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What Entrepreneurship Research can do for Business and Policy Practice[1]

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Abstract. This article discusses the problem of making practical use of research on entrepreneurship. The introduction deals with the general problem of different goals and knowledge interests on the part of researchers and practitioners, and how the differences can be bridged. Regarding entrepreneurship research specifically, it is argued in the following section that there are numerous indications that such research does "Nothing much, really" for business and policy practice. The suggested reason is partly an inability of researchers to address the most relevant issues and to present their findings in a form and place that reaches practitioners. However, the inherent difficulties of the researcher's task as well as shortcomings on the receiver side are also to blame. In the next section it is suggested that entrepreneurship research can actually do "A lot of harm" for business and policy practice. Flaws in research design and analysis, abuse of academic credibility, and structural pressures towards producing research for career management purposes rather than for satisfying curiosity or meeting societal needs may lead in this direction. However, the next section argues that the normal outcome is rather that entrepreneurship research does "Some good" for business and policy practice. Through various routes scholarly knowledge does reach practitioners, and by making abstracted sense of successful entrepreneurship practice, scholars in entrepreneurship can speed up the diffusion of good ideas within a domain and inspire entrepreneurial endeavors in other domains. In the final section examples are given that entrepreneurship research sometimes can make "All the difference in the world." It is argued that the potential for making really important contributions increases if the scope of entrepreneurship research is broadened from a narrow focus on current best practice, towards developing an ability to understand the entrepreneurial implications of technological, cultural, socio-economic, demographic and institutional changes.

Keywords: entrepreneurship research, enterprise policy, business consultancy

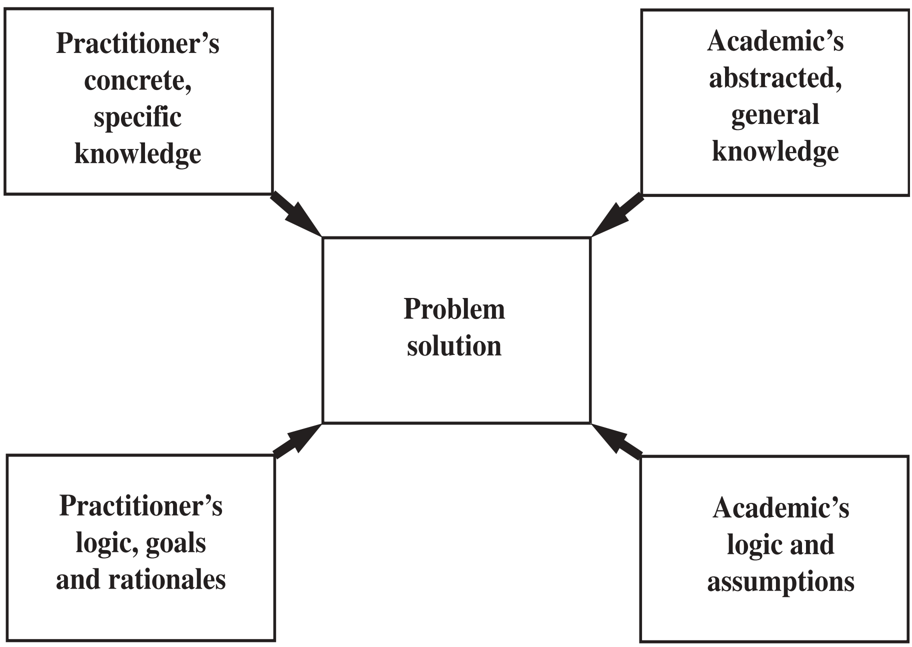
1. Introduction

Academic research can almost never deliver fully developed solutions to any practical problem, and entrepreneurship research is no exception. In order to be useful, scholarly knowledge has to be combined with domain- and situation-specific practical knowledge. Hence, the application of research-based knowledge to practical problems requires a joint effort. This holds true for direct communication in consulting and educational situations as well as

1. This manuscript is based on a keynote address to the International Council for Small Business (ICSB) world conference in Brisbane, 2000. As a result I will here use "entrepreneurship" for "entrepreneurship/small business", largely leaving corporate entrepreneurship and other "modes-of-exploitation" aside.

indirect communication via scholarly articles, textbooks, or trade books. If the practitioner turns to the academic in the belief that she will get a very clear and accurate answer to her particular questions she will get disappointed. If the academic believes she can give such answers without digging deeply into the particularities of the practitioner's task environment she is overly pretentious. The top half of Exhibit 1 illustrates this need for blending of abstracted and specific knowledge for arriving at good solutions to practical problems.

Exhibit 1. Matching of academic and practitioner knowledge and logic



Academics and practitioners have different types of knowledge interests. The academic's duty is to observe generalities and to make abstracted sense of "reality" (whether "reality" refers to something that is objectively existing or socially constructed). This is the upper right hand box. When confronted with a particular problem, the academic's role and habit is to ask: *This is a special case of what?* Looking for generalities and potential for abstracted sense making, she almost as a reflex wants to classify the problem at hand into categories that she knows something about. So she asks: *This is a special case of what?* In the particular case of launching the AutoMower, Husqvarna's robot lawn mower, she might come up with the following:

- This is a marketing problem (not primarily an organization problem)
- This is about *consumer* marketing (not business-to-business)
- This is about *durable* goods (not non-durables)
- This is about *new product introduction* (not defense of market share)
- This is about *radical innovation* (not me-too)
- This is about finding the *early adopters* in order to get extensive and speedy *diffusion.*
- This is about critical *attributes* of the *augmented product* that would appeal to those presumptive early adopters.

Classifying the problem in this way is a natural consequence of having research-based knowledge about marketing and innovations (Howard 1989; Petty and Cacioppo 1986; Rogers 1995). Based on this the academic would advise the practitioner to look for those would-be customers for whom the innovation would have the biggest advantage relative to existing alternatives; to make sure that the product fits with existing norms and with existing technical systems of which it forms part; to reduce complexity as much as possible in the design and descriptions of the product; and to reduce buyers' uncertainty by finding ways that allow them to try out the product without fully committing themselves to it. Further, the academic would suggest that the product be made observable through media coverage and strategically placing it with role model customers; that informative rather than emotional marketing communication be used; and that it be carefully considered to either skim the market with a high introduction price, or try to achieve rapid diffusion and lasting high market share through a bargain introduction price.

Turning the focus in this direction is likely to be good advice to the clever practitioner. But in order to get somewhere we badly need the upper left box as input as well. That is, the practitioner's specific knowledge. The practitioner typically does not approach a problem by asking "This is a special case of what?" Rather, she asks herself "How do I solve this problem? How do I solve *this* problem? What do I do now? What do I *do* now?" The question is urgent, concrete, specific, and begs action. Often the practitioner comes up with a good enough *ad hoc* answer based on unsystematic and subconscious use of experiential knowledge. With the clever application and translation of the academic's abstracted sense making she can often do even better. This requires, however, that the practitioner also has some ability to make use of abstractions. She has to be able to see what the product attributes *relative advantage*, *complexity*, *norm compatibility*, *trialability* and *observability* (Rogers 1995) might mean in her particular case. As regards the AutoMower the academic may be able to give concrete advice on how to improve trialability with money-back guarantees and observability by making samples of the product visible in public places. However, only the practitioner is in a position to judge what of all tricks to achieve trialability and observability for the product fit within existing budgetary restrictions. The academic can talk about high-advantage customers, but it is often the case that only the practitioner, with her thorough market knowledge, is able to make informed guesses as to who these might be, how they can be reached, and what message might appeal to them. The academic would almost certainly be unaware that for men in northern Sweden, tractor-type lawn mowers are a macho attribute, and that users in the UK would want the device to make stripes rather than walk the garden randomly. Both are examples of compatibility problems that are specific to this particular product-market.

Successful practitioners have the kind of creative imagination that is needed to translate the academic's abstracted knowledge into actions suitable for the particular domain. Practitioners who cannot make sense of the abstracted knowledge that the academic delivers should not only question the competence and communication skills of the academic. She should also seriously consider the possibility that it is her own creative imagination that is limited.

Hence, we need both the upper right and the upper left boxes. Both sides have reason to keep this at the top of their minds. Academics who believe their knowledge reaches farther than it actually does run the risk of overly pretentious, erroneous, or just useless advice. Practitioners who believe academics talk a lot of baloney may thus be right. It may also be the case, however, that what the academic says is wise and what is lacking is the practitioner's ability to make the appropriate translations, adaptations and additions that the academic can never provide.

There is one more inherent problem in the application of academic knowledge to practical problems, as indicated by the bottom half of Exhibit 1. When academics and practitioners talk to each other, either directly or indirectly, they may start out from fundamentally different "logics" or perceptions of what the goals are. Academic theorizing often naïvely attributes to practitioners more or less idealistic (or simplistic) goals such as "what is best for society" or "profit maximization." The true goal of the practitioner may not be "to do what I think is right for this company." Instead, it could be "to please my stupid boss." The goal might not be "maximum profit" but "to undo that particular competitor that I hate." On the policy side it may be the case that the true goal is not "to facilitate for small business" but "to get a deal with the coalition party we are dependent upon." When such mismatches in goal perceptions are present, advice based on the incorrect assumptions may be misdirected or appear stupid. There is thus reason for both parties to make sure that there is a common understanding about the goals. A highly relevant next question for academics to ask themselves is whether these are goals that they want to contribute to at all.

This introduction has dealt with the general challenge of matching academic and practitioner knowledge and logic in order to solve particular problems. We can now turn more directly to the specific question "What entrepreneurship research can do for business and policy practice". Four possible answers to this question are:

- Nothing much, really
- A lot of harm
- Some good
- All the difference in the world

The remainder of the paper will elaborate sequentially on these four possibilities, each of which may contain an important part of the truth.

2. Nothing much, really

If research, statistics, case histories, casual observation and other sources of knowledge about entrepreneurship are combined it is relatively easy to compile a long list of embarrassing findings and observations. A few will be considered below. That entrepreneurship research and training can do nothing much, really, for business and policy practice is exemplified by the following:

- We publish our research in books, reports and journals with minimal circulation. This way, our research-based knowledge does not reach business practitioners or policy makers, and not even intermediaries like journalists, consultants or educators who are not researchers themselves. Instead, the "How to" advice that really sells and diffuses comes from practitioners rather than academic sources. Peer reviewed academic outlets are no doubt very important for keeping up the quality of research. Nonetheless, it may be a valid criticism that some universities in some countries create incentives to *only* care about pleasing reviewers and editors and not care at all about what the results mean for practice. To make matters worse there is not even any guarantee that many fellow researchers read what is published, because what the system rewards is *getting published*, not necessarily learning from what others published.
- If we look at individual, big-time entrepreneurs they tend not to be the products of academic training. Consider Ingvar Kamprad, the creator of IKEA and voted the entrepreneur of the (last) century in Sweden. He showed his entrepreneurial tendency in his early teens, long before he had any

business schooling. The little business schooling he eventually got was not entrepreneurship oriented. The most important thing he learnt he derived not from what they taught but from what they did not teach. At the time there was a preoccupation with production costs and how to cut them. Kamprad's early key insight was that production costs were becoming a lesser and lesser share of the price the consumers paid whereas distribution costs were becoming more and more significant. So he focused on the latter, with flat packs and efficient inventory management (Salzer 1994; Torekull 1998). Similar stories apply to many big-time entrepreneurs.

- As regards more mundane entrepreneurs there are several studies that show weak, zero or even negative correlation between taking start-up courses or counseling on the one hand, and successfully launching and/or running a business on the other (Dahlqvist and Davidsson 2000; Dahlqvist, Davidsson and Wiklund 2000; Honig and Davidsson 2000; Maung and Ehrens 1991; Tremlett 1993). This is doubly embarrassing, for it may be interpreted as showing that a) those who have entrepreneurial talent do not come and take the courses or counseling, and b) those who actually come are not turned into successful entrepreneurs.

- Likewise, compilations of research on the effects of SME management training show that it is difficult to find evidence of its effectiveness in well-controlled and/or independent evaluations (Stanworth and Grey 1991; Storey 1994). It is probably safe to add that there is a similar lack of independent and/or well-controlled studies showing that entrepreneurship programs at universities actually create successful entrepreneurs (Alberti 1999).

- On the micro level a lot of research has concerned psychological traits and other personal dispositions of entrepreneurs (Brockhaus 1982; Stanworth, Blythe, Granger and Stanworth. 1989; Chell, Haworth and Brearley 1991). Researchers may vary in their interpretations of the strength of the results that have emerged from this line of research. While some generalities no doubt have been found (Gasse 1996; Johnson 1990; Miner 1996) the present author would argue that it can be safely concluded that there exists no profile of personal characteristics that most entrepreneurs adhere to, or that can predict someone's becoming a business founder with high accuracy. This relative weakness of the results, however, is not the main reason for agreeing with Gartner (1988) that the research on "who is an entrepreneur" has been largely misdirected. Instead, consider what we could ever hope to achieve with that kind of knowledge. What would the advice be? Go get yourself some entrepreneurial parents during your childhood? Go get yourself some innate risk-taking propensity? Clearly, these are qualities that cannot be taught. The best we could ever hope for was a basis for selection: "sorry, you're not the right stuff – try becoming an accountant instead." And what we really have are results that are not strong enough to do a proper job for selection purposes either. The sad thing is that the *lack* of predictive power is what *could* have been predicted with high accuracy from day one. Personal background and psychological traits are what psychologists call "distal" variables. It is well established that such variables may have some effect on many different behaviors in many different situations, but they almost never have a determining influence that over-powers more domain- and situation-specific variables (Delmar 1996 pp. 23-25). It is mainly the latter type of variables that can predict the occurrence and outcomes of particular processes, the creation of a new venture being one example.

- There are also indications that entrepreneurship scholars are unable to locate the most relevant arenas for entrepreneurship. A few examples from Sweden may serve to illustrate this point. The late 1980s was a time of deregulation in agriculture (shortly before Sweden joined the EU and had to regulate again). Suddenly, agriculture became a dynamic industry with lots of innovation. Did we develop entrepreneurship programs for farmers? No. Did entrepreneurship researchers look specifically at that sector? No, instead we continued by habit to exclude that industry from our research, just like we continued by habit to exclude the Eastern European countries from our cross-national research even after the fall of the Berlin Wall. More recent examples suggest that researchers continue to look in the wrong direction. Fashion design and the specialty garment industry are growing significantly in a country where the textile industry was believed to be once and forever dead after the crises in the 1970's. Even more impressive is the recent growth and success of the Swedish popular music industry. Have these industries been the foci of entrepreneurship research in Sweden? The answer is "No". It is probably not very difficult to find similar examples in other countries.

- So far, almost all the responsibility for the "nothing much, really" answer has been placed with the researchers. As explained in the introduction, however, it takes two to tango. Policy-makers did not turn to the new success industries any earlier than researchers did. And sometimes it is clearly an inability from the practitioners' side to make use of research-based knowledge that creates the "nothing much, really" result. As an example consider the following experience of the author's from a few years ago. The Swedish Minister of Industry decided the Government should show some real interest in small business. One of the measures was to form an advisory board on SMEs, consisting of five small business owner-managers and five business professors specialized in small business and entrepreneurship. This was a good initiative. Meetings were held with fruitful discussions where the three parties – practitioners, academics, and policy-makers – gained insights

> into each other's logic, and so forth. The board worked seriously on issues and the feeling was that real results were about to be achieved on working out research-supported policy measures that were both practically and politically realistic. Then what happens? All of a sudden the social democrat government launches a package of small business related measures as part of a deal they have negotiated with their coalition party. Apparently, this happened over the weekend and the small business package was thrown in to balance for something else. Its contents had absolutely nothing to do with the work of the advisory board, and none of its members had been asked to give their opinion on the suggested policy measures. In such cases it is hardly the academic's fault if research-based knowledge about entrepreneurship does nothing much, really, for business and policy practice.

From all these observations it would seem fair to conclude that most entrepreneurship-related business and policy practice goes on relatively independently of our petty little research efforts. This may sound like a sad story but many would hold it to be partly or even wholly true. Rather than getting depressed, though, we have reason to be *im*pressed by the fact that despite incomplete knowledge and flawed analysis, entrepreneurship practice is constantly capable of providing us with new and better products, services and processes, and it is apparently able to do that without making use of researchers' attempts to make systematic sense of reality. Academics and policy-makers alike have reason to be humble and realize that their potential for direct, positive impact on practice is marginal at best in most cases. Most of the important knowledge creation and learning takes place in the daily practical experimentation in the market place.

Another way to look at it is this: academic research is a high-risk endeavor in the sense that the probability of success is very small for each individual research study. Social reality is extremely complex and making valid generalizations and predictions about social events is therefore an extremely demanding task. So, unfortunately, most research projects do not generate a positive net yield of knowledge and impact upon practice. From a portfolio risk perspective there is nothing wrong with this as long as once in a while some researcher actually does come up with something that really is important, and important enough to pay for all the vain efforts. This possibility will be elaborated upon later on in the section "All the difference in the world". But it gets worse before it gets better, as we now turn to the second possible answer to the question "What can entrepreneurship research do for business and policy practice?" namely "A lot of harm".

3. A lot of harm

When entrepreneurship academics do really badly what we do boils down to creating *an inaccurate description of current practice, which we then present as normative advice*. That is, due to flaws in our research design or execution we get the description wrong. To make matters worse we then also confound description with prescription.

It was just noted above that social reality is extremely complex and, therefore, making valid generalizations and predictions about social events is an extremely demanding task. In the language of Method this means that it may be a major challenge to obtain operationalizations that are *valid* indicators of the theoretical constructs we use. A further challenge is to ascertain that the empirical material under study correctly *represents* the part of reality we want to make claims about. Yet another important complexity to uncover is that the empirical relationships that are detected are correctly interpreted as to their *causal order*.

The sad fact is that a standard piece of empirical research in entrepreneurship typically is subject to fundamental problems in all of these regards. Consider us doing a cross-sectional survey of small-business owner-managers, asking them how they go about developing new products and finding new customers. We then claim that they represent “entrepreneurship” although what they represent is actually the population of small firm owner-managers, many of whom are pretty conservative in their behavior and attitudes. Some of them probably were not involved in the start-up of the firm or in any other truly entrepreneurial endeavor. Actually, they do not represent the population of small firm owner-managers either, because studies of this kind typically have a 10-40 percent response rate. So we do not know what the studied “entrepreneurs” represent. Apart from response bias, representation problems arise also from applying results to a different time period or geographical (cultural) context than that from which it originates. At any rate, we let the selected respondents answer a number of questions about their behavior and their perceived success. Then we analyze this and tell the world “If you behave like this, success is likely to follow.”

The fact is that in such a study we have not studied business behavior, and we have not studied how something follows from – is caused by – something else. What we have studied is *not* how entrepreneurial behaviors cause business success. What we *have* studied is the correlation between two paper and pencil behaviors measured at the same time. Is there a tendency for people who answer question (battery) A in a particular way to also answer question (battery) B in a particular way? If yes, we are still very far from proving causal relationships between entrepreneurs’ behaviors and their degree of success.

Contrary to what some researchers believe these problems are not solved by turning to making a few so-called in-depth interviews instead. With that approach the problem remains that you do not actually study behavior. In addition the representation problem and at least some aspects of the validity problem have been further aggravated. So if we base normative advice on research that is subject to these problems, of course we run the risk of doing a lot of harm. Entrepreneurship researchers need to consider that seriously. We have to ask ourselves what right we have to pep-talk individuals into potential financial disaster or governments into wasting tax-payers money, just because we needed some research output in order to meet a quota and please a Dean.

There are other popular ways of creating erroneous normative advice on the basis of inaccurate descriptions of current reality. One is to study only surviving cases. If we sample cases today – entrepreneurs, firms, or projects – we learn nothing about those cases that failed or chose to withdraw along the way. One consequence of this is that everything that increases the likelihood of *both* success *and* failure will be interpreted as success factors. The typical example would be all types of risk taking (less perhaps pure foolishness). Hopefully no social scientist would study lottery winners only and conclude from that research that buying lottery tickets is a sure way to success. The fact is that we do similar things, albeit in a somewhat more subtle manner, when we study only surviving cases and present their behavior and strategies as success factors. Hence, we run the risk of doing a lot of harm when we base our advice on this type of evidence.

Far from being innocent the present author is also one of these clowns disguised as scientific experts. To some extent, however, we may be excused for doing this kind of "not-so-impressive" research and giving the questionable advice that goes with it. Again, social reality is very complex and researching it properly is an extremely demanding task. It can be argued that as entrepreneurship researchers we are worse off than researchers in almost any other domain because what we try to study is a phenomenon that is by definition irregular, unpredictable and seemingly irrational. And we are only human, albeit hopefully mentally well equipped and well educated specimens. We may be excused, at least in part, because it is not easy to stand up against a university incentive system that demands a certain amount of research output whether or not we really found something worth reporting, and whether or not we feel ourselves that we have fully understood as yet what our results really mean. Likewise, it is difficult to resist the pressures of external research grant providers who demand strategic or policy implications already when no more than a surface level understanding has been reached, or policy-makers who want unambiguous, cock-sure advice also when no basis for such advice really exists.

So there are excuses. However, we do have responsibilities as researchers and cannot excuse ourselves for everything. For one thing there are far too many examples of attempts to "sell research findings" rather than providing a balanced account of what the results really are worth. That is, an author presents as hypotheses thoughts that struck him only after he had examined the relationships in the data, remains silent about the critical shortcomings of his research in the hope that the reader will not spot them, and at the same time he tries to portray the implications of his results as more far-reaching than they really are. These are not very excusable practices and they are certainly not reflections of a sound academic culture. Likewise, it is still not unusual to come across entrepreneurship research that justifies questions like "Is this *really* an effort to find out about the stated research questions? Is the researcher himself curious to know the true answer? Or is it *only* an attempt to get *something* out – or an entry ticket to a conference at some attractive location? Is this about producing *knowledge,* or is it just about producing *research output?*"

Another example of less excusable behavior is the entrepreneurship scholar who acts as consultant to, e.g., the enthusiastic new regional development policy maker. It is conceivable in such a situation that the researcher's conclusion is that the well intended initiatives are likely, on balance, to make more harm than good to the economy because they crowd out sound firms or artificially keep alive inferior ones. Rather than saying her true meaning, however, she keeps quiet and happily accepts the consultancy fee. By so doing she has given academic sanction to the measures that the policy maker herself may have regarded as amateurish speculation up to that point. Such abuse of academic expertise is clearly unacceptable. There is enough risk already of making unintended harm because what we set out to do is so inherently difficult.

This section has dealt at length with research that yields an inaccurate image of reality that is then converted into normative advice. When we do marginally better, we end up with an *accurate* image of current practice, on which we base normative advice. The problem here is that most of what practitioners do is probably not worth copying. What the average entrepreneur does may be a feasible thing to do but only rarely the best thing to do. Often their survival and success is a function of fortuitous circumstances rather than their purposeful behavior. Sometimes they do stupid things. Some things they do are really clever, but copying them would not necessarily have the same implications as had inventing them. So, even when we do better and are able to arrive at fairly accurate descriptions in our research, there is still the risk of doing harm rather than good when we convert those descriptions into normative advice.

4. Some good

The standing of entrepreneurship research is not truly as dismal as the early parts of this manuscript may have indicated. There is widespread agreement that the quality of entrepreneurship research has improved markedly in the last decade, and this is also beginning to show in formal evaluative reviews (Busenitz, West, Shepherd, Nelson, Chandler and Zacharakis, forthcoming; Chandler and Lyon 2001). Therefore there is also reason to believe that entrepreneurship research can do – and does – "some good" for business and policy practice. That is what we should turn to now.

Gartner (1988) helped to increase entrepreneurship researchers' chances of doing some good by urging them to re-direct interest from whom the entrepreneur is to what s/he does. However, description of the latter may not suffice. We have dealt already with research that creates inaccurate as well as accurate images of current practice and from that derives normative advice. In both cases we run the risk of doing harm. When we do better we arrive in our research at a reasonably accurate image of not what average entrepreneurs do, but of the behaviors that tend to distinguish successful entrepreneurial processes from the less successful ones. Here, Hornaday (1990), Stevenson (e.g., Stevenson and Jarillo 1990) and Venkataraman (1997; cf. Shane and Venkataraman 2000) are among those who have helped the field turn away from indiscriminately equating "entrepreneurship" with (any kind of) small firm ownership-management.

When we are able to describe what repeatedly successful entrepreneurs do we are starting to have a sound basis for normative advice, and then we can actually do some good. We can observe and compare practice and from that make out what is "best practice". Through our teaching, authoring and consulting efforts we can facilitate the diffusion of such best practice. It can be argued that within narrowly defined industries diffusion takes care of itself, but this is where the academic's special training in abstracted sense making comes in. By making abstracted sense of the observed "best practice" the academic creates potential for spreading, by analogy, new good ideas to other industries or other areas of application. By providing credible explanations for why a new machinery maintenance concept works in the business-to-business market and describing those explanations in more general terms, the academic can inspire another potential entrepreneur to develop a new successful gardening service concept for the household market. That should amount to doing some good. A very promising sign here is that not only piecemeal empirical insights but also theoretical sense-making of successful entrepreneurs' (seemingly irrational) behavior have started to appear, such as McGrath's application of "real options theory" and Sarasvathy's theorizing about entrepreneurial decision-making as "effectuation" (McGrath 1996; 1999; Sarasvathy 2001).

It might be counter-argued that as we hide our results in scholarly journals that non-researchers find utterly boring, research-based insights do not reach practice anyway. But that is not entirely true. We do spread our insights in undergraduate, MBA and executive education. This already adds up to many souls. We do consulting on the side, and sit on boards. The establishing of the *International Journal of Entrepreneurship Education* is in itself an example of an effort to reduce the gap between research and practice. In addition, it may appear that practitioners learn solely from other practitioners, from non-academic consultants in direct interaction, or through “how-to”-books and popular media. However, in these channels one would also find traces of research-based knowledge. It does trickle down to some extent. Sometimes in a somewhat twisted form and frequently as more cock-sure advice than academic hedging prescribes, but really important insights from research do reach practitioners through various routes and intermediaries.

Even some of the less successful research discussed earlier may facilitate doing some good. There is at least one important take-away from the lack of strong results in the hunt for the “typical psychological profile” of the “successful entrepreneur”. One plausible conclusion from the fact that successful entrepreneurs are a very heterogeneous group along most dimensions is something that can be converted to a very positive message to students: it is *not* about being born of “the right stuff.” What the results mean is probably that under the right circumstances, i.e., when faced with the right opportunity, a very large proportion of the population is capable of assuming the entrepreneur’s role. Hearing that message may be very important inspiration for a young person with (as yet) limited self-confidence.

Suggesting that normative advice be based on the characteristics that distinguish particularly successful entrepreneurial processes from less successful ones could easily be criticized from within entrepreneurship research, as it has been shown that we are not able to come up with very strong models for venture performance prediction (Cooper 1995). Thus, there might seem not to exist many general success factors for entrepreneurial processes. However, combining that insight with other results actually does give a basis for abstracted sense making, albeit perhaps at an even higher level. Both policy-makers and would-be entrepreneurs tend to have a spontaneous, naïve belief in simple and general cause-effect relationships, just like researchers have until their own results suggest they should abandon that world-view. Entrepreneurship scholars can do some good by telling practitioners that entrepreneurship is much more about enactment and about fit between opportunity-specific and person-specific qualities than about any specific characteristic in itself (Shane 2000). This, however, is a notion that would require further empirical backing before it can be fully embraced.

It is the present author’s conviction and hope that “some good” is what we usually do in our teaching. Most of the time we probably do no more than that, but no less either. “Some good” is also what most of us, the author included, can ever hope to achieve individually in the better moments of our research. Some good is the likely outcome as long as we can refrain from the inexcusable forms of bad research that were described above. And doing some good is not bad at all.

5. All the difference in the world

Earlier in this manuscript it was suggested that entrepreneurship research can be regarded as a high-risk endeavor and that we should therefore not be surprised that many research projects fail to come up with results or ideas that have an impact on business or policy practice. The other side of that coin is that in the upside tail of the distribution we should find a small number of studies that mean all the difference in the world. With that we have reached the fourth and final answer – by temporal order – to the question "What can entrepreneurship research do for business and policy practice?"

As a primary example of making all the difference in the world has to be mentioned David Birch's study "The Job Generation Process" (Birch 1979). In that study Birch found out about the very important role small and new businesses have for the supply of new jobs in the economy. Subsequently the academic, political and mass-media interest in entrepreneurship started to grow impressively after a long period of marginalization. The last couple of decades have seen tremendous growth in the number of professorships in entrepreneurship, entrepreneurship programs, annual conferences, academic journals devoted to entrepreneurship, et cetera. Public opinion about entrepreneurs appear to have shifted from "crook" to "hero" in many countries, and politicians talk a lot about their importance and sometimes they even do something to encourage their activities. Of course, we should not attribute all of this to Birch's findings. It may well be argued that behind his results were real changes in the economy that would lead to this increased interest, and that someone had to find out about it. It happened to be David Birch.

While it is in all likelihood true that the increased interest in entrepreneurship and small business would have come anyway it is also likely that it would not have come as early or as strongly without Birch's contribution. And David Birch certainly did not just *happen* to be first to find what he found. Some may have opinions about the credibility of Birch's later, non-academic work, but "The Job Generation Process" is clearly a serious piece of research. It was obviously conducted by a researcher who wanted to find out about an important issue. This is not the researcher who takes the easy solution to use the nearest available data set, runs a few regressions and then sends in for publication. On the contrary, Birch realized that no available data set could answer his research question, so he went through the painstaking effort to create one by matching and cleaning available data sets. That is why he was able to come up with an important finding. The quality of his data has subsequently been questioned, but he who cares to read the report will find that this is not the researcher who tries to hide the remaining problems from his readers. Rather, he spends several pages explaining these limitations. Importantly, Birch was not a small business-lover on a crusade, determined to find out about their importance. The great importance of small and new firms was not one of Birch's hypotheses; it was a surprise finding made possible by arranging the data in a more appropriate way than had previously been the case. Neither was Birch of 1979 the researcher who only cared about fancy academic publication and had no interest in practice. Rather the other extreme – the "Job Generation Process" is actually an unpublished report from MIT – but he certainly found ways to reach the minds of practitioners. Interestingly, "The Job Generation Process" does *not* look like a piece by a career-minded researcher who is tactically trying to "collect points" for his next raise or promotion. But by putting the horse in front of the cart rather than the other way round, Birch created bigger career effects than any career-anxious researcher will ever do.

Social scientists very rarely make "scientific discoveries" like that, and it will continue to be rare that a single study or single researcher makes all the difference in the world. But there are other ways to do it. David Storey's book "Understanding the Small Business Sector" (Storey 1994) was an effort to synthesize and give a well-balanced account of a very large number of studies, primarily for a policy-maker audience. Such efforts are very, very important – and undervalued in the academic system. All individual studies have their shortcomings, and so have all individual researchers. Therefore, efforts to compile and synthesize our work in as comprehensive and unbiased a manner as possible are critically important. Actually, policy-makers have little reason to listen to an individual researcher's suggestions for "policy implications" based on a single study. Efforts like Storey's are worth taking seriously, and this book along with his other work has gained a well-deserved position to make a big difference among SME policy practitioners in several European countries. All readers may not agree with all of Storey's conclusions, but that does not change the fact that his is by far the most serious effort to make balanced sense of SME research for SME policy makers.

Even without an identifiable attempt to synthesize our efforts, they may collectively add up to making all the difference in the world. Sometimes immensely important insights creep up upon us little by little, so that we do not realize what a difference has been made. This is a case that could be made for all the research that has gradually taken us away from an "omnipotent, lonely wolf" view, and towards a "relationship manager" view, of the successful entrepreneur. Rather than having them assess whether they were born of "the right stuff" we can now relatively safely direct students and practitioners towards finding one's proper role in an entrepreneurial team and developing an ability to make things happen *with and through other people,* i.e., that the most important entrepreneurial competence is the ability to cultivate and make use of other people's competencies. That is a distinction that can make all the difference in the world.

If we back from asking for making a difference for the entire research or practitioner community we all have fair chances to make all the difference in the world in individual cases. Entrepreneurs are made, not born. Those who believe otherwise may consider us having taken little infant Ingvar Kamprad, or little infant Bill Gates, put them in a dark room and kept them alive with nutrients in there, only to let them out when they turned twenty. Is this thought experiment convincing enough that they would not have created IKEA or Microsoft under such conditions? Clearly, they learnt some things somewhere. In their early careers there was little entrepreneurship research and even less entrepreneurship education around in organized form. Their present-day counterparts, however, are likely to learn some of their skills from formal and research-based entrepreneurship education, even if systematic evaluation studies have as yet not been able to prove the general effectiveness of such programs.

Entrepreneurship scholars are actually in a wonderful position, surrounded by young students with high aspirations to make a difference in the business world, and by practicing entrepreneurs with a proven ability to do so. We also enjoy the privilege of having such people listen to us. This means that if and when we come up with a clever piece of information or inspiration there can be a huge leverage to it. Imagine an entrepreneurship researcher or educator who in his entire career only *once* provides *one* student with a key insight that shapes her future entrepreneurial efforts. If it is the right student, that one key insight may actually justify the entrepreneurship scholar's entire existence in economic terms. Even average golfers occasionally hit a hole-in-one, maybe even two or more in their golfing careers. Entrepreneurship scholars may well do something analogous to that although we do not get as concrete feedback as the golfer on the hits we make. It is a comforting thought that it does not sound unlikely that for some students or entrepreneurs we counsel, the message we give them actually has a key, positive influence on the smart things that they do. These are things entrepreneurship scholars might not have been able to come up with themselves, but the point is that *neither would the practitioners* without their help.

Earlier in this manuscript different bases for normative advice were discussed. With Exhibit 2 we now return to that theme.

- Normative advice based on an inaccurate description of current "average practice"
- Normative advice based on an accurate description of current "average practice"
- Normative advice based on an accurate description of current "best practice"
- Normative advice based on theory-based implications of technological, cultural, socio-economic, demographic and institutional changes

Exhibit 2. Bases for normative advice

It was argued above that for obvious reasons bullet point number three is much preferable to the first two points. However, the third point actually shares with one and two the view that the scholarly study of entrepreneurship is all about trying to find out about current practice. This is a very narrow and delimiting view, which sentences entrepreneurship research to always lag behind entrepreneurship practice. Fully adopting this view means accepting that before there is any work to do for the entrepreneurship scholar, at least some entrepreneurs must already have understood the opportunities that open up because of changes in society. Alternatively, they did not really understand the implications but by chance they acted in a way that was rewarded because of these changes. Either way, research is always lagging behind practice, at least best practice. All we can do is to speed up its diffusion.

In order to sometimes make all the difference in the world entrepreneurship researchers should consider taking on a greater challenge than that. Point four on this exhibit is, arguably, what we really should excel in. To prove that we are experts in abstracted sense making we should really be able to predict what will happen on the market as a consequence of demographic, cultural, socio-economic, and technological changes. Making predictions of that kind is the same as pointing at entrepreneurial opportunities. To study what successful entrepreneurs *have* done is important, but an even more important and interesting question is what *could* be done right now, before somebody else preempts the opportunity that is open at this very moment? What is going on out there right now? What opportunities, if any, does that open up for people, given their particular interests and competencies? Entrepreneurship scholars should be able to answer such questions, too, if we are the experts at abstracted sense making that we claim to be. And entrepreneurship educators could emphasize developing such skills among their students.

The beauty of taking on this challenge, and this broader view of what the scholarly treatment of entrepreneurship should entail, is that it provides entrepreneurship scholars with much more powerful tools than just the empirical generalizations we have about “average” or “expert” entrepreneurs. Making out what entrepreneurial opportunities are implied by societal change is, among other things, about understanding why certain people come to assume certain roles and about how new ideas and new products are adopted and diffused in society. There is a discipline about that: *sociology*. This discipline is full of theories and findings about such issues. Discovering and exploiting opportunities is also about how people get motivated and how they make decisions. There is a discipline for that too: psychology, or *individual psychology* to be more precise. The practice of entrepreneurship is also about convincing others: investors, customers, and employees. That is *social psychology*. Again, the discipline exists, and lots of tools that are ready for use. Things like demand, costs and market structure largely determine the value of an opportunity, i.e., it is about *economics*. Here, too, there are lots of concepts and tools to borrow. And then there is, of course, our mother, alter ego, or next-door neighbor, *management research*. If entrepreneurship is about discovery and exploitation (Shane and Venkataraman 2000), at least the exploitation part overlaps with the concerns of managers and management scholars. Thus, there are still some more wheels we do not have to invent ourselves.

The quote "There is nothing more practical than a good theory" has been attributed to many people. Whoever *really* said it the message is the same: to make more of a difference for practice, we should use more – not less – theory. Existing theories from the disciplines can provide entrepreneurship researchers with stronger frameworks for the domain-specific particularities they want to study. Even when entrepreneurship researchers have failed to do so, entrepreneurship educators can use more general theories from the disciplines as organizing frameworks for the empirical generalizations that emerge. Unlike scholars who are 100% in the disciplines and only look at entrepreneurship as a side issue and at arms-length distance, entrepreneurship scholars are used to viewing reality through entrepreneurship lenses and have enough close-up knowledge about entrepreneurship to *really* make practical entrepreneurship sense of theories from the disciplines.

Point four on Exhibit 2 is still narrow in the sense that it suggests that our main goal is to come up with normative advice related to specific opportunities. Entrepreneurship research and education need not be that restricted. The scholarly treatment of entrepreneurship may well be directed more broadly at enlightening young people with input form many disciplines, and cultivating their ability to criticize current practices and ways of thinking. This accords perfectly with age-old university ideals: broad enlightenment and critical thinking. Of course, the enlightenment and critical thinking would have to be actively geared towards application on entrepreneurship problems, and we would have to be better than universities have been traditionally in one regard: *doer* training. We need to create not just clever critics, but competent actors. That can make all the difference in the world.

In conclusion, this article has argued that there are four possible answers to the question "What can entrepreneurship research do for business and policy practice?" These answers are "nothing much, really", "a lot of harm", "some good", and "all the difference in the world." The argument has been that all four are true to some extent.

For the future, it is the author's hope that entrepreneurship research will continue to do a lot of harm. That is, I hope that our research-based teaching of students and counseling for change-oriented practitioners will do a lot of harm to the "fat cat", conservative and risk-averse practitioners who are not willing to take entrepreneurial risks. I also hope that entrepreneurship research will continue to make life hard for policy-makers of the kind that with well-intended but over-ambitious support measures make the entrepreneurial spirit choke rather than flourish. That is, I hope we can continue to, and become better at, doing some good, and sometimes even making all the difference in the world, for the truly entrepreneurial efforts that will shape our future.

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