MKT 791--Research II
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EXEMPLARY RESEARCH: DISTILLED EXPERIENCE

I first prepared these notes for a presentation to the Marketing Department's Doctoral Student Association on December 4, 1992. I update them as seems appropriate. Personal experiences and *Doing Exemplary Research*, edited by Frost and Stablein (1992), are my primary sources.

PREPARE YOURSELF TO DO EXEMPLARY RESEARCH

Read widely and voraciously!

There *is* life beyond *JM, JMR,* and *JCR*. Read novels (Joseph Conrad, Danielle Steel), magazines (*Mad Magazine, The Atlantic*), newspapers, fortune cookies, Celestial Seasonings boxes, etc.

Take as many ***theory*** *classes as you can.*

E.g., social psychology (most useful), sociology, cognitive psychology, etc.

Theory classes will also introduce you to many new methods and procedures--methods and procedures that aren't discussed in "methods" classes (qualitative or quantitative).

SEVEN KEYSTONES TO EXEMPLARY RESEARCH

1. Identify an interesting and important research question . . .

Interesting research examines a fundamental issue. Or it might examine an old issue from a different perspective.

E.g., What does advertising do? Does it build awareness, change/create product beliefs, change attitudes *or* does advertising change the meaning individuals ascribe to the advertised product (cf. McCracken 1986)?

. . . and don't lose sight of it!

Theories, methods, procedures, and samples should be selected and/or developed because they serve your question. Theories or methods should never be chosen simply because they are convenient or because others have used them.

Theories can illuminate a research question, however, be careful that a theory doesn't come to define your research question. It's easy to become enamored with a particular theory and have your view defined entirely by the variables characteristic of that nomological net.

When theory or method-blindness carries the danger that you will overlook (be blinded to) important aspects of your focal phenomenon that fall outside the theory (or method). Be sensitive to that. Don't be afraid to expand the theory or method, combine elements from complementary theories/methods, or construct your own theory/method.

2. *Persistence.*[1]

2.a. *Follow your own nose and stick to your guns.*

If you are pursuing an interesting research question, odds are that others don't know much about it (including your chair). The natural tendency is for others to steer you toward issues/approaches familiar to them or that have worked for them.

Truth is they may not understand the issue that interests you. In that case you must educate them about the issue. You have to help them see what is interesting about the issue and why it is worth pursuing.

Don't be discouraged by nonsupporting results. Unexpected results may signal that you're overlooking something important, that constructs are misconceptualized, measurement sucks, and/or you don't know the phenomenon as well as you think. Seize unexpected results as a learning opportunity. I've learned the most from surprise results!

2.b. *Don't rush it! Exemplary research takes time!*

Exemplary research takes time to do *and* it takes time to publish!

Structure your program to allow for this.

Focus on *quality* not *quantity.* Unfortunately, the P&T process tends to reward quantity (how many?) rather than quality (how good is it?).

3. *Handle your own rat!*[2]

Drown yourself in the details of the research. Be personally involved with all aspects of the project: theory, measure development (if appropriate), instrument preparation, duplication, administration, data coding, punching, analysis, photo copying, collating, etc.

Remember, we do research to learn. The greatest lessons come from unexpected places and phases of the research process. Unless you're intimately involved you'll miss these opportunities.

4. *Don't be a Caveperson: Talk to people!*

Idea creation and development is a social process. Ideas don't evolve in a vacuum, sitting in front of your pc at home. They emerge when talking with other people!

Colleagues are especially good . . . so are spouses, neighbors, etc. . . . evan a conference, on occasion.

Each person you share your ideas with will react to them in a different way. Each person will raise different issues depending on their idiosyncratic perspective and the literature they explore.

The most helpful comments frequently come from people who know little about your topic. They are most likely to ask you questions that are both simple and thought provoking. Simple because you feel like you ought to be able to readily answer them. Thought provoking because you don't have a ready answer.

5. *Write (iterate . . . iterate . . . iterate)*!

Write frequently and a lot! Capture all ideas on paper--no matter how half baked! Ideas can only become fully baked if you spend time with them, get inside of them! Writing gives you something to share with others; something concrete that they can respond to. Writing for yourself and to yourself yields the freest flow of ideas onto paper.

6. *Think simple and elegant.*

Exemplary research looks and feels "right." Ask yourself, "What's the simplest and most direct way I can address my research question and hypotheses?" Keep the KISS principle top of mind!

Tricky or complex manipulations, elaborate designs, or lengthy questionnaires, indicate that you are probably trying to do too much. Complexity increases the likelihood something will go wrong. (My dissertation research cost me about $30!)

7. *Get published.*

This is the best way to communicate your ideas to others.

I was lucky, I was able to publish the major paper from my thesis in the first journal to which I submitted it (*JCR*) . . . and the second paper in the first journal to which I submitted it (*PMS*).

Frost and Stablein make clear that this is not always the case, especially when the research is path breaking. Why?

Authors frequently don't know how best to ***position*** the work (certainly true in my case)

I've been told that one sign of a good thesis is that you don't fully appreciate the significance of what you've done until some years later.

The research may ***not be presented in a manner that helps the reviewers or editor see its contribution***.

This can occur in the conceptual development or in how the results are presented. The challenge here is to bridge the gap between your study and parts of the world familiar to readers of your research. Avoid jargon: use terms a reader will readily understand .

Also, when a paper explores uncharted territory, you are unlikely to find a model in the extant literature to follow.

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1. Also identified by Frost and Stablein (1992) as characteristics of exemplary research.

2. From a book by Heinz Pagels.