of status quo while failure induces progressively wider exploration. The complexity in such case affects the behavior not directly but through the feedback conditions it creates. Further, *Billinger (et al., 2020)* showed that early-stage and separate feedback separately govern continuous search and the extent of the search. *Billinger (et al., 2023)* in a recent study showed that dyads searching interdependent modules learn to coordinate their joint search despite complexity and misaligned incentives. Complementary group-level experiments confirm that structure and complexity jointly shape collective outcomes in networked human workgroups (*Mason & Watts, 2012; Almaatouq et al., 2021*). While most simulation works continue to return complexity to center of design questions, *Heydari (et al., 2026)* showed that the optimal allocation of heterogeneous inventors between the core and periphery of a firm's network structure reverses between low and high-end complexity landscapes. **Table 1** summarizes representative studies from both computational and experimental streams that emphasize how each treated complexity.

Table 1. Representative NK Model Studies in Management Research

Study	Method	Focus	Key Findings
Levinthal (1997)	Simulation	Organizational adaptation	Higher K degrades adaptation, local search traps organizations on local peaks
Rivkin (2000)	Simulation	Imitation	Complexity makes successful strategies difficult to imitate faithfully.
Gavetti and Levinthal (2000)	Simulation	Cognitive vs experimental Search	Cognitive representations can guide search beyond the nearest local peak.
Siggelkow and Levinthal (2003)	Simulation	Temporary decentralization	Temporary divisions outperform permanent structures when interdependence is pervasive.
Knudsen and Levinthal (2007)	Simulation	Stages of search	Separating alternative generation from evaluation improves complex search
Mason and Watts (2011)	Online experiment	Networked search	Efficient networks outperformed inefficient ones, reversing computational predictions
Billinger (et al., 2014)	Lab experiment	Human search behavior	Success narrows and failures widen search; complexity acts through feedback, not directly
Wall (2015)	Simulation	Temporal modes of organizational change during search	Mid-search organizational dynamics can raise search effectiveness, but the benefit's size depends on the mode of temporal change and problem complexity; dynamics shift search toward exploration
Songhori and García-Díaz (2018)	Simulation	Endogenous network evolution in collective search	Agents learn to create and sever ties via reinforcement learning; performance effects are contingent on aspiration levels, learning rates, and problem complexity
Almaatouq et al., (2021)	Lab experiment	Task complexity and group synergy	Complexity moderates whether groups outperform their best member
Billinger et al., (2020)	Lab experiment	Feedback and search decisions	Early and recent feedback separately shape whether and where search continues
Billinger et al., (2023)	Lab experiment	Collaborative search in dyads	Dyads learn to coordinate joint search despite complexity and misaligned incentives.
Heydari et al., (2025)	Simulation	Talent allocation in networks	Optimal core-periphery allocation of heterogeneous searches reverses with complexity

The NK model is a matured and consolidated research program that had decades of simulation results, and a growing body of experiments converge on the conclusion that problem complexity is the central agency in organizational search. The experimental stream of the model has validated the core behavioral premise (bounded, feedback driven search) survives laboratory scrutiny where humans not always are greedy hill-climbers at early stage, but they over-explore early and respond to temporal pattern of feedback. The frontier of the field has shifted to distributed search that defines interconnectedness of searchers to be organized (precisely where complexity, structure, and adaptation interact). The current study follows the tradition of treating K as theoretically motivated moderator of any structural interventions in search.

2.3. Network Structure and Collective Search Performance

The intellectual foundation of this stream stems from *Lazer and Friedman's (2007)* work who modelled teams of agents searching for an NK landscape and learnings from their neighbors. Comparing structures that range from sparse local lattices to fully connected networks, they demonstrated a fundamental tradeoff. If a network more efficiently disseminates information, the better it will be for the system's short-run performance but the worse its long-run performance. Rapid diffusion destroys the diversity on which exploration depends, producing premature convergence. For immediate time horizons, connectedness exhibits an inverted U relationship with performance.

Subsequent work has mapped this tradeoff across the principle structural dimensions an organization can design. Along with density, the foundational result is robust in simulation which *Fang (et al., 2010)* showed that semi-isolated sub-groups linked by sparse bridges balance exploration and exploitation better than either fully connected or disconnected designs, echoing the logic of small world structures (*Watts & Strogatz, 1998*). Along the hierarchy, *Koçak (et al., 2022)* identified the 'dual challenge' of search and coordination: flat influence structure fosters exploration but struggle to converge, whereas hierarchical influence improves coordinated adaption even when the apex actor holds no superior knowledge. *Heydari et al. (2025)* showed that problem complexity dictates where heterogeneous searchers perform best. On rugged landscapes, independent searchers excel at the network's core; on smooth landscapes, they belong at the periphery. *Barkoczi and Galesic (2016)* found that networks that help one type of social learner actively harm others. *Baumann (et al., 2024)* in their study revealed that skill diversity impairs performance in simple tasks but boosts it in complex ones, and only in sufficiently dense networks.

Experimental tests, however, have not simply confirmed that computational story rather they complicated it. *Mason and Watts (2011)* in 256 web-based experiments, found that efficient networks outperformed inefficient ones even in a problem space whose properties were thought to favor the latter, reversing the foundational prediction. Their explanation is behavioral. Contrary to the theory, participants in efficient networks explored more, and network structure shifted individuals explore-exploit decisions. *Brackbill and Centola (2020)* in their field experiment found computational prediction upheld where groups with insufficient networks consistently discovered better solutions because of slower diffusion that preserved the diversity of exploration. *Almaatouq (et al., 2021)* in their controlled laboratory experiments found that task complexity moderates whether groups outperform their best individual member. Based on available evidence, no single structure dominates, and the effect of any given structure depends on the agents' learning behavior and most robustly, the complexity of task (*Centola, 2022*).

In existing literature, one structural assumption is common: the network is fixed. Structure is treated as a design parameter chosen before search begins and held constant while the group works. Even in experimental studies, the network remains frozen for the duration of the task. Organizational design scholars have suggested to move beyond 'collective intelligence' paradigm to a 'collective adaption' paradigm, make the limitation explicit. As notified by *Galesic (et al., 2023)*, the pressing question is now: *how do collectives change their strategies and network structures in response to the problems they face?* This study looks at the focal point whether and when restructuring networks during search improves performance, and if so, by what rule of structuring.

2.4. From Static Structuring to Dynamic Restructuring

Two literatures bear on the questions of whether and under what conditions changing an organization's structure during search improve performance. *Rumelt (1974)* documented the historical relationship between strategy and structure. *Bowman (et al., 1999)* pursued on 'when does restructuring improve economic performance?' over reviewing 52 studies found that organizational restructuring shows a little consistent impact compared to financial and portfolio restructuring. In recent years, *Girod and Whittington (2017)* study with large U.S. corporations showed a pervasive restructuring and incremental configuration have opposite performance consequences that reverse depending on environmental dynamism. Part of the ambiguity is mechanical where reorganization in the field is endogenous to performance and confounding with simultaneous changes in strategy, personnel, and market. Part of it is substantive where the structural inertia holds. Organizations pay a real 'liability of change' for departing from

established routines (*Hannan & Freeman, 1984*). Change in practice tends to arrive not as continuous fine-tuning but as rare, punctuated transformations (*Romanelli & Tushman, 1994*). Therefore, field studies cannot be isolated by the casual distribution of structural change itself. Computational literature takes up exactly this problem under controlled conditions. Its studies can be organized by who or what initiates and shapes the change, a distinction that proves consequential.

The earliest and most influential results involve designer-led change. *Siggelkow and Levinthal (2003)* showed that temporary decentralization (dividing an organization into independent divisions for a period and then reintegrating) outperforms both permanent centralization and permanent decentralization where interdependencies among decisions are prevalent. *Siggelkow and Rivkin (2005)* extended the analysis to turbulent environments, showing that the payoff to board search and structural configuration depends jointly on complexity and the speed of environmental change. The logic of such deliberate alternation between organizing modes was anticipated theoretically by *Nickerson and Zenger (2002)*, who argued that organization should toggle between governance forms as relative costs of search and coordination shift.

A second cluster induces structural dynamics exogenously or lets them emerge. *Wall (2015)* systematically varied the mode of temporal change in the organizational setup of distributed search processes and found that organizational dynamics can improve search effectiveness. However, the mode of change and complexity jointly determines the magnitude of benefits, that dynamics systematically shift the exploration and exploitation balance toward exploration. *Wall (2019)* showed that coordination arrangements themselves can emerge through reinforcement learnings as organizations grow, with more hierarchical coordination arising as problem complexity rises and the outcome contingent on search strategy and the cost of the effort.

A third cluster locates the initiatives in the agents or in the composition of the team. *Songhori and García-Díaz (2018)* experimented where the communication network to evolve with agents and sever ties through reinforcement learning. The findings suggested that the performance consequence of endogenous network evolution are contingent on aspiration levels and landscape complexity. *Blanco-Fernández (et al., 2023)* studied periodic changes in group composition and found that reorganizing even well-performing groups can improve task performance in the scenario where individual learning is restricted, and that adaptation can actively harm performance otherwise. Extending this line, *Blanco-Fernández (et al., 2024)*

modeled dynamic team composition as a deliberate design choice in an NK Framework and showed that its performance effect interacts with coordination mode and is moderated by task complexity and individual learning.

Across these clusters, a consistent pattern emerges. Temporal structural change is neither panacea nor a placebo. It is value contingent depending on the complexity and mechanism that initiate change, and on what agents themselves are doing in parallel. How these mechanisms compare with one another, under identical agents, landscapes, and baselines, is precisely what the existing one-mechanism-at-a-time literature has not established.

2.5. Synthesis and Research Gap

Although the four streams discussed above in this segment have developed parallel yet together, they form a coherent arc. The ambidexterity literature names the problem: sustaining exploration and exploitation within a single organization and identifies temporal alternation as the least understood of its solution. The NK tradition supplies the environment in which that problem can be posed with precision where the search task complexity is a controlled and tunable property rather than a nuisance. The network search literature establishes the proximate lever where the communication structure through which solutions diffuse among searchers, and whose effects are contingent on the complexity of the NK tradition allows one to manipulate. The structuring literature opens the possibility that this lever need not be set once and left alone, and the structure itself can be changed during the search. Following its logical conclusion, this arc specifies a complete research program. Given a team of boundedly rational searchers on a landscape of tunable complexity, connected by a modifiable communication network, a pertinent question is yet to be answered: *Does restructuring the network during search improve outcomes, and if so, by which rule, at which complexity, and at what cost?*

Three gaps keep the existing literature from answering the question. *First*, the mechanism has never met one another in the same laboratory. Designer-led, agent-led, and passive dynamics have been studied in separate models with separate baselines, parametrization, so existing evidence cannot say which class of restructuring dominates, and whether any structure beats a static structure at all. *Second*, the boundary conditions are unmapped. Complexity is the field's most consistently replicated moderator, yet structuring studies have found it to have one or two levels of it. Therefore, it remains unknown whether an absence of reported effects reflects genuine ineffectiveness or simply untested regions of complexity space. *Third*, the evidence is inferentially weak. Majority of these studies have compared mean performance across

conditions without hypotheses tests, effect sizes, or corrections of multiple comparisons, leaving reported differences uninterpretable against the stochastic variation randomized landscapes inevitably produce.

This study is built to close all three gaps simultaneously. Its computational framework places four structuring rules spanning from passive to active restructuring and decentralized to centralized dimensions that the literature has so far studied in isolation (under identical agent behavior, identical landscapes, and a common static baseline). In this study, the performance differences are attributable to the rules itself. Further, it varies complexity across the entire usable range, from $K=0$ to the maximally rugged $K=N-1$. It treats simulation replications as experimental data, applying the inferential standards of empirical research that includes *priori* power analysis, hypotheses tests, effects size, and Bonferroni corrections for multiple comparisons. The study does not seek to propose a new theory, instead it operationalizes a missing empirical link by executing the comparative simulation framework necessitated yet left unrealized in existing works.

3. Methodology

3.1. Research Design

The study took a computational simulation approach using NK landscape model (*Kauffman & Levin, 1987; Kauffman, 1993*) to examine *how dynamic restructuring of influence network affects organizational search performance*. The simulation is appropriate for the research question by enabling controlled experimentations with network structures and restructuring rules that would be difficult to manipulate in field settings. Further, it allows systematic variation of problem complexities (*Harrison et al., 2007*). The design is an adaptation of agent-based modelling tradition established by *Lazer and Friedman (2007)*, and extended by *Csaszar (2013)*, *Siggelkow and Levinthal (2003)*, and *Kocak, Levinthal, and Puranam (2023)*.

3.2. The NK Landscape

The NK Model represents the organizational problem space as fitness landscape of N binary decision dimensions where each configuration of choice carries a fitness value. The parameter K controls the interdependence among decisions. For instance, If $K=0$, it is a smooth landscape with single global optimum. On the other hand, $K=N-1$ produces a maximally rugged landscape with many local optima. For this research, the author set $N=12$, yielding $2^{12}=4096$ possible configurations. Subsequently, varied K across six levels - $K \in \{0, 2, 4, 6, 8, 11\}$ to

systematically examine how the problem-complexity moderates the effect of network restructuring. For each (N, K) combination, the landscape is constructed by assigning each dimension 'i' a contribution table of size $2^{(K+1)}$, indexed by state of the dimension i and its K immediate neighbors (following the standard adjacent-neighbor convention; *Kauffman, 1993*). The total fitness of a configuration is the average of all N contributions. Each landscape is seeded for reproducibility, and we enumerated all 4096 configurations to identify both global and all local optima.

3.3. Agents and Search Behavior

The author modeled a team of **20** agents where each holding a binary solution vector of length N representing the agent's current beliefs about all decision dimensions. At each time, every agent updates its solutions through one of the two mechanisms:

Exploitation: The agent observes the solutions of its network neighbors, copies the best performing neighbor's solution if it is superior to its own, and then performs local hill-climbing by testing all N single bit flips and adopting the one that yields the greatest fitness movement. The mechanism captures informational benefits of network ties: agents learn from those they are connected to.

Exploration: With a probability (p_{exp}) of 0.15, the agent deviates from exploitation and instead flips one or more bits at random, with each bit having a mutation rate (m) of 0.15. This mechanism captures experimentation and deviation from established practice. The exploration probability governs the fundamental exploration-exploitation tradeoff at the individual agent level.

3.4. Network Topologies

The influence network among agents is represented as a "direct adjacency matrix" where entry $(i, j) = 1$ indicates that agent i can observe agent j's solution. The author examined four static network structures as baseline:

- I. **Fully connected:** All agents can observe each other (maximum density). This maximizes information diffusion but minimizes diversity of search, as agents rapidly converge towards the same solution.
- II. **Random:** Each possible directed tie exists with probability of 0.3, yielding approximately 30% network density. This represents an unstructured "clueless designer" baseline (*Clement & Puranam, 2018*).

- III. **Ring:** Each agent observes only its two immediate neighbors. This sparse structure preserves diversity but limit information diffusion.
- IV. **Small World:** A ring lattice ($k = 2$ nearest neighbors) where 30% of ties are randomly rewired, combining local clustering with long-range bridges (*Watts & Strogatz, 1998*).

The baselines are derived from *Lazer and Friedman (2007)* static structure experiments which are utilized for identifying network density influence on tradeoff between exploitation and exploration.

3.5. Dynamic Restructuring Rules

The central manipulation of this study was the dynamic restructuring of the influence network over time. The author examined five conditions: a static baseline (no restructuring) and four restructuring rules spanning between passive and active dimensions proposed by the study framework.

- I. **Static Baseline (No Restructuring)-** The network remains fixed for the entire simulation. This condition serves as the control against which restructuring rules are compared. It corresponds to the conventional assumption in organizational design that structure is a fixed managerial choice (*Galbraith, 1974*).
- II. **Passive Restructuring (Random Reset)-** At every $T = 20$ step, all existing ties are dissolved, and a new random network is generated at 30% density. This is a performance independent, exogenous shock (a controlled earthquake that completely resets the communication structure). It represents radical makeovers such as wholesale reorganizations that disrupt all existing relationships regardless of performance.
- III. **Passive Restructuring (Dyad Switching)-** At every $T = 20$ step, each possible directed dyad is independently flipped (connected become disconnected or vice versa) with a probability ($p_{\text{switch}} = 0.10$). Unlike the full reset, this preserves most of the existing structure while introducing gradual random perturbations. It models incremental personnel rotations or periodic reassignment of reporting relationships.
- IV. **Active, Agent-initiated Restructuring-** At every $T = 20$ step, a subset of three agents (randomly selected) rewires their outgoing ties. Each rewiring agent identifies within its second-degree network reach (neighbors of neighbors), evaluates their current fitness, and preferentially attaches to the highest performer, with attachment probability proportional to fitness. To maintain a roughly constant network degree, the rewiring

agent drops one existing tie if the degree exceeds two. This rule models decentralized adaptation where employees endogenously seek high-performing colleagues for informal advice-seeking, mentorship, or self-organized collaboration.

V. **Active, Designer-initiated Restructuring-** A central designer (e.g., a project head) applies phase-based interventions, with each phase lasting $L = 50$ steps and exploration and exploitation phases alternating: steps 0–49 constitute the exploration phase, steps 50–99 the exploitation phase, and this pattern repeats, so that the 200-step simulation contains two complete exploration–exploitation cycles.

- a) Exploration phase (Phase 0)- The designer identifies agents with a degree of network greater than two and removes one to two of their outgoing ties, breaking dense clusters to promote independent exploration. This is a deliberate decentralization to encourage diverse search.
- b) Exploitation phase (Phase 1)- The designer identifies agents with degree one or zero (isolated or near isolated) and connects them to the top-performing agent (top third by fitness). This represents deliberate integration by connecting isolated agents to the dominant cluster to enable diffusion of superior solutions.

This rule models strategic leadership that actively manages the tension between exploration and exploitation by creating different structures for different phases of search processes.

3.6. Experimental Design

The following table (**Table 2**) delineates the experimental parameters.

Table 2. Experimental Design Parameters

Parameter	Value	Description
N	12	Number of decision dimensions
K	0, 2, 4, 6, 8, 11	Interdependence parameter (6 levels)
M	20	Number of agents
p_exp	0.15	Exploration probability per agent per step
m	0.15	Mutation rate during exploration
T	20	Restructuring interval (steps)
L	50	Designer phase length (steps)
p_switch	0.10	Dyad switch probability
Max steps	200	Simulation length
Seeds	50	Replications per cell

3.7. Conditions

The full factorial design crosses:

- I. 6 levels of K (Landscape Complexity)
- II. 5 restructuring conditions (none, passive_reset, passive_dyad, agent_initiated, and designer_initiated)
- III. 4 network structures (random, fully connected, ring, and small world)
- IV. 50 replications per cell

This yields **6000** simulation runs ($6 \times 5 \times 4 \times 50$). Each replication uses a unique random seed for landscape generation and agent initialization, ensuring independent samples. Network structures are reinitialized for each replication.

3.8. Dependent Variables

The study carries five outcomes:

- I. **Best fitness achieved-** The highest fitness value reached by any agent during the simulation. This captures team's discovery capability (Did the team find a high-quality solution at any point?). The best fitness is divided by global optimum fitness for the landscape, controlling for between-landscape variance.
- II. **Final fitness-** The average team fitness at the final step (**200th**). This captures team's consolidation capability (Did the team retain and diffuse its best solution?)
- III. **Global optimum found-** A binary indicator of whether any agent's solution matched the global optimum configuration at any point during the simulation. This is the strictest measure of search success.
- IV. **Solution diversity-** The average pairwise Hamming distance among all agents' solutions at each step, averaged over the full simulation. This captures the degree to which agents maintain independent search trajectories versus converging towards a common solution.
- V. **Normalized fitness-** The best fitness is divided by the global optimum fitness for that landscape, used as robustness check. We treat raw best fitness as primary metric because normalization reduces between landscape variance only at low K and inflates

it at high K, where the global optimum fitness itself varies widely across landscapes. It also degenerates at $K = 0$, where every run finds the optimum.¹

3.9. Analysis

The study reports descriptive statistics (mean and standard deviation) for each condition across 200 runs per cell. To identify the best fitness, the author looked at the highest fitness achieved by any agent during a run with normalized metric (best fitness divided by the landscape's global optimum fitness) reported as robustness check. Further, the study treated the raw metric as primary because normalization reduces between landscape variances only at low K, inflates it at high K, and is degenerated at $K = 0$ (a stage where all runs find the optimum-section 3.8). Final fitness was analyzed as a secondary outcome. Global optimum discovery (a binary outcome with low base rates at high complexity) was treated descriptively, supplemented by a logistic regression on restructuring rules and network structure.

The inferential analysis proceeds in three steps. First, at each K-level, the one-way ANOVA was performed to examine whether all five conditions differ overall, followed by the omnibus test and Tukey HSD post-hoc comparisons among all pairs of rules. Second, each restructuring rule was compared against the static baseline at each level of K using two-sided independent sample t-tests. Multiple comparisons across this family of 24 planned tests were controlled with Holm correction. Third, because best fitness was bounded above and therefore not normally distributed in all conditions. The parametric assumptions were verified (Shapiro–Wilk tests of normality; Levene's tests of variance homogeneity) and every key results were confirmed with its non-parametric counterpart (Kruskal–Wallis for the omnibus tests; Mann–Whitney for pairwise comparisons).

Effect sizes were reported as Cohen's d for mean differences (negligible: $|d| < 0.2$; small: 0.2–0.5; medium: 0.5–0.8; large: > 0.8) and η^2 for ANOVAs. Statistical power at 200 runs per condition exceeds 97% for effects of $d = 0.40$ at $\alpha = 0.05$; 80% power corresponds to effects of $d \approx 0.28$.

¹ The simulation code (Python 3.12; NumPy; platform-independent) and all 6,000 random seeds are available at [anonymized repository link].

4. Results

4.1. Baseline Validation

Before examining restructuring, the author verified that the simulation replicates the *Lazer and Friedman (2007)* tradeoff. At $K=0$, all structures performed identically (100% optimum discovery). As K increased, sparse structures (rings) consistently outperformed dense structures

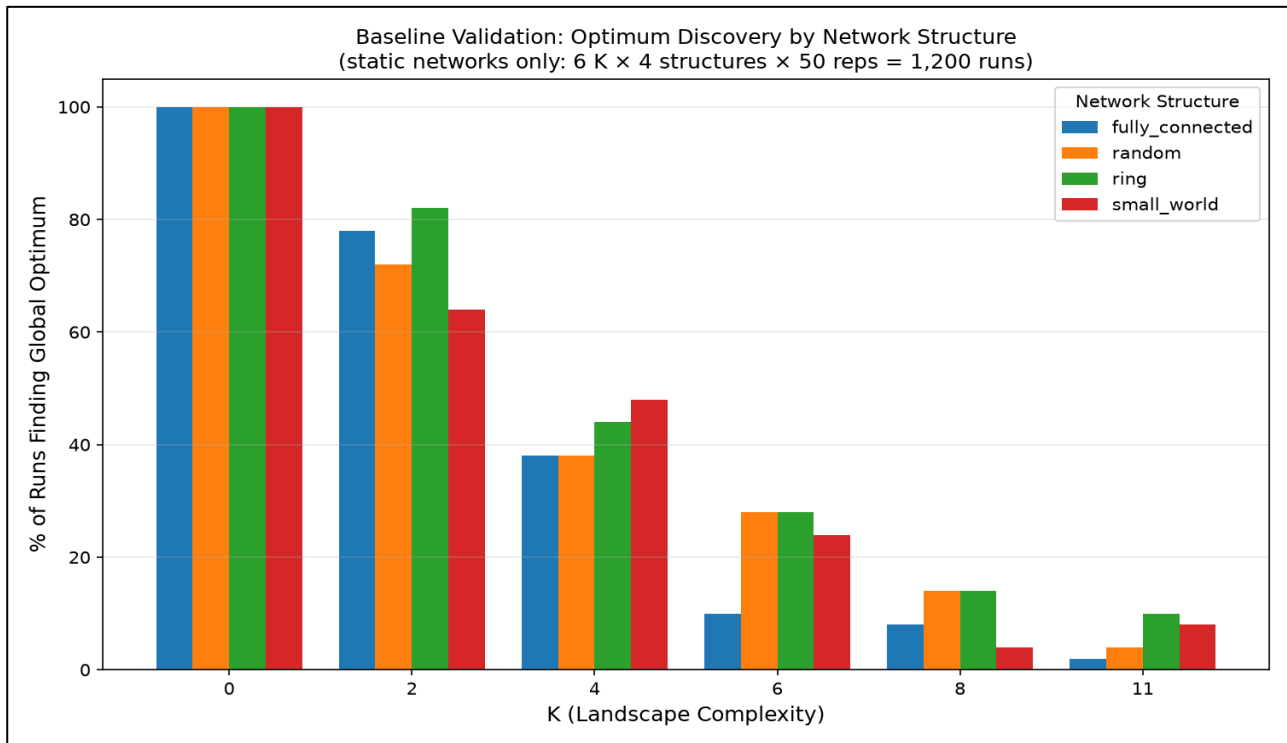


Figure 1. Global Optimum Discovery by Network Structure

(fully connected) (**Figure 1**). At $K=8$, ring networks found the optimum in 14% of runs versus 8% of fully connected. At $K=11$, the gap widened to 10% versus 2%. Small world networks occupied no consistent position, performing best at $K=4$ but worst at $K=2$ and $K=8$. This confirms that dense networks promote premature convergence while sparse networks preserve diversity but cannot diffuse solutions (foundational motivation of the study).

4.2. Main Effects

Table 3 presents the main results across all conditions, and **figure 2 to 4** provides visualization of the three outcome measures (best fitness, global optimum discovery, and final fitness) by restructuring rules and K . **Table 3.1 to 3.3** carry search performance by restructuring rules and landscape complexity ($n = 200$ runs per cell: 50 replications $\times$ 4 network structures).

Table 3.1. Best Fitness, M (SD)

K	Static	Passive Reset	Passive Dyad	Agent-initiated	Designer-initiated
0	0.661 (0.074)	0.666 (0.065)	0.669 (0.069)	0.662 (0.064)	0.666 (0.070)
2	0.742 (0.044)	0.743 (0.046)	0.744 (0.042)	0.743 (0.042)	0.746 (0.043)
4	0.757 (0.034)	0.755 (0.035)	0.759 (0.036)	0.756 (0.033)	0.756 (0.034)
6	0.750 (0.030)	0.749 (0.033)	0.752 (0.030)	0.753 (0.033)	0.754 (0.031)
8	0.743 (0.027)	0.743 (0.027)	0.741 (0.028)	0.742 (0.030)	0.746 (0.031)
11	0.734 (0.028)	0.735 (0.026)	0.737 (0.030)	0.736 (0.027)	0.743 (0.031)

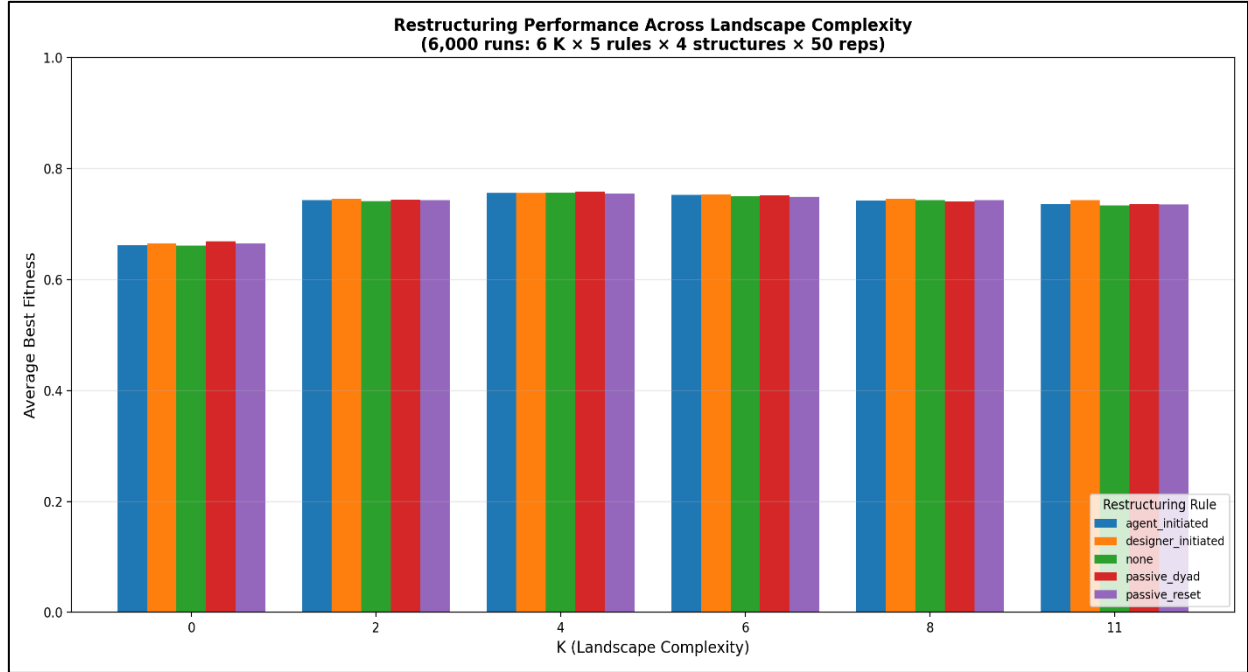


Figure 2. Average best fitness by restructuring rule across K

Table 3.2. Difference from static baseline on best fitness, Δ [95% CI] and Cohen's d

K	Passive reset	Passive dyad	Agent-initiated	Designer-initiated
0	+0.005 [-0.009, +0.018], d = 0.07	+0.008 [-0.006, +0.022], d = 0.11	+0.001 [-0.012, +0.015], d = 0.02	+0.005 [-0.009, +0.019], d = 0.07
2	+0.001 [-0.008, +0.010], d = 0.03	+0.002 [-0.006, +0.011], d = 0.05	+0.002 [-0.007, +0.010], d = 0.04	+0.004 [-0.004, +0.013], d = 0.10
4	-0.002 [-0.009, +0.004], d = -0.07	+0.001 [-0.006, +0.008], d = 0.04	-0.002 [-0.008, +0.005], d = -0.05	-0.002 [-0.008, +0.005], d = -0.05
6	-0.001 [-0.007, +0.005], d = -0.03	+0.001 [-0.004, +0.007], d = 0.05	+0.002 [-0.004, +0.009], d = 0.07	+0.003 [-0.003, +0.009], d = 0.11

8	+0.000 [-0.005, +0.005], d = 0.01	-0.002 [-0.008, +0.003], d = -0.08	-0.001 [-0.007, +0.005], d = -0.04	+0.003 [-0.003, +0.008], d = 0.09
11	+0.001 [-0.004, +0.006], d = 0.05	+0.003 [-0.003, +0.008], d = 0.09	+0.002 [-0.003, +0.007], d = 0.07	+0.009 [+0.003, +0.015], d = 0.31

Table 3.3. Global Optimum Discovery Rate (%)

K	Static	Passive reset	Passive dyad	Agent-initiated	Designer-initiated
0	100	100	100	100	100
2	74	74.5	77	70	76
4	42	49	37.5	41.5	45
6	22.5	15	19	23.5	23.5
8	10	12	9.5	13	13
11	6	5.5	6.5	7.5	9

(Note: Bold highlights the single comparison that is statistically significant (designer-initiated vs static at $K = 11$, $t(398) = 3.10$, $p = 0.02$, Holm adjusted $p = 0.050$). All other differences are non-significant ($p = 0.10$). Full descriptive statistics for all outcomes measures are in Appendix A.)

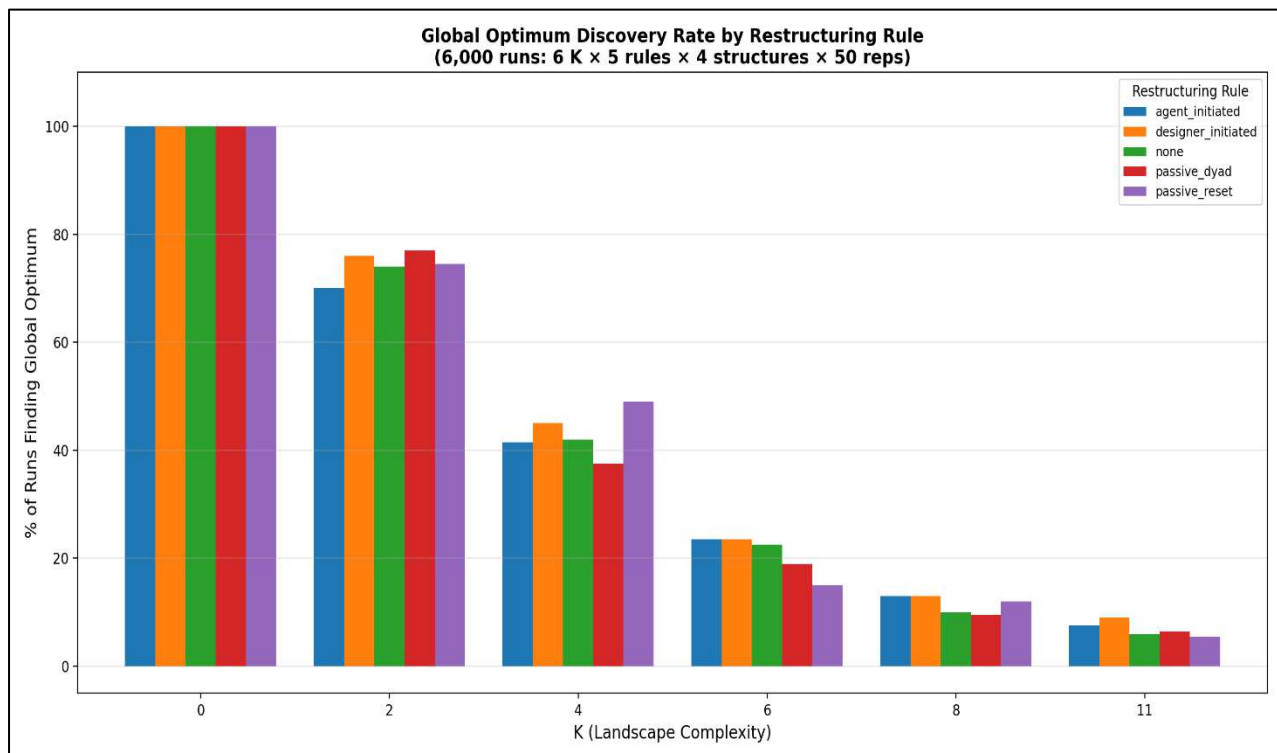


Figure 3. Grouped bars of % of runs finding the global optimum by rule across K.

Table 3.1 shows that mean best fitness followed an inverted U across complexity: it was lowest at $K = 0$ (0.661), where the optimum was bounded by the per-dimension maxima of independent contributions (expected value $2/3$ for $N = 12$ binary dimensions), rises to peak at $K = 4$ (0.757) as interdependence widens the attainable range, and then declines steadily as ruggedness makes superior configurations harder to reach. **Table 3.2** reports the differences on which all claims rest, showed that restructuring barely moved performance at any level of complexity. Only in designer-initiated restructuring at $K = 11$ (+0.009), whose confidence interval was the only one that excludes zero, every rule shifted best fitness by less than 0.01 at every level of K , and all these confidence intervals include zero. These null effects were also estimated with precision: at $K = 11$, the confidence intervals for the passive and agent-initiated rules extended only about 0.005 in either direction from zero. Therefore, any true effect of these rules at higher complexity is negligibly small rather than merely undetected. **Table 3.3** shows the discovery rate collapsing with complexity for every rule, from 100% at $K = 0$ to below 10% at $K = 11$, with rule differences remaining small at every level. The designer-initiated edged at $K = 11$ (9% vs 6%) are treated as descriptive, given the binary nature of outcome and low base rates at high complexity (*section 3.9*), and the occasional blips in individual cells (e.g., passive reset at $K = 4$) are examined in *section 5.6*.

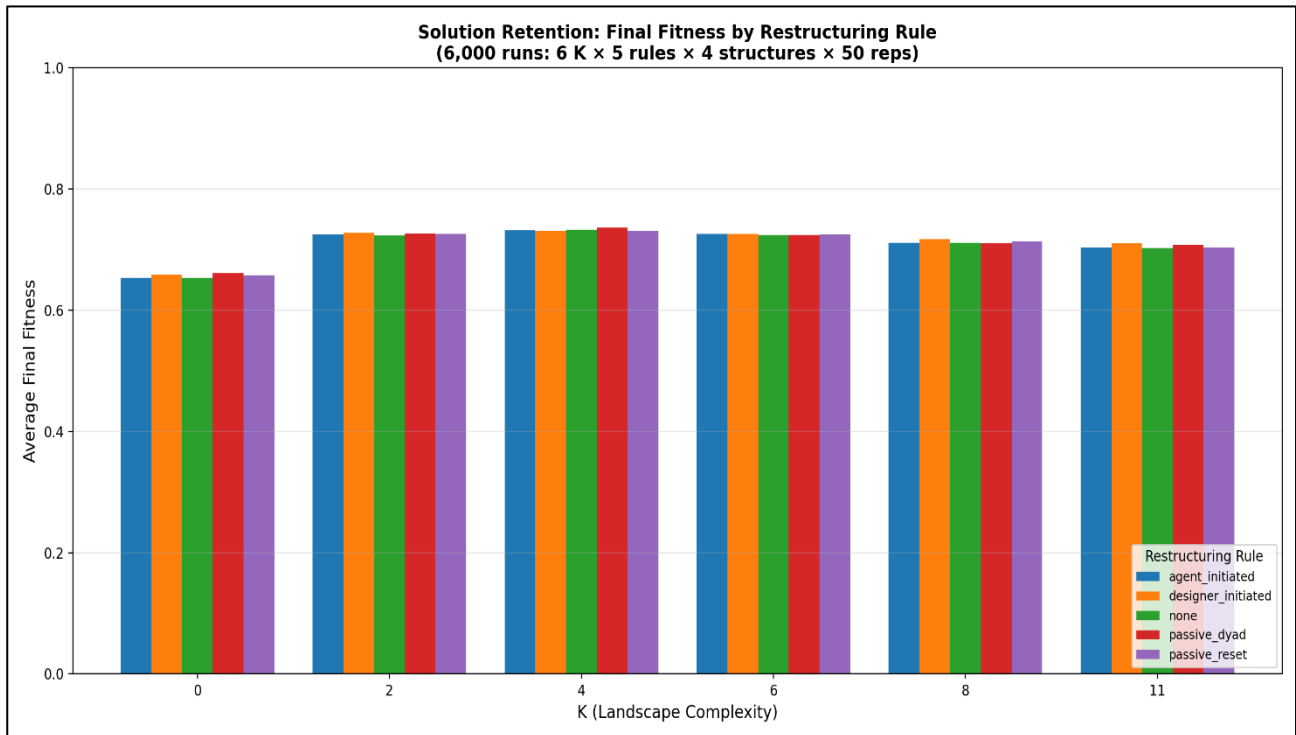


Figure 4. Grouped bars of average final fitness by rule across K

4.3. Statistical Analysis

A one-way ANOVA at each K level revealed significant differences among restructuring rules only at K= 11 $F(4, 995) = 3.11, p = 0.015, \eta^2 = 0.012$). At all other K levels, the rule explains virtually none of the variance in best fitness (all $F \leq 0.83$, all $p \geq 0.51, \eta^2 \leq 0.003$). Tukey post-hoc comparisons at K =11 indicated that the designer-initiated rule differs significantly from both static baseline ($p = 0.013$) and passive reset ($p = 0.045$), while no other pair of rules differs. A logistic regression of optimum discovery on rule and structure at K = 11 showed no significant rule effect (all $p \geq 0.26$), consistent with treating discovery rates effectively.

Table 4. One-way ANOVA of best fitness by restructuring rule, at each level of K (n = 1,000 runs per K level: 5 rules $\times$ 200 runs)

K	SS between	SS within	F (4, 995)	p	η^2
0	0.008	4.634	0.42	0.797	0.002
2	0.002	1.878	0.27	0.9	0.001
4	0.002	1.172	0.38	0.825	0.002
6	0.003	0.982	0.64	0.634	0.003
8	0.003	0.817	0.83	0.507	0.003
11	0.01	0.801	3.11	0.015	0.012

The best fitness found to be not normally distributed in all conditions. Shapiro-Wilk's test indicated 12 out of 30 rule-by-K cells did not meet normality condition. This particularly got noticed at K= 8 and 11, where the best fitness was bounded above and mildly right skewed (0.4-0.9). Nevertheless, parametric tests were retained for three reasons. *First*, the research questions concern mean performance, which the parametric framework quantifies through differences, confidence intervals, and effect sizes (Cohen's d, η^2) that rank-based tests do not provide. *Second*, the departures of 12 rule-by-k cells were modest and occurred alongside homogeneity of variance at every K (Levene's tests, all $p > 0.07$); with 200 runs per condition, the group means are approximately normal by central limit theorem. *Third*, every inferential conclusion was confirmed nonparametrically. Kruskal-Wallis's test analysis reproduced the ANOVA pattern (K = 11: $H(4) = 10.77, p = 0.029$; all other K: $p \geq 0.48$). Further, a key pairwise comparison was additionally confirmed by a Mann-Whitney test. The agreement between the two families of tests indicated no conclusion depends on the normality assumption.

Pairwise t-tests against the static baseline (**Table 3.2**) confirmed that only in designer-initiated restructuring at $K = 11$, produced a statistically significant improvement in best fitness ($t(398) = 3.10$, $p = 0.002$, $d = 0.31$). This result survived both Bonferroni and Holm correction across the full family of 24 planned comparisons ($p_{\text{adjusted}} = 0.050$). The advantage echoed in two secondary analyses. First, it persisted in final fitness ($t(398) = 2.42$, $p = 0.016$, $d = 0.24$). Second, in the Mann-Whitney test, the result found significant ($p = 0.034$). On the normalized metric the effect points in the same direction with a smaller effect size ($t(398) = 2.21$, $p = 0.028$, $d = 0.22$, uncorrected). The author treated the raw metric as primary because the normalization inflated variance at high K and degenerated at $K=0$ (section 3.8). No other rules reached significance at any K level on either metric.

Table. 5. Tukey HSD post-hoc comparisons among restructuring rules at $K = 11$ (all 10 pairwise comparisons; familywise $\alpha = 0.05$; $n = 200$ runs per rule)

Comparison	Mean difference	95% CI	p adjusted
Designer-initiated – Static	0.009	[+0.001, +0.017]	.013*
Designer-initiated – Passive reset	0.008	[+0.000, +0.016]	.045*
Designer-initiated – Agent-initiated	0.007	[−0.001, +0.015]	0.097
Designer-initiated – Passive dyad	0.006	[−0.001, +0.014]	0.155
Passive dyad – Static	0.003	[−0.005, +0.010]	0.888
Agent-initiated – Static	0.002	[−0.006, +0.010]	0.952
Passive dyad – Passive reset	0.001	[−0.006, +0.009]	0.988
Passive reset – Static	0.001	[−0.007, +0.009]	0.993
Agent-initiated – Passive reset	0.001	[−0.007, +0.007]	0.998
Agent-initiated – Passive dyad	−0.001	[−0.008, +0.007]	0.999

*Note: Differences are on raw best fitness; positive values favor the first-named rule. Rows are ordered by effect size. * Significant at familywise $\alpha = 0.05$.*

4.4. Structure-Level Analyses

To test whether the pooled effects conceal differences within specific network strictures, the study required repeating the rule versus static comparisons within every (K , structure cell)

which had 96 comparisons on the best fitness, 50 runs per cell. In this case, six comparisons stood significant at $p < 0.05$, against roughly five expected by chance at this level. However, none survived corrections for 96 tests ((smallest uncorrected $p = 0.0015$; adjusted $p = 0.14$). The six cells were scattered across rules, and mixed signs, and showed no coherent pattern. Only one cell- designer-initiated restructuring in random networks at $K = 11$ ($t(98) = 3.05$, $p = 0.003$, $d = 0.61$, a medium-to-large effect) was significant on both raw and normalized metrics and consistent in direction with pooled $K = 11$ effect.

Examining the designer-initiated rule at $K = 11$ within each initial structure reveals a systematic gradient. In random networks, the improvement was significant ($t(98) = 3.05$, $p = 0.003$, $d = 0.61$; mean best fitness 0.732 vs. 0.750). In fully connected network, it was marginal ($t(98) = 1.96$, $p = 0.053$, $d = 0.39$). In the small world network, it was small and non-significant ($t(98) = 1.03$, $p = 0.31$, $d = 0.21$). Finally, in ring network, the improvement was negligible ($t(98) = 0.29$, $p = 0.77$, $d = 0.06$). The ordering tracks how much independent exploration the initial structure had already affords. Ring network, that preserves the most diversity, performed the best under static conditions (0.743, the highest of the four structures) gained nothing from the designer, dense, and unstructured networks that converged earliest with most room gain (fully connected: 0.723, the lowest). With only 50 runs per structure, these within-structure tests are powered to detect only large effects, so the pooled analysis remained primary. The gradient was reported as a consistent descriptive pattern.

A two-way ANOVA at $K = 11$ confirmed significant main effects of restructuring rule ($F(4, 980) = 3.17$, $p = 0.013$) and network structure ($F(3, 980) = 4.26$, $p = 0.005$) with a marginal interaction ($F(12, 980) = 1.74$, $p = 0.054$). The structure's main effect showed the baseline trade-off operating within the most complex landscapes. Rich networks achieved the highest best fitness pooled across rules (0.741). The marginal interaction echoed the gradient where restructuring helped depends partly on the structure it modifies. Notably, under designer-initiated rule, the four structure converge towards similar performance (0.733–0.750), suggesting that the phase-based intervention substitutes for the diversity that structure alone would otherwise have to provide.

4.5. Diversity as a Mediating Mechanism

Designer-initiated restructuring maintained the highest average solution diversity at every K level from K= 4 onward (**Figure 5**). At K = 11, diversity under designer rule is 0.655 against

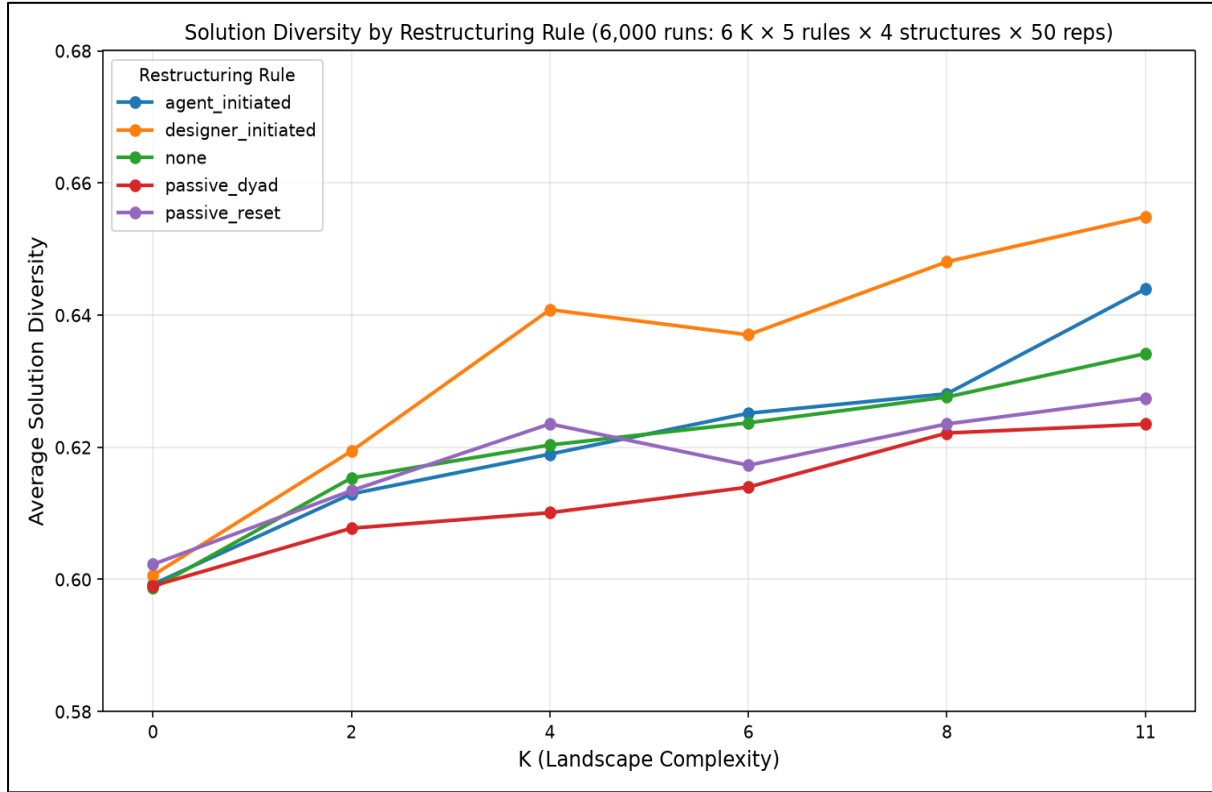


Figure 5. Solution Diversity Across K-level

0.634 for the static baseline which is positive and significant ($t(398) = 1.83$, $p = 0.068$, $d = 0.18$). Passive rules did not raise the diversity at all as both sat below the static baseline at K= 11 (passive reset 0.627, passive dyad 0.623). Random structural churn disrupted diffusion channels without creating the independent search trajectories that exploration requires. Agent-initiated restructuring raised diversity modestly (0.644) without any corresponding fitness gain. In this case, preferential attachment to current top-performers changes who talks to whom, but not where agent search. The designer rule was the only condition that combines elevated diversity with targeted reconnection of isolated agents to high performers. Additionally, designer-initiated rule was the only condition found to have significant effect. Therefore, diversity alone was thus insufficient because the pairing of diversity with directed diffusion was what plausibly drives the advantage. In future research, with the input of per-step trajectory data, the mechanism of diversity would be clearer.

4.6. Non-Significant Patterns

Several descriptive patterns in the data did not reach statistical significance but were worth noting as suggestive directions for future research. Passive reset showed the highest discovery rate at $K=4$ (49% vs. 42% static) but the difference is non-significant ($t(398) = 1.41, p = 0.16$). Agent-initiated restructuring showed the lowest discovery at $K=2$ (70% vs. 74% static) but again non-significant ($t(398) = -0.89, p = 0.37$). The designer-initiated rule itself was not uniformly ahead of baseline. Its differences at $K=4$ and 6 are negative ($\Delta = -0.002$ at both, both $p > 0.60$), indicating that its advantage emerges only at the highest complexity rather than growing monotonically with K . These patterns may indicate real differences but presently have small effects. With larger sample size or paired design, the results may come differently.

5. Discussion

5.1. Summary of Findings

The study resulted in four key findings. *First*, the static-network baselines replicate the canonical *Lazer and Friedman (2007)* tradeoff. Sparse structures discovered superior solutions more often while dense structures diffuse them faster, and no static structure was optimal at every task. *Second*, restructuring the network randomly or by decentralized agent initiative leaves search performance statistically unchanged at every level of complexity. The null effects were tightly bounded rather than unpowered. *Third*, designer-initiated phase-based restructuring improves best fitness, but only at the highest complexity ($K = 11: \Delta = +0.009, d = 0.31$). The advantage persisted in final fitness ($d = 0.24$), survived non-parametric confirmation, and separated the designer rule not only from static baseline but also from passive reset under family-corrected post-hoc comparisons. *Fourth*, the designer-initiated advantage concentrates in random and fully connected initial structures, was absent where initial structure already preserves diversity (ring), is accompanied by the highest maintained solution diversity from mid-range complexity onward.

5.2. Structural Change Carries Information: Confirmations and Contrasts with Prior Work

The study's first contribution to prior work is confirmation to existing works. The clean replication of *Lazer and Friedman (2007)* trade-off across all six complexity levels establishes that the study framework produced the static network logic on which the collective-search literature is built. Its subsequent results reflect the restructuring manipulation rather than

implementation artifacts. Replication of a canonical result with an extended instrument is itself informative in literature where computational implementations vary widely (*Baumann et al., 2018*).

The static trade-off also survives, with a caveat, in behavior field evidence. *Brackbill and Centola (2020)* experimental study suggested group holding inefficient networks consistently outperform those holding efficient ones (section 2.3) has its algorithmic counterpart in ring-versus- fully connected baseline, and together the two bound the *Mason and Watts (2012)* reversal discussed below. The comparison adds a distinction that the study results then sharpen. In their design, as in all experimental work in this stream, the network is inefficient for the duration of the task. In this study, the passive rules showed that transiently breaking ties did not capture the benefits of standing sparseness. It could possibly be due to the value of inefficiency lies in the structure an organization keeps, not in act of severing what it has.

The designer rule also had a theoretical ancestor whose logic it temporalizes where the complementarity between search-encouraging and stability-providing design elements identified by *Rivkin and Siggelkow (2003)* (section 2.2). The exploration phase created the diversity whose value the exploitation phase then harvests by reconnecting isolated agents to top performers. Each phase raised the value of the next. Random change severed the complementarity because its timing was unsynchronized with anything the search needs, and agent-initiated change collapses it by attaching everyone to incumbents.

The closest prior result to this study was of *Siggelkow and Levinthal's (2003)* "temporarily divide to conquer" findings (section 2.4). The study findings confirmed and quantified its parameters. The designer rule formalized the 'divide and integrate' logic, and it is the only rule that helped in the study experiment at the complexity level where interdependence is maximal. The confirmation, however, came with a sharper boundary than *Siggelkow and Levinthal's (2003)* study. Across the full complexity range and under inferential testing, the benefit was confined to the most extreme landscape of the study. At moderate complexity ($K = 2$ to 8), temporary division and reintegration was indistinguishable from leaving the network alone. Their design compared structural forms under one change logic. This study compared logic for generating change, and the comparison showed that celebrated benefits were real but narrow. Further, one thing should be noted that in this study, the landscapes were static corresponding to the slow-change extreme of complexity-by-turbulence space *Siggelkow and Rivkin (2005)*.

This study also witnessed two contrasting results from existing literature that are highly valuable. First, *Wall (2015)* reported that induced organizational dynamics, including randomized change, can raise effectiveness of distributed adaptive search (section 2.4). In this study, the passive rules produced precisely bounded null effects at every complexity level. The contrast is likely informative rather than contradictory. Wall's interventions altered coordination mechanisms of searching units. In his work, the benefit varied with mode, frequency, and scope of change. The study passive rules uniformly rewire communication tied among myopic hill-climbing agents on a fixed schedule ($T=20$), a single point in that larger parameter space. This study results suggested that the benefit of exogeneous change did not extend to unscheduled rewiring of who observes whom the specific intervention most analogues to involuntary reorganization in organizations. *Songhori and García-Díaz (2018)* found that endogenous ties evolution can improve collective problem-solving when agent learn about their ties over time (section 2.4). In this study, the agent-initiated rule, in which agents myopically attach top performers, yields no benefit at any complexity. Together the two contrasts imply that structural change helps only when it carries information either learned (which ties are worth keeping) or designed (which phases the search needs), not when it is the noise.

Finally, the study results are directed at the boundary between computational and behavioral evidence contested ground. *Mason and Watts (2011)* identified the computational prediction reversed while *Brackbill and Centola (2020)* noticed it to be upheld in field condition (section 2.3). In this study, the study agents were algorithmic that follow *Lazer and Friedman (2007)* dynamics, allowing for a precise baseline prediction. However, the designer-initiated advantage documented in the study findings may not hold true when subjected to human behavioral biases such as conformity and status-seeking. Consequently, behavior validation remains an open question and represents the next logical phase of this research.

The pattern that unifies these comparisons is complexity as recurring moderator. *Heydari et al.'s (2025)* core-peripheral allocation reverses between smooth and rugged landscapes (section 2.3). The study finding suggested that the value of restructuring flips from null at moderate complexity to positive at the extreme is a dynamic-structure instance of the same principle. Optimal design reverses with the ruggedness of the problem, and any general claim about restructuring that ignores complexity is incomplete. Where the individual-level experimental stream located adaptation in search behavior (*Billinger et al., 2014*). The study framework relocated it to the network itself, comparing adaptation rules inside a single instrument. This is

the contribution the study offers to the emerging collective adaptation agenda (*Galesic et al., 2023*). Besides that, collectives can restructure their networks during search, but that how they do it (with or without information) determines whether it matters.

5.3. Why Most Restructuring Fails: A Diversity-Diffusion Account

The null results in this study were not merely absent effects. Rather, diversity data suggests a mechanism for them. Search on rugged landscapes requires two things that pull in opposite directions. Independent exploration (agents sampling different regions) and efficient diffusion (superior solutions spreading). Static structures trade these off, also captured exactly *Lazer and Friedman's (2007)* findings. The current study findings indicate that most ways of restructuring the network fail because they improve either term of trade off. The passive rules did not even raise diversity even at static baseline of $K = 11$. Therefore, random churn forfeits the diffusion value of established ties without purchasing any compensating exploration. The agent-initiated rule raised diversity modestly but attached agents to current incumbents, changing who communicates without changing where the team searches. Herding toward early winners is a known failure mode of decentralized adaptation. Although the simulated $K = 2$ discovery pattern (70% vs. 74%) did not reach statistical significance, it remained conceptually consistent with this tendency.

The designer rule succeeded at the one complexity level where it serves both terms of the trade off in sequence rather than simultaneously. The exploration phase deliberately breaks dense clusters (creating diversity), and the exploitation phase reconnects isolated agents to top performers (creating directed diffusion). This temporal structural ambidexterity (*Brown & Eisenhardt, 1997; Nickerson & Zenger, 2002*) deals with selection between a structure suited to exploration and one suited to exploitation, the organization alternates between them under central timing. The advantage only appeared at $K = 11$, where the landscape offered approximately 315 local optima and unaided teams were almost certainly trapped. This indicates that the sequencing pays only when the exploration phase has genuine work to do, or when the team would otherwise be stuck.

6. Conclusion

The study asked whether organizations could improve search performance by restructuring their influence networks during search, if so, when and by what rule. By evaluating a static baseline against four restructuring rules across six complexity levels and four network

structures, the 6000 runs delivered definitive and sobering insights. *First*, it showed that most restructuring is indistinguishable from leaving the network alone, with effects bounded close to zero. *Second*, the exception is identified with central designer alternating between dividing the network for exploration and reintegrating it for exploitation witnessed improved search. However, this got materialized only at the highest complexity where landscapes reliably trapped unaided teams.

Restructuring, in this study results indicate, is a contingency rather than a general design lever. Its value emerged only at the highest complexity, consistent with evidence that optimal organization reverses with problem ruggedness, and the only rule that helped was the ones whose changes were informative (dividing the network structure when exploration needed and reconnecting it when diffusion needed). Serving exploration and diffusion in sequence, rather than setting on a single structure that compromises both, is the designer rule's structural counterpart to temporal ambidexterity. For practice, the results support stability as the default at low and moderate interdependence, a diagnosis of complexity before reorganizing, and centrally timed alternation rather than churn. The findings are complementing, not replacing structures that already preserve diversity.

These conclusions are held within stated boundaries (algorithmic agents, a single parameterization, costless change, and static landscapes) and the null results bound rather than exclude small effects. Behavioral validation, trajectory-level mechanism data, and turbulent or costly settings mark where the calculus may differ, and the framework itself remains a reusable instrument for asking such questions under controlled complexity. Organizations will keep organizing. The study results add discipline that asks how interdependent the problem is before changing who talks to whom and make changes carry information about where the search should go next.

7. Implications for Organizational Design

The study proposes three implications. *First*, for the problems of low to moderate interdependence, the default should be stability as none of the four restructuring logics improved any outcome at $K=2-8$. The common managerial practice of periodic reorganizations appears, on this evidence, to be wasted disruption-consistent with empirical finding that organizational restructuring has the weakest and the least consistent performance impact among restructuring types (Bowman et al., 1999). *Second*, when problems are highly interdependent, restructuring helps only if it is designed. Second, when the problems are highly

interdependent, restructuring can help but only when it is designed. The one rule that worked has a central actor alternating on purpose who split teams into smaller groups so that members search independently for a while and then reconnect isolated members to the best performers. This way good solutions get spread. The rules without this design (random resets or employee choosing their own contacts) produced nothing, even at the highest complexity. The inference to draw here is that change helps when it follows a plan, not when it happens for its own sake. Third, one should look at the structure already existing before restructuring. The designer's benefit was largest when the starting network was dense or unstructured, and vanished when the starting network was already sparse which already giving members the independence the designer would otherwise create. Restructuring can add to such a design but cannot replace it. One cautionary note to the interventions is that organizations already running small and loosely connected subunits may not gain substantially as identified in this study.

8. Limitations

A set of five limitations qualify these conclusions. *First*, the agents in this study were algorithmic hill-climbers with a fixed exploration probability. Human search behavior may not follow such systematic assumption (Mason & Watts, 2012). Further, designer-initiated advantage awaits behavioural validation. *Second*, a non-significant comparison does not prove that a restructuring rule is as good as the static baseline. It only means no differences detected. Formally concluding 'no effect' would require an equivalence test showing that the true difference is smaller than a negligible margin ($d = 0.2$), and at the current sample size, that test remains inconclusive. Here the data did establish is narrower but useful. Confidence intervals around passive rules' differences were so tight that any effect they might have confined to magnitudes too small to matter in practice. *Third*, parametrization is single point: exploration probability (0.15), restructuring interval ($T = 20$), designer phase length ($L = 50$), team size (20 agents), and run length (200 steps) are each fixed, and sensitivity analysis needed to establish robustness across these values remains to be run. *Fourth*, reorganizing network structures in this study model costs nothing (no time, disruption, or lost productivity) which allows restructuring its best chance. The four rules still produced no benefit under these favourable conditions makes null conclusion conservative rather than fragile. The one positive results points the other way is the designer-initiated advantage at $K = 11$. It was measured in absence of costs, so its real-world net benefit might be smaller than reported, and restructuring would need to be valuable still before it paid for itself. *Fifth*, the landscape is static and the team faces

a single, unchanging problem, restructuring may be more valuable when the environment shifts benefit the search. Fifth, the landscape is static and the team faces a single unchanging problem. Restructuring may be more valuable when the environment shifts beneath search.

9. Future Scope

Each open question maps to next step. The proposed mechanism that includes diversity created in exploration phases, solutions, diffusing in exploitation phases, was inferred from run-level summaries. However, retaining per-step trajectories in the framework as built would make it observable directly. The algorithmic-agent boundary points to a behavioural experiment where the same five conditions would be evaluated with human teams would give answers clearer whether designer-centric advantage survives conformity and status biases that have reversed comparable computational predictions (*Mason & Watts, 2012*). The two contrasts that this study could not resolve. Two of the earlier studies did not match ours. *Wall (2015)* found that induced organizational change can improve distributed search, while this study (passive rules) produced no benefits. Researchers can look upon in future works by extending how often, how extensively, and in what way network is changed. This may reveal whether null results hold across that larger design space. *Songhori and García-Díaz (2018)* found that agents' own tie changes improve collective search when agents to current top performers, produced none. Adding learned, aspiration-driven tie changes to that rule would test whether learning is the missing ingredient. Both follow-ups would put the study's central proposition to direct test (structural change helps in when it carries information). Restructuring costs, environmental turbulence, and larger or heterogeneous teams remain unmapped boundary conditions that can be incorporated in subsequent works. The most immediate steps are methodological that includes but not limited to common random numbers, pairing rules on identical landscapes to sharpen power for small effects, and the sensitivity analysis over the fixed parameters such as exploration probability, restructuring interval, and phase length.

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Appendix A: Full Descriptive Statistics

Table A1. Descriptive statistics for all outcome measures by condition (n = 200 runs per cell)

K	Restructuring rule	Best fitness M	Best fitness SD	Final fitness M	Final fitness SD	Optimum found (%)	Diversity M	Diversity SD
0	Static	0.661	0.074	0.654	0.073	100.0	0.599	0.033
0	Passive reset	0.666	0.065	0.657	0.064	100.0	0.602	0.031
0	Passive dyad	0.669	0.069	0.661	0.068	100.0	0.599	0.031
0	Agent-initiated	0.662	0.064	0.653	0.064	100.0	0.599	0.029
0	Designer-initiated	0.666	0.070	0.658	0.069	100.0	0.601	0.031
2	Static	0.742	0.044	0.724	0.043	74.0	0.615	0.070
2	Passive reset	0.743	0.046	0.726	0.046	74.5	0.613	0.040
2	Passive dyad	0.744	0.042	0.726	0.042	77.0	0.608	0.035
2	Agent-initiated	0.743	0.042	0.725	0.042	70.0	0.613	0.053
2	Designer-initiated	0.746	0.043	0.728	0.043	76.0	0.619	0.042
4	Static	0.757	0.034	0.733	0.034	42.0	0.620	0.062
4	Passive reset	0.755	0.035	0.731	0.035	49.0	0.624	0.039
4	Passive dyad	0.759	0.036	0.736	0.036	37.5	0.610	0.040
4	Agent-initiated	0.756	0.033	0.732	0.035	41.5	0.619	0.042
4	Designer-initiated	0.756	0.034	0.731	0.037	45.0	0.641	0.157
6	Static	0.750	0.030	0.724	0.033	22.5	0.624	0.061
6	Passive reset	0.749	0.033	0.725	0.036	15.0	0.617	0.041
6	Passive dyad	0.752	0.030	0.725	0.033	19.0	0.614	0.038
6	Agent-initiated	0.753	0.033	0.726	0.035	23.5	0.625	0.074
6	Designer-initiated	0.754	0.031	0.726	0.032	23.5	0.637	0.066
8	Static	0.743	0.027	0.712	0.031	10.0	0.628	0.051
8	Passive reset	0.743	0.027	0.714	0.030	12.0	0.624	0.039
8	Passive dyad	0.741	0.028	0.710	0.031	9.5	0.622	0.043
8	Agent-initiated	0.742	0.030	0.712	0.031	13.0	0.628	0.095
8	Designer-initiated	0.746	0.031	0.717	0.035	13.0	0.648	0.088
11	Static	0.734	0.028	0.703	0.031	6.0	0.634	0.065
11	Passive reset	0.735	0.026	0.704	0.031	5.5	0.627	0.039

11	Passive dyad	0.737	0.030	0.707	0.029	6.5	0.623	0.042
11	Agent-initiated	0.736	0.027	0.703	0.030	7.5	0.644	0.105
11	Designer-initiated	0.743	0.031	0.711	0.033	9.0	0.655	0.146

***Note:** Best and final fitness are raw values on the [0, 1] landscape scale. Diversity is the average pairwise Hamming distance among agents' solutions, averaged over the 200 steps. Final fitness is the team mean at step 200. Optimum found (%) is the share of runs in which any agent matched the global optimum. Source: full 6,000-run experiment (6 K × 5 restructuring rules × 4 network structures × 50 replications); all values computed from the run-level data on the 'Source data' sheet available as a supplementary file.*

Supplementary Files

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