Theory, Schmeory. Let's Keep Our Models Simple and Strong

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Craig Russell has done us all a service in his review of current turnover research (2013), but I think he is too severe in his judgment of the validities and relevance of our field's research, and he encourages us to go down some questionable research paths in the future. Despite his expertise in the field, there are several points where I believe other views are more attractive.

We can recognize the usefulness of assessing so many studies and meta-analyses done in recent decades, as summarized in his Table 1 (drawn from Griffeth, Hom, & Gaertner, 2000). But let us also note that Russell would have done readers a bigger favor if the results were presented in some logical order. Right now, not even an alphabetic order among the variables is evident. This is a flaw that obscures key understandings. Looking at the data another way can make one more optimistic about its predictive validity and relevance.

Validities, and Value, Are High

If we array the findings from high to low on column 5 (the value *p*, or the adjusted correlation between variables and the turnover outcome), it becomes obvious that the strongest predictors of turnover are thinking about it and doing it. That is, the highest relationships are with cognitions of withdrawal (0.32), intention to leave (0.39), and using methods of job search (0.47). That should not be a surprise, and these correlations are quite strong.

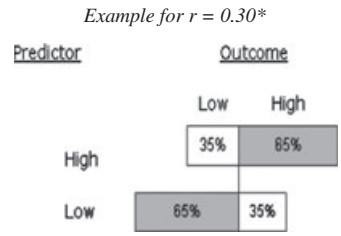
Unfortunately, Russell repeats the lament that not ''more than 10–15% of variance in voluntary turnover decisions'' is predicted. Statistically correct, practically wrong. It is better to ask how much our predictions improve by knowing predictors like intent to turn over and the answer is a ''lot better.'' A correlation of 0.30 will mean a 30% point difference in a criterion (like turnover) between people in the top and bottom halves on a predictor (like intent to leave). In practical terms, for staffing issues and organizational functioning, that is a huge difference.

To illustrate, the work of Rosenthal and Rosnow (1984) would yield this chart to show the effect-size of a 0.30 correlation (Figure 1).

We should also point out that this powerful result is probably understated. The Rosenthal and Rosnow approach describes a situation where both the predictor and outcome variables have reasonably normal distributions. In actuality, intentions to leave and actual turnover are not normally distributed and are usually skewed. That means that the maximum correlation is considerably below the theoretical maximum of 1.00, so a correlation of 0.39 (as with intent to leave) is closer to the maximum attainable correlation than we might think at first.

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*From Rosenthal & Rosnow (1984) Binomial Effect – Size Display, pp. 209–211.

Figure 1. ''Modest'' correlations show powerful practical effects.

Good Theory Should Not Be Too Complex

While Russell notes the search for the ''holy grail'' of better predictors of voluntary turnover, we may be headed for a wild goose chase if we follow his advice to use more and more complex methods (such as overly specified equations and data analysis with unfolding modeling techniques). Although a valid empirically based model can be valuable, much of the effort in this area is wasted due to the search for better and stronger theories, and that often results in a search for more and more variables to throw into a model or formula. I think academic expectations may be too high and the real answer is much simpler. Namely, people are telling us what they will do.

Each individual combines the things important to themselves in deciding whether to stay or go. This yields a formula unique to each person. I am not hopeful that we will soon have a theory that predicts turnover any better than we now do. As I noted some years ago in an early study of voluntary turnover, each person is their own ''most effective integrator of all the relevant factors into the algorithm predicting his/her turnover'' (Kraut, 1975, p. 241). Trying to develop a ''one size fits all'' model requires a universal personality that just isn't there.

Yes, we can accept the importance of various job facets in job-leaving decisions. We can appreciate how ''embeddedness'' or investment in a job makes a difference for all longer term employees, but people vary greatly in how much they are willing to trade off facets like convenience of location, job security, stability of work, and so on for job challenge, continual demands for change, and opportunities for high income. Also, what is important at one point in a person's life can change greatly with age, education, marriage, and children. Opportunities also shift in hard times and boom times. All together, these unique combinations of important job aspects are why many variables shown in Russell's Table 1 have relatively low relationships to turnover. They each get at only a small part of an individual's truth.

Application Varies, But Let's Not Lose Hope

I accept that Russell's experience is that managers facing turnover don't apply our research very much, but I would add that I have also seen HR executives get quite exercised when they see changes in long term trend data that shows rises in employees' Intention to Leave. They don't need our theory to go out and make appropriate changes in compensation, selection, training, recognition, and supervisory practices.

The power of ''Intent to Leave'' as a predictor of actual turnover is a useful sign, even if Human Resource managers don't always use it well. Yes, it would be better if the well-supported theory that such intention really leads to leaving behavior was accepted and used. Just as a person's fever signals infection and can prompt diagnosis and treatment, organizations can use turnover intentions as a signal that action is needed, either to forestall turnover or to staff up to get replacements.

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(One can have a useful theory even if it is not used. Germ theory was well demonstrated in the 1860s, but it took some decades before all physicians washed their hands between patients.)

Future Research

I'd like to suggest two areas that future turnover researchers will find worthwhile. For one, groups rather than individuals ought to be much higher on our voluntary turnover research agenda. This is an attractive but neglected area that would be of great utility to Human Resources Management. Groups are overlooked. My own unpublished work shows huge differences among job families. For example, sales people are much more likely to intend and actually turn over than are factory workers. In general, salaried employees move voluntarily more often than do hourly workers. These differentials may reflect education and/or opportunities. Rank order correlations between a diverse set of groups' average intentions to leave and actual attrition rates can go as high as 0.85. When talking to HR executives, I guarantee that correlations so high are attention getters!

More importantly, within job families, we could assemble intent data that shows that different units are much more at risk than others. Looking at turnover intentions across branch offices, as one example, often shows that certain offices have a much higher proportion than others of people who want to leave. Such high risk offices can become focal points for remedial and fruitful practical attention.

To continue working at the individual level of prediction and theory-building, we should consider better use of the proximal (or close-in) measure of turnover. The three strong predictive measures noted above that deal with intent and action to turn over could be usefully combined to form an index and be a useful criterion that researchers would want to predict from more distant measures of turnover, such as job attitudes and life circumstances like family, education, and job opportunities. That way we might better see what job facets best predict (or ''drive'') the intent and acts of leaving. And we could easily test many hypotheses. (For example, is it true as one saying goes that ''employees join organizations but leave managers?'')

Two other tactics may yield better results. One would be to meld qualitative data based on interviews and case studies, not just survey-based statistics, to point us to the important considerations people use in deciding to leave a job. The other tactic would be to seek out very large national samples for our surveys, so that many different aspects of a person's situation can be included in our model-building. This would be a much stronger approach than trying to pull together the results from lots of different studies with samples that are small and/or from one company.

Conclusion

I appreciate the great thought and effort that Craig Russell has put into this examination, and his suggestions for improved direction on future research on voluntary turnover. However, I think he is too pessimistic about the value of what we already know and its usefulness in organizations. I remain skeptical about the desirability of over-specifying and over-complicating our research models and theories.

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