PUBLISHING INSTITUTE
Journal Website
Article history:
Received 06 January 2026
Revised 03 April 2026
Accepted 19 April 2026
Initial Publication 10 May 2026
Final Publication 01 June 2026

Psychological Research in Individuals with Exceptional Needs

OPEN PEER-REVIEW REPORT

E-ISSN: 3060-6713

The Mediating Role of Self-Efficacy and Social Support in the Effect of Psychological Hardiness on the Quality of Life of Physically Disabled People

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the opening paragraph states that “Disability is considered one of the major public health and social challenges worldwide, affecting millions of individuals across different age groups and cultural contexts.” This statement is too general for a scientific introduction and should be strengthened with current epidemiological data, preferably from WHO, national welfare statistics, or recent Iranian disability reports. The authors should specify the global and Iranian prevalence of physical/motor disability and explain why Mazandaran Province represents an important context for this investigation.

The Introduction needs a stronger theoretical foundation for the proposed mediation model. The manuscript discusses psychological hardiness, self-efficacy, social support, and quality of life separately, but it does not sufficiently explain why self-efficacy and social support should mediate the hardiness–quality of life relationship. The authors should explicitly integrate hardiness theory, Bandura’s social cognitive theory, and the stress-buffering model of social support to justify the causal ordering assumed in the SEM.

The descriptive statistics in Table 1 appear implausible and require substantial revision. Psychological hardiness is measured by a 27-item scale scored from 0 to 3, so the total score should range from 0 to 81, yet Table 1 reports M = 4.05 and SD = 2.88. Similarly, self-efficacy is a 17-item Likert scale, but the reported mean is 4.11 with SD = 2.97. The authors must clarify whether these are item-level means, total scores, transformed scores, or incorrectly reported values. If item-level means are used, the SD values appear too large and likely impossible for the scale range.

Table 2 reports extremely high correlations, especially between psychological hardiness and quality of life, r = .853. Such a high correlation between theoretically distinct constructs raises concerns about common-method variance, item overlap, response bias, or data-entry error. The authors should test for multicollinearity, report variance inflation factors, consider Harman's single-factor test or a common latent factor approach, and discuss whether the magnitude of this correlation threatens discriminant validity.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

In the paragraph beginning "Despite the growing body of research concerning disability and quality of life, several important gaps remain in the literature," the gap statement is relevant but still too broad. The authors should identify exactly what is novel about their model compared with previous studies: whether the novelty is the population, the simultaneous testing of two mediators, the Iranian provincial context, or the use of SEM. Without a more precise gap statement, the study risks appearing as a simple replication of known associations among positive psychological variables.

The Methods section states that "the total number of individuals with disabilities covered by welfare services in Mazandaran Province exceeds 68,000," but then states that the study population was "approximately 26,000 individuals" with physical disabilities. This distinction is important but underdeveloped. The authors should clarify whether the 26,000 figure refers only to physical/motor disabilities, whether sensory disabilities were included or excluded, and how participants' disability type and severity were verified from welfare records.

The sampling description requires more methodological detail. The manuscript states that Mazandaran Province was divided into "north, south, east, and west" and that Sari, Amol, Behshahr, and Chalus were selected. However, the selection procedure is not sufficiently described: were cities randomly selected, purposively selected, or selected based on welfare office accessibility? The authors should report the number of eligible individuals in each selected city, the proportional allocation method, recruitment procedures, response rate, and reasons for nonparticipation.

The inclusion and exclusion criteria are insufficiently reported. The manuscript only states that participants were aged 14–40 years and had welfare records, but it does not clarify cognitive ability to complete questionnaires, literacy requirements, severity of disability, comorbid psychiatric conditions, neurological disorders, or concurrent rehabilitation/psychological treatment. Because the sample includes adolescents as young as 14, the authors should also specify parental consent, adolescent assent, and whether questionnaires were developmentally appropriate for younger participants.

The Measures section contains an important error regarding the SF-36. The manuscript states that the "36-item Short Form Health Survey (SF-36)" was "developed by the World Health Organization," which is inaccurate. The SF-36 is associated with the Medical Outcomes Study and Ware and colleagues, not WHO. The authors should correct the origin of the instrument, provide appropriate citations, and report whether the validated Persian version was used.

The description of the quality-of-life instrument contains another conceptual problem: the manuscript states that "Previous findings supported the construct validity of all dimensions of the instrument except the spiritual dimension." The SF-36 does not include a spiritual dimension. This suggests that the authors may have confused SF-36 with another quality-of-life instrument. The authors should revise this section carefully and ensure that all dimensions, scoring procedures, and validity evidence correspond specifically to the SF-36.

Authors revised the manuscript and uploaded the document.

E-ISSN: 3060-6713

2. Revised

Editor's decision: Accepted.
Editor in Chief's decision: Accepted.

E-ISSN: 3060-6713