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Review of the PhD thesis " The Influence of a Parent Intervention Program on the Patterns of Sleep and Consumption of Media of Their Adolescent Children in the Era of Modernization in Israeli Arab Society" by Sundus Abu Ghaneem, supervised by dr hab. Monika Frąckowiak-Sochańska.

The reviewed thesis exemplifies applied social sciences well, as it combines a theoretical framework, practical implications, and an interdisciplinary approach. It explores an unquestionably important subject from a variety of perspectives: sociological, psychological, and public health studies, but would also fit a shelf with educational and evaluation studies. As an educational researcher and a father of three teenagers, I have no doubts that adolescents' sleeping patterns are an important and underexplored subject and that detriment in the quality of sleep, caused by many factors, can seriously impact public health. The second element of the dyad shown in the title – "media consumption" – assumes one of these factors.

The thesis portrays a society in transition. Although the Author does not map the modernization process in detail, it is clear from the copy that global and local processes of change result in changes in the structure of Israeli Arab social groups, including familial roles and relationships and individual social practices. The post-positivist perspective adopted by the Author is, in my humble opinion, not the best framework to explore societal and cultural change, as it tends to focus on social homeostasis and show change as a challenge to the homeostasis, but it perfectly fits the evaluation project, which is the empirical basis of the thesis.

The thesis is divided into two parts. The first part provides a literature review, and the second – presents and discusses the results. The literature review consists of four chapters, describing a general landscape, the Internet and the implications of its proliferation, adolescents' sleeping patterns and their correlates, and the role of parental involvement in adolescents' health. The second part was divided into three chapters. The first one describes the methodology, next two – subsequent stages of the research. The overall structure of the thesis is logical and clear from the reader point of view.

Theoretical framework and literature review

The literature review provides strong evidence of the Author's knowledge of the relevant literature. Starting from a general consideration of "big" sociological terms like globalization and modernization, she describes more specific implications of societal and technological changes. The overall references to sociological literature are satisfactory but not deepened. The Author referred to general sociological literature, quoting authors from different times and paradigms (from Merton through Habermass to Giddens and Bauman). It is a nice introduction, but not useful for interpretation (at least not appearing in the interpretation). Middle-range theories would probably bring more to the data interpretation. Some interpretations are disputable. For example, I don't remember Lyotard writing about "new" media, whatever it would mean in the 1970s (p.24). Baudrillard's paragraph does not contain any reference notes.

I am not sure whether all the sections are necessary. For example, what is the use of a two-page analysis of the role of transnational corporations (pp.12-14)? The second remark is the consistency of the narrative. Especially in the first chapter, I had an impression of reading partially lousy notes and missing connections between the paragraphs (see pp. 23-24 or 26-27 for example). It was notably more coherent in subsequent chapters.

I also missed a discussion of the research reviewed in the first part of the thesis. For example, the Author describes research on attitudes toward women's roles (p.21). I assume that it was intended to work as evidence of the conservatism of the rural Arab population, but there is still no discussion of its relevance. To what extent can young adults from Riyadh (funny coincidence: I am writing these words while being there) work as a reference point for adolescents from Israeli villages? The same goes for the quite old (1972) research on the influence of menopause on Arab and Israeli women (p.22). What is the link with the youths of today?

The subsequent parts, starting from the subchapter "The process of modernization in Arab society" are more coherent and comprehensive. The subchapter provides a good picture of the population in study, even if some references are quite old for the description of the contemporary societal changes. The 50-year-old studies (p. 33) should be clearly marked as portraits of their times. Some of them show intriguing similarities with old sociological theory. For example, Al-Haj 1983 study (p. 34) resembles the Parsonian idea of the gender-based distribution of instrumental and emotional roles in a nuclear family. Also, a portrait of the US society of Talcott Parsons's times is questionable in different times and contexts.

Chapter two, describing the role of the Internet, is the longest section of the thesis and shows the Author's good knowledge in the literature. The only things I missed were the discussion of quoted research reliability and relevancy to Arab youths in Israel and the use of systemic reviews and meta-analyses. The latest two are easily accessible and provide strong evidence supporting the Author's

statements, including neuroscience. Some of the studies quoted in this chapter are correlational studies, which do not show causality. A good example can be found on page 52, where the Author repeats the claim about the influence of internet use on ADHD occurrence. Some information is irrelevant, such as a paragraph on creativity in the health-related subchapter (p. 57), and some are unnecessary (sections on sexual harassment and bullying are not used in the research, pp. 72 and following). There are also sections that should be rearranged, particularly in the "Causes of addition" subchapter, where information about consequences and symptoms is included. The review of research on MPA (p. 97 and following) should be better organized. I am also not sure what the difference is between "reasons" and "causes" in the title of the section on page 102.

By the way, there are some signs of the Author's moral attitude toward the new media use by adolescents, for example, when she claims that "too many users do not understand the implications" (p. 44), "it is impossible to ignore the worrisome fact" (p. 48), "people become lazier" (p. 57) or when she recommends a "must-have" reactions (p. 49). The clear example of moralization can be found on page 56. "They [youths] do not remember the feel of the page between their fingers; they do not know the meaning of the development of the imagination and the enrichment of the knowledge entailed by the world of books". Independent from my personal feeling on the change in youths of today, I think that every valuing statement should be carefully considered. The Author extensively reviews research showing a negative influence of the Internet and new media but barely indicates opposite results (p. 63 and 106). The statement that children "generally" acquire ICT skills before their parents (p. 63) is surprising in the 2020s. It may be true regarding specific communities, including the one studied by the Author, but definitely not the general pattern. Consider another example: "When the young people have an entire world on their computer screen within the four walls of their home, there is no motivation to go outside and to search for concrete friendly relationships, since on the Internet they know new people and create worlds of their own where it is good and safe for them" (p. 62). The wider sociological perspective is missing here. Some of the changes are due to societal and lifestyle changes and should be analyzed with specific context and include a perspective of the actors involved. Simplistic judgments from the point of view of other generations belong to journalism, not to science. The Author also misses the complexity of changes, attributing the decrease in parental authority to the Internet only (p. 65).

Chapters three and four are some of my favorite parts of the thesis. They are well-written, and the works quoted are relevant. Definitions important for further research, particularly of the sleep patterns and parenting style, plus the research hypotheses, are presented here. From the sociological perspective, I would love to read more about Author's thoughts on the influence of culture in sleep patterns and a more elaborate analysis of Durkheim and Merton's works (p. 136). I must also note two pages on the parenting styles without any reference (pp. 133-134).

Research desing

The research project combines a cross-sectional survey among two ethnic groups and a summative evaluation of the intervention in the Arab population. I think that the evaluation label is not an exaggeration, as the Author uses an evaluation procedure and refers to evaluation theory (Mertens, Patton, Guba and Lincon). Clearly, it is not a mixed design (p. 153), as the qualitative component is limited to an analysis of open-ended questions from the survey. The evaluated program has a strong theoretical and empirical rationale. As an evaluation practitioner and theorist, I thoroughly enjoy combining evaluation with scientific research, even if we are not exactly on the same page with the Author regarding the evaluation "paradigms". However, the relation between a cross-ethnic survey and an intervention evaluation is not clear to me. What exactly does the comparison between Jewish and Arab adolescents bring there? Are the Jewish youths a kind of benchmark? If so, what is the rationale behind this design? The research is based on non-random, non-representative samples. There is no overlap between samples used in the survey and evaluation, plus there is a time gap between the research, so how can we be sure that the survey brings relevant knowledge for the intervention and its evaluation?

The use of a quasi-experimental design with a control group for the intervention's evaluation is one of the strongest methodological solutions available. The fact that the Author invested a lot of effort in arranging a delayed second measurement should be appraised. Two main drawbacks of the design are the use of second-hand data (surveying parents instead of adolescents) and the lack of triangulation of the methods. I understand the decision to focus on parents. It can be justified by the specifics of some of the research questions, directly referring to changes in parents (their knowledge, for instance). I am also fully aware that the youths might not have been easily accessible. But, I would discuss the decision not to use any qualitative methods. The Author had parents in the room and didn't use this opportunity to collect data, which could shed some light on the quantitative results and enrich knowledge about the subject and perception of the program. I remember Author's post-positivist declaration, but what wasn't stated in Donna Mertens textbook is the heteredoxy of contemporary evaluation. Mixing "paradigms" and methodologies is common in today's evaluation. I believe it can bring a lot of added value to this kind of project.

From an editorial point of view, I would recommend summarizing operational definitions, research questions, and hypotheses in one place, preferably in the methodology description. It would make the reader's life easier.

An analysis

There is one important remark I should start from. Almost the entire analysis is based on p-value testing made on non-random samples. I am aware that this strategy is common in many

disciplines, in particular in psychology, and that one may find it in high IF journals. Still, it is disputable from a statistical point of view, and I miss the Author's acknowledgment. Being aware that statistical testing and a holy grail of $p<0.05$ are questionable here, the Author might have been more open in her interpretation of descriptive statistics. See the (nonsignificant) change in watching television shown in Table 14 as an example.

The Author has shown good analytical skills. She seamlessly uses intermediate-level statistics and provides a good quality, comprehensive description of the results. The correlational analysis shows some significant, albeit weak, associations. The Author identified a negative correlation between electronic media usage and sleep quality, as well as interesting differences in sleeping patterns and electronic media usage between Jewish and Arabic adolescents. What I miss is a slightly wider variety of the methods used in the analysis. In particular, the intersection of gender and ethnicity could have been explored more. The structure of subsamples is skewed, with a high overrepresentation of females in the Jewish subsample. An analysis in cohorts and/or mediation analysis would shed more light on the association between gender, ethnicity, and outcome variables. It would be particularly interesting when the main effects are significant, but interaction effects are not, especially when gender-related sleep patterns contradict results from other research. The last finding may be associated with an intersection of gender and ethnicity but also may come from the sampling bias. All comparisons of gender-related sleeping patterns between Jewish and Arab adolescents are questionable due to the extremely low representation of male informants in the Jewish subsample. Also adding effect size (Cohen f, partial eta-squared etc.) to the analysis would also be very informative.

The discussion sections are well written. The Author explores other relevant research, but she hardly goes beyond the comparison of the distributions. This kind of discussion starts pretty late, on pages 208-209, when the Author reviews research on sleep latency among Ultra-Ortodox adolescents, trying to argue the role of culture, particularly the influence of what she calls "Western" countries and culture. I have two comments here. The first: the term "Western" is tricky when we look at the map and country neighbouring Israel. I think that the widely used term Global North would be more precise. Or maybe not: maybe the right reference is Western Europe or the USA. Anyway, the mythical "West" sounds like a popular cliche. The second comment is on the Author's focus on the influence of external, globalized cultures as only (at least the only described in the thesis) factor of change. I am really wondering whether other factors, both mezo and micro level, could play a role here.

The second part of the analysis focused on a program evaluation. The Author decided to measure the impact of the intervention on the participants' knowledge, parenting styles, health behaviors, and life quality of their children. The results showed changes in some areas, particularly in the parents' knowledge and adolescents' media use, and limited changes in sleeping patterns and parental style. What does the lack of change in the quality of life and significant change in sleep patterns and media usage say about the logic underlying the program?

The biggest drawback in analysis, at least from my point of view, is a lack of testing group homogeneity. My intuition says there can be a bias. There are some non-direct signs of it: a control group's average orientation toward authoritative parenting in the first measurement is almost equal to the intervention group post-test score, and a control group reveals visibly higher dispersion values for the bedtime at the end of the week. It may indicate that groups were different in some important aspects, influencing the value of key outcome variables, before the intervention.

And finally, one important note on the visualizations: the vertical and horizontal axes in figures in Chapter 7 are not crossing at the zero point, which may lead to the impression that the difference is higher than it actually is.

Minor remarks:

Table 2 (p. 190) should show the percentages in the columns. Considering differences in subsample sizes, the share in the overall sample is not informative.

What are the characteristics of correlations? The extremely weak R values indicate a non-linear association. By the way, APA recommends upper case for the symbol of Pearson correlation.

The M and SD values in the results of MANOVA analysis are missing. I know they are shown in tables, but including them in the text would make reading easier.

How could the parents from the control group answer the questions about change observed after the intervention (p. 241 and following)?

Editorial comments

- The Author overuses masculine forms for the nouns that should be referred to as gender-neutral.
- There are passages that need English proofreading, but I would not overemphasize it as English is not the native language of the Author (nor of the reviewer). Some additional proofreading (in particular use of capital letters and typos) would help.
- I noted singular mistakes in references, for example date of Abu-Becker's publication (p.36) misses one number. More issues are on the reference list: the title of Lyotard's 1985 work in the reference list is cut. I also do not think that Durkheim's book on sociological method was translated into Hebrew already in 1895. Plus, there are many editorial mistakes.
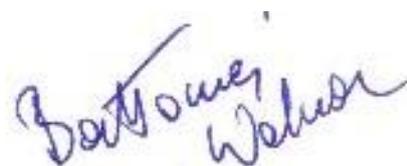
- From time to time, the Author uses *maiestatis pluralis* forms ("we discuss"), which is rather unusual in contemporary scientific language and inconsistent with the rest of the thesis.
- p. 179: the Author says "the following table", but there is no table in this section.

Summary

After reading the dissertation, I felt that it is a good piece of interdisciplinary work, but it is more oriented toward psychology/health studies than other (social) sciences. It is visible in how the Author presents the theoretical framework and conducts analysis. There are some attempts to bring some sociology in, mostly by referencing classic authors, but I do not see sociological thinking in conclusions. Just the traces of the sociological approach, for example when the Author hypothesizes about the role of children's gender, family structure, and gender-driven differences in parental role (pp. 243-244). There are visible influences of evaluation, which I understand as a transdicispline more than a separate branch of science. The Author refers to it when she reveals her paradigmatical orientation and clearly brings a part of her research design from program evaluation. I also think that a homeostatic positivist paradigm does not help to get the wider picture of studied phenomena. Anyway, sociology could shed more light on the results, but my overall view of the theoretical framework and research design adequacy to the subject is positive. Our discipline is differentiated, and obviously, there is a space for good scientific work growing from other perspectives.

I strongly recommend the publication of the results of program evaluation in a relevant journal. Apart from my remarks, it is a theory-driven, well-designed, and scientifically argued evaluation piece. The intervention successes and failures should be disseminated among professionals.

In my review, I tried to show areas for improvement, hoping that it may help the PhD Candidate in her future career. She presented a competent literature review, good knowledge of the theory, and outstanding research and analytical skills. Her research fills the gap in the literature and should be disseminated. In my opinion, the presented thesis meets the criteria for the doctoral grade, and I strongly recommend allowing Mrs. Sundus Abu Ghaneem to take the next steps of the doctoral procedure.

Bartłomiej Walczak