给研究生的一些忠告

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译者按：征得 Stephen Stearns 教授本人同意后，本文译自他的著名非学术论文《Some Modest Advice for Graduate Students》。Stephen Stearns 是耶鲁大学生态学与进化生物学讲座讲授，开设有精彩的耶鲁大学公开课《进化、生态和行为原理》。Stearns 教授坦言该忠告是他所有著作中被阅读次数最多，但被引用最少的论文。该译文自 4 月 11 日发布于我的科学网博客后，两天内浏览次数过万。英文原文附于本文文末。既译之，则校之。一校于 2014 年 5 月 1 日

1 永远做好最坏的打算

古人曰：凡事预则立，不预则废。早期的一点点先见之明或许就能帮你避免读研期间的一些灭顶之灾。想吐槽就吐槽吧 (Be cynical)。假设你设计的实验泡汤了，并且某个导师对你的研究计划非但不支持，反而还嗤之以鼻。那么，建议你还是赶紧考虑换一个课题吧。

2 没人来管你

现实中，有些教授会管你，有些则不会。大部分教授估计想管你，但他们都太忙了，意味着实际上他们爱莫能助，因为他们没有额外的时间来管你。因此，你得完全靠自己，而且最好习惯于此。我说这话，主要有以下两层含义：

1. 你最好尽早确定你的研究项目。你的学位需要你自己去争取。你的导师会指导你，并会在一定程度上帮你解决研究生培养过程中和实验经费上的困扰，但是导师不会告诉你下一步研究该怎么做——这些事情都完全取决于你自己。如果你需要指导，就去问导师——这正是他们的职责。

2. 如果你打算学习他人的知识和技能，你必须主动去找他/她，因为他们不会主动来找你。

3 你必须清楚你的研究工作的重要性之所在

当你初来乍到，你得在第一年广泛地阅读文献，并对其深入地思考。刚开始阅读文献时，你可能会感觉到这些论文都在胡说八道，但到后来，你会慢慢体会到其实并非如此。如果你不能理解文献中的一些内容，请别沮丧——其实这不是你的过错，而是作者的问题，因为他没有把论文阐述得足够清楚。

如果某些权威人士告诉你你将一事无成，原因是你没有去上课，没有去收集数据，那么请你告诉他们，你做的研究将来会出成果的。如果他们依旧固执己见，请让他们滚蛋，因为你自己最清楚你所做的研究工作的重要性所在，我了个去！

这个阶段会比较受煎熬，甚至让人感觉度日如年，因为你会由于你的研究停滞不前而万分焦躁。这个时候，你需要不断拷问自己：我现在究竟在干啥？淡定淡定，因为该阶段对你的个人生涯发展至关重要，同时也是产生新想法的关键时期。此时，你得仔细想想，一个重要的科学问题应该是由哪些要素组成的。这个决定必须得由你自己独立作出，理由有以下两点：

1. 首先，如果你所研究的问题是别人给你的，你会感到这个问题的"所有权"并不属于你自己，反而感觉是别人要求去你做的。因此，你将不会主动地去研究该问题、去捍卫它、并为它奋斗，以至获得漂亮的结果；
2. 其次，你攻读博士期间的研究工作将影响你的未来发展。至于你打算以后在哪个领域进行研究，并为之奋斗终身，你得独立地作此决定。

慎思后再决定你的研究方向，这对科学的发展也是很重要的。你可以基于某个研究点，并由此开拓一个全新的研究领域。请记住：如果你根本不清楚你为什么要做这个研究，就开始盲目地收集数据，这将有何意义呢？

4 心理问题是最大的障碍

在你的研究生生涯早期，就必须开始培养坚韧的心理素质，这样就不至于被后来碰到的各种挫折所干扰。如果你一旦放松警惕，来自课业、教学、语言以及其他意料之外的各种压力会把你忙得团团转，正似一个温顺的大分子一样被推来推去做杂乱无章的布朗运动。此处，有一些事情需要注意：

1. 你得仔细斟酌你将在博士期间开展的研究，并能说服你为之奋斗终生。之后，无论你多么努力地工作，你都将难以绕开你在博士期间所做的研究。对于这一点，每个人都一样，因为这源于研究工作的开放性 (open-ended nature)。你需要学会如何判断论文课题的“好坏”。论文通常会越改越好，这促使你可以永无止境地对论文进行修改。

   你得明白你不可能写出一篇“完美”的论文。正如其他任何事物一样，论文也总会有瑕疵。静下心来，合理安排你的时间、金钱和精力，并尽你所能，争取把论文修改到最好。

   你可以提早完成博士期间的其他既定学习任务，以此来稍稍缓解论文撰写的压力。请尽早修满规定课程的学分，并完成相应的考试。这不仅让你提前扫除了开始准备学位论文前的障碍，而且在成功完成这些学习任务后，也能给自己增加信心，并让自己觉得我干得还不错嘛。

2. 恭顺的行为是不会有出色的表现的。期待并强烈要求别人像同事一样看待你。发表论文是你博士生涯中必须得达到的硬指标，而同事或者合作者的态度有时则会有较大的不确定性。如果你表现得像一个同事，别人也会像同事一样对待你。

3. 读研究生可以影响你的未来发展，但这仅是你目前可以选择的途径之一。如果有更好的机会出现，可以考虑暂时放弃读研。以下三种情形可能值得你尝试这样做:

   (a) 首先，一个真正让你心动的机会可能会出现在你眼前，而且这个机会比读研期间所做的任何事都更有创造性和挑战性。当你有足够长的一段时间来证明放弃读研是一个不错的选择时，那就暂时放弃读研吧。这种机会比如是一个跟你博士课题不相关的非洲野外工作项目、一个软件开发的合同、一个在首府科技政策制定部门当助手的机会，或者在某个主流报纸/杂志社当科学栏目实习记者等等；

   (b) 其次，只有在读研期间同时关注这些机会的研究生，才是一个真正自立的研究生。如果你认为读研究生是人生的唯一出路，那么你的心态会变得不稳，并会逐渐感到有一丝沮丧，甚至会因此失去信心，使你难以达到你的最佳状态；

   (c) 最后，如果读研真的不合适你，那么选择继续读研将会伤害你自己，甚至你会因此失去许多其他的工作机会。除了当一名科学家，人生中其实还有许多有趣的事情值得你去做，或者你直接去人力市场找一份工作也可能会比当科学家干得还好。如果你真的不喜欢搞科研，你或许应该尝试其他的工作。但是，千万不要鲁

莽行事 (go off half-cocked)。这是一个很严肃的决定。在做出最后决定前，确保跟学院相关负责人和人品不错的老师沟通一下你的想法。

5 避免上课——上课通常很低效

如果你对你的研究领域已经相当熟悉，那么就尽量少修一些额外的课程。这个建议看上去和之前的有点自相矛盾，其实也有它得道理。当下，你应该学会如何为你的博士课题着想。这需要你主动出击，而不是被动地听课和机械地重复。

学会思考，你需要两样东西：首先你得有一段充裕的时间；其次是，你需要跟比你对该科学问题理解得更透彻的人进行尽量多的一对一交流。

上课可能反而碍事。如果你很有积极性，那么阅读和讨论将会比听课更有效率，更具有启发性。组织少数几个同事一起研讨一个共同感兴趣的话题，并邀请一两位同样对此话题感兴趣的老师参加——这通常会是一个好主意。老师们一般都会愿意参加这种研讨会。毕竟，他们对讨论的话题也感兴趣，所以他们也会因此喜欢上你们的创意。这种讨论同时会给教该门课的老师积累一定的信誉，而且他们还不用做任何工作。何乐而不为呢？

当然，这些“避免上课”的建议不适用于一些介绍专业技能的课程，比如电镜成像、组织学和轻潜水技术 (scuba diving) 等等。

6 写一份研究计划并征求同行的意见

一份研究计划有许多功能：

1. 总结一年来你所读的文献和对它们的思考。通过梳理这些文献和思路，进一步激发你的新想法；
2. 通过一个系统性的总结 (concrete demonstration) 来证明你能合理地利用时间，以此表明你能自觉地学习。
3. 别人因此可以手把手帮你。直接口头沟通心中的想法是相当复杂的，因为心中的想法会相当琐碎，缺乏条理。只有经过精心整理、组织成简洁的文档后，才能在同行之间传阅，并征求他们的评审意见。同行们只有阅读了你的研究计划后，才有可能给出一些具体的建设性意见。
4. 你，也包括其他人，都需要练习写作技能。

5. 找到让你自己满意的科学问题，这一点是很重要的。你需要向同事表明你对此问题已经有一定程度的理解，并且争取你的想法能够得到大家的支持和帮助。其中提出一个研究计划，并达到这个目标的方法是：

   (a) 简要陈述你的研究计划，可以是一个科学问题或者假说。

   (b) 从学术角度指出为什么该假说很重要，而不是从你个人的角度认为该假说很重要。以及，指出该假说与你所从事的研究领域到底有多大的普适性。

   (c) 用详尽的文献综述来证实上述 (b) 点。

   (d) 把你的科学问题拆分成一系列的小问题，然后逐个解决这些小问题。在设计实验、观察或分析等各个步骤，你都将要一一排除一些不合理的备选结果。你需要把这些结果整理出来，并开始解决这些问题。通过把大问题转化成一系列的小问题，你可以明确知道下一步该怎么做，以此减轻刚开始面对一个大问题时的手足无措。你可以从中知道该问题哪些步骤比较费劲，或者困难比较大，并由此你能列出各个步骤的优先顺序。当你碰到某些步骤暂时难以解决时，你可以立即动手先做其他的事情。

6. 列出可能在你的实验过程中出现，并对整个实验具有毁灭性打击的关键步骤，然后依次列出这些关键步骤的其他备选实验。以防在这些关键步骤真的出现错误时，还有备选实验能够补救。

7. 设计两到三个实验，并同时开展，以此确定哪个实验最可能成功——这或许是个不错的主意。在验证你的想法时，可能两到三个模型均有相似的解释力度，但是实际操作过程中，一些不合适的模型会被逐渐排除。设计实验时，“未雨绸缪”总比“亡羊补牢”更有效。

8. 为你博士论文的口头报告选定一个截止日期，然后合理安排从今天到预定截止日期这一段时间。给自己设定好截止日期后，你便会感觉如芒在背，有一种紧迫感。暂且别慌——以这样的心态度过一段时间后，这种紧迫感会更加强烈的。

9. 当你结束文献阅读之后，安排两到三周的时间用来撰写研究计划，并争取让更多的同行对你的研究计划进行评阅，以此争取获得尽量多的评审意见。希望他们返回的这些意见是有所助益的，然后你再针对这些意见认真地修改。

10. 当这些步骤都已经完成时，其实你也差不多写好了博士论文的引言部分，而此时，你从入学到当下，可能方才不到一年至一年半的时间。

7 “管”好你的导师

让你的导师知道你目前在忙什么，但不要打扰到他 (们)。你需要选择一个合适的时机向导师报告你的进展，也就意味着你应该在恰当的时候出现，而不要让导师看到你像看到“害虫”一样。每年至少主动提交给导师一份一至两页的研究进展报告。他们会欣赏这样的做法，并会对你的举动留下良好的印象。

预见并尽量避免跟导师出现个人争端。如果你跟你的导师真的难以继续相处，那么趁早换导师吧。所以，刚开始选择导师时需要非常谨慎，其中最重要的一点是：你对导师 (们) 的研究方向得感兴趣。

8 博士论文的类型

千万不要在一些现成的但模糊不定的想法上说一大堆华而不实的套话。直入主题，并验证一些主要环节中具有重要研究意义但未曾检验的假说，或者直接列出一个新领域的研究纲要。当然，这里还有其他类型的论文：

1. 常规论文包括模型的演绎推算。这些模型应该相当新颖，并能得到令人惊奇的预测结果。在此之后，你若能在对该假说不利的条件下，还能客观地验证该甲说，这将是事半功倍的做法。

2. 对现有的某个重要研究理论进行评述。同样，如果能够合理解释，你也能成为少数几个令人尊敬的赢家。

3. 纯理论研究的论文。这需要勇气，尤其是在一个经验主义者占主导的研究所，但是如果你在数学以及推理能力上足够强的话，你也会成功。

4. 收集一些别人也能同样收集的数据。这是最糟糕的论文，但是有时候可以帮你渡过难关。对于一部分拥有一大堆数据的人，那怕他没有验证一个假说，有时也会给人留下深刻印象：至少结果能说明你已经努力工作了，你也因此可以向你的评审委员会“勒索”(blackmail) 并要求授予你博士学位。

博士论文的类型其实相当地多，就好比有很多种类型的研究生。之前所列举的四种论文分别提供了好、差以及糟糕的案例。准备博士论文的过程中，其实是给你提供了尝试各种研究的机会，并让你体会哪种研究方式最适合你：理论研究、野外工作还是室内工作？理想情况下，你若能合理地权衡这三种研究，那么你将会成为凤毛麟角的集大成者：从经验主义者的角度思考理论问题，从理论学家的视角解决现实世界的问题。

9 趁早发文章

别自欺欺人了。你可能已经参与“发表论文”这个游戏中了，并可能已经暂时搁下你所钟爱的动植物、所好奇的大自然及探索真理的热情。因为，如果你没有学术论文发表，你将找不到工作。你需要在国际上受认可的同行评审期刊上连续发表论文。如果你没有论文，那么你可以放弃科研这条路了。这听上去很残忍，但也不无道理。发表论文其实是一个很有趣的挑战。科学是知识的分享。科学研究成果之所以能存在，是因为它们进行了充分的交流 (“无交流，不科学”)。发表论文只是整个科研工作的一部分，直到文章发表，才意味着该研究工作的结束。撰写学术论文要求简洁扼要并精心组织，而你你必须得掌握这些技能。以下是几点有关论文发表的温馨提示：

1. 跟有更多论文发表经验的学者合作。主动跟目前有共同研究兴趣的教授交流，如果他 (们) 愿意合作，他 (们) 会在你的论文发表过程中提供一些指导。作为回馈，给他署名为该论文的合作者。他会对署名心存感激，并会给你的论文提供许多好的修改建议。

2. 别认为你的第一篇文章就能一鸣惊人。许多杰出的科学家都是从很小的研究工作开始的。学术论文所报道的平均信息量可能比你想像的要少。先在一些不太出名的期刊上发表一到两篇完整的论文，之后再向主流杂志进军。你将会迅速发现，不管杂志的声誉如何，所有杂志编委都会对他们的稿源严格把关，并竭尽全力地保证所发表论文的质量——这也正是他们的职责！

3. 如果你的研究计划已经足够完美，那么可以尝试以评论性的综述论文来发表。如果论文发表了，表明你可能已经选择了合适的研究领域，并可以对此继续开展研究。

4. 不要把你的博士论文写成专著 (monograph)。把你的博士论文写成一系列可以发表的稿件，然后尽早把它们投出去，因此确保在博士学位答辩前，至少博士论文中的一到两章可以成为在刊或刊出的文章。

5. 购买一本 William Strunk 和 E. P. White 合著的《Elements of Style》。在你准备开始撰写第一篇论文前，请仔细阅读此书，然后每隔三四年至少再阅读一遍。Robert Day 的《How to Write a Scientific Paper》也不错。

6. 在你投稿之前，让一个有时间能对你论文的写作、想法和条理性提出建设性修改意见的同行修改。

10 别小看硕士论文

不读硕士的理由常常成为一个普遍的误区：我已经足够优秀了，没必要再做类似硕士论文的事情。其实，完成硕士论文有许多好处：

1. 如果你有心换一所学校或去其他研究所，可以在硕士毕业时重新选择，这是最常见的途径了。你可以利用这个机会拓宽你的研究背景。此外，在你目前的成长过程中，有关你对“一个重要的科学问题应该是由哪些要素组成的”的想法可能会迅速转变。通过换学校，你会立即了解更多其他学者的研究工作，以及他们各自研究工作的野外地点。如果换学校，选择读硕士是最好的方式。你离开母校，如果母校的同行对你的表现也挺满意，那么他们会帮你写一份很给力的推荐信。此时，你其实已经达到攻读博士的大部分要求了。

2. 你积累了许多科学研究中所需的经验，并且已撰写比博士论文难度相对较小的硕士论文。你可以逐渐挑战你自己。通过科研训练，你将能大致地判断解决某个科学问题的难易程度。经历过硕士阶段培训的同学常常会更加容易上手博士论文的研究工作。

3. 你已有发表的论文。

4. 你急啥呢？如果你过早地开始找工作，可能你还没有完全准备妥当。最好稍微晚点出手，先逐渐增强你的个人实力，然后当你积累了更多资本的时候，再去找工作。

11 定期发表论文，但别太频繁

发表论文的压力已经侵蚀了杂志的质量，并同时也侵蚀了科研工作者的精神生活。发表少数几篇能够被广为阅读的高质量论文会比发表一堆迅速被人遗忘的小论文要好很多。不过，现实总是很残忍。为了申请博士后，你需要发表数篇论文；为了获得一个教职，或者

是终身教职，你得发表更多的论文。但是，如果你能把你的研究工作持续发表成连续几篇高质量的论文，那么无论对于你个人，还是你所从事的学科，都是极好的事情。

大多数人只能发表少量的论文，并且对学科发展的贡献也是相当微小。大多数文章的被引用数会很少，甚至不会被引用。90% 的引用来自 10% 的论文。没有被引用的文章其实是时间和精力的浪费。追求高质量，而不是数量。不过，这需要勇气和毅力，但是你不会对此后悔。如果你能够发表一至两篇精雕细琢、同一研究方向的好文章，并且每年能被持续地引用，那么表明你已经干得很不错了，也表明你已经把时间花在刀刃上了。

致谢

感谢Frank Pitelka的提议，以及与Ray Huey共同合作，为未来广大积极有志的研究生提供这些忠告。同时感谢Peter Morin的建议，让我写完这篇忠告并使其发表。

一些很有用的参考书

- **Robert A. Day**. 1983. How to write and publish a scientific paper. 2nd ed. iSi Press, Philadephia. 181 pp. *wise and witty*.
- **R.V. Smith**. 1984. Graduate research - a guide for students in the sciences. iSi Press, Philadelphia. 182 pp. *complete and practical*.
- **William Strunk Jr.**, and **E.B. White**.1979. The elements of style. 3rd Ed. Macmillan, New York. 92 pp. *the paradigm of concision*.

译者跋

英文原文中的第 11 点《补充说明》(Postscript) 未曾翻译，其内容是简要介绍该忠告的写作背景。待我翻译完原作者的《Designs of Learning》后，再在我的博客上一并发布。

本文翻译的早期参考了郑春梅的部分译文，后期修改过程中采纳了刘立对本译文的部分校对，在此表示感谢！

SOME MODEST ADVICE FOR GRADUATE STUDENTS

Stephen C. Stearns

Always Prepare for the Worst

Some of the greatest catastrophes in graduate education could have been avoided by a little intelligent foresight. Be cynical. Assume that your proposed research might not work, and that one of your faculty advisors might become unsupportive - or even hostile. Plan for alternatives.

Nobody Cares About You

In fact, some professor care about you and some don't. Most probably do, but all are busy, which means in practice they cannot care about you because they don't have the time. You are on your own, and you had better get used to it. This has a lot of implications. Here are two important ones:

1) You had better decide early on that you are in charge of your program. The degree you get is yours to create. Your major professor can advise you and protect you to a certain extent from bureaucratic and financial demons, but he should not tell you what to do. That is up to you. If you need advice, ask for it: that's his job.

2) If you want to pick somebody's brains you'll have to go to him or her, because they won't be coming to you.

You Must Know Why Your Work is Important

When you first arrive, read and think widely and exhaustively for a year. Assume that everything you read is hogwash until the author managed to convince you that it isn't. If you do not understand something, don't feel bad - it's not your fault, it's the author's. He didn't write clearly enough.

If some authority figure tells you that you aren't accomplishing anything taking courses and you aren't gathering data, tell him what you're up to. If he persists tell him to bug off, because you know what you're doing, dammit.

This is a hard stage to get through because you will feel guilty about not getting on your own research. You will continually be asking yourself, "What and I doing here?" Be patient. This stage is critical to your personal development and to maintaining the flow of new ideas into science. Here you decide what constitutes an important problem. You must arrive at this decision independently for two reasons. First, if someone hands you a problem, you won't feel that it is yours, you won't have that possessiveness that makes you want to work on it, defend it, fight for it, and make it come out beautifully. Secondly, your Ph.D. work will shape your future. It is your choice of a field in which to carry out a life's work. It is also important to the dynamic of science that your entry be well thought out. This is one point where you can start a new area of research. Remember, what sense does it make to start gathering data if you don't know - and I mean really know - why you're doing it?

Psychological Problems are the Biggest Barriers

You must establish a firm psychological stance early in your graduate career to keep from being buffeted by the many demands that will be made on your time. If you don't watch out, the pressures of course work, teaching, language requirements and who know what else will push you around like a large, docile molecule in Brownian motion. Here are a few things to watch out for:

1. The initiation-rite nature of the Ph.D. and it's power to convince you that your value as a person is being judged. No matter how hard you try, you won't be able to avoid this one. No one does. It stems from the open-ended nature of the thesis problem. You have to decide what a "good" thesis is. A thesis can always be made better, which gets you into an infinite regress of possible improvements.

Recognize that you cannot produce a "perfect" thesis. There are going to be flaws in it, as there are in everything. Settle down to make it as good as you can within the limits of time, money, energy, encouragement, and thought at your disposal.

You can alleviate this problem by jumping all the explicit hurdles early in the game. Get all of your course requirements and examinations out of the way as soon as possible. Not only do you thereby clear the decks for your thesis, but you also convince yourself, by successfully jumping each hurdle, that your probably are good enough after all.

2. Nothing elicits dominant behavior like subservient behavior. Expect and demand to be treated like a colleague. The paper requirements are the explicit hurdle you will have to jump, but the implicit hurdle is attaining the status of a colleague. Act like one and you'll be treated like one.

3. Graduate school is only one of the tools that you have at hand for shaping your development. Be prepared to quit for awhile if something better comes up. There are three good reasons to do this.

First, a real opportunity could arise that is more productive and challenging than anything you could do in graduate school and that involves a long enough block of time to justify dropping out. Examples include field work in Africa on a project not directly related to your Ph.D. work, a contract for software development, an opportunity to work as an aide in the nation's capital in the formulation of science policy, or an internship at a major newspaper or magazine as a science journalist.

Secondly, only be keeping this option open can you function with true independence as a graduate student. If you perceive graduate school as your only option, you will be psychologically labile, inclined to get a bit desperate and insecure, and you will not be able to give your best.

Thirdly, if things really are not working out for you, then you are only hurting yourself and denying resources to others by staying in graduate school. There are a lot of interesting things to do in life besides being a scientist, and in some the job market is a lot better. If science is not turning you on, perhaps you should try something else. However, do not go off half-cocked. This is a serious decision. Be sure to talk to fellow graduate students and sympathetic faculty before making up your mind.

Avoid taking Lectures - They're Usually Inefficient

If you already have a good background in your field, then minimize the number of additional courses you take. This recommendation may seem counter-intuitive, but it has a sound basis. Right now, you need to learn how to think for yourself. This requires active engagement, not passive listening and regurgitation.

To learn to think, you need two things: large blocks of time, and as much one-on-one interaction as you can get with someone who thinks more clearly than you do.

Courses just get in the way, and if you are well motivated, then reading and discussion is much more efficient and broadening than lectures. It is often a good idea to get together with a few colleagues, organize a seminar on a subject of interest, and invite a few faculty to take part. They'll probably be delighted. After all, it will be interesting for them, they'll love your initiative - and it will give them credit for teaching a course for which they don't have to do any work. How can you lose?

These comments of course do not apply to courses that teach specific skills: e.g., electron microscopy, histological technique, scuba diving.

Write a Proposal and Get it Criticized

A research proposal serves many functions.

1. By summarizing your year's thinking and reading, it ensures that you have gotten something out of it.

2. It makes it possible for you to defend your independence by providing a concrete demonstration that you used your time well.

3. It literally makes it possible for others to help you. What you have in mind is too complex to be communicated verbally - too subtle, and in too many parts. It must be put down in a well-organized, clearly and concisely written document that can be circulated to a few good minds. Only with a proposal before them can the give you constructive criticism.

4. You need practice writing. We all do.

5. Having located your problem and satisfied yourself that it is important, you will have to convince your colleagues that you are not totally demented and, in fact, deserve support. One way to organize a proposal to accomplish this goal is.

   a. A brief statement of what you propose, couched as a question or hypothesis.

   b. Why it is important scientifically, not why it is important to you personally, and how it fits into the broader scheme of ideas in your field.

   c. A literature review that substantiates (b).

   d. Describe your problem as a series of subproblems that can each be attacked in a series of small steps. Devise experiments, observations or analyses that will permit you to exclude alternatives at each stage. Line them up and start knocking them down. By transforming the big problem into a series of smaller ones, you always know what to do next, you lower the energy threshold to begin work, you identify the part that will take the longest or cause the most problems, and you have available a list of things to do when something doesn't work out.

6. Write down a list of the major problems that could arise and ruin the whole project. Then write down a list of alternatives that you will do if things actually do go wrong.

7. It is not a bad idea to design two or three projects and start them in parallel to see which one has the best practical chance of succeeding. There could be two or three model systems that all seem to have equally good chances on paper of providing appropriate tests for your ideas, but in fact practical problems may exclude some of

them. It is much more efficient to discover this at the start than to design and execute two or three projects in succession after the first fails for practical reasons.

8. Pick a date for the presentation of your thesis and work backwards in constructing a schedule of how you are going to use your time. You can expect a stab or terror at this point. Don't worry - it goes on like this for awhile, then it gradually gets worse.

9. Spend two to three weeks writing the proposal after you've finished your reading, then give it to as many good critics as you can find. Hope that their comments are tough, and respond as constructively as you can.

10. Get at it. You already have the introduction to your thesis written, and you have only been here 12 to 18 months.

Manage Your Advisors

Keep your advisors aware of what you are doing, but do not bother them. Be an interesting presence, not a pest. At least once a year, submit a written progress report 1-2 pages long on your own initiative. They will appreciate it and be impressed.

Anticipate and work to avoid personality problems. If you do not get along with your professors, change advisors early on. Be very careful about choosing your advisors in the first place. Most important is their interest in your interest.

Types of Theses

Never elaborate a baroque excrescence on top of existing but shaky ideas. Go right to the foundations and test the implicit but unexamined assumptions of an important body of work, or lay the foundations for a new research thrust. There are, of course, other types of theses:

1. The classical thesis involves the formulation of a deductive model that makes novel and surprising predictions which you then test objectively and confirm under conditions unfavorable to the hypothesis. Rarely done and highly prized.

2. A critique of the foundations of an important body of research. Again, rare and valuable and a sure winner if properly executed.

3. The purely theoretical thesis. This takes courage, especially in a department loaded with bedrock empiricists, but can be pulled off if you are genuinely good at math and logic.

4. Gather data that someone else can synthesize. This is the worst kind of thesis, but in a pinch it will get you through. To certain kinds of people lots of data, even if they don't test a hypothesis, will always be impressive. At least the results show that you worked hard, a fact with which you can blackmail your committee into giving you the doctorate.

There are really as many kinds of theses as there are graduate students. The four types listed serve as limited cases of the good, the bad and the ugly. Doctoral work is a chance for you to try you had at a number of different research styles and to discover which suits you best: theory, field work, or lab work. Ideally, you will balance all three and become the rare person who can translate the theory for the empiricists and the real world for the theoreticians.

Start Publishing Early

Don't kid yourself. You may have gotten into this game out of love for plants and animals, your curiosity about nature, and your drive to know the truth, but you won't be able to get a job and stay in it unless you publish. You need to publish substantial articles in internationally recognized, referred journals. Without them, you can forget a career in science. This sounds brutal, but there are good reasons for it, and it can be a joyful challenge and fulfillment. Science is shared knowledge. Until the results are effectively communicated, they in effect do not exist. Publishing is part of the job, and until it is done, the work is not complete. You must master the skill of writing clear, concise, well-organized scientific papers. Here are some tips about getting into the publishing game.

1. Co-author a paper with someone who has more experience. Approach a professor who is working on an interesting project and offer your services in return for a junior authorship. He'll appreciate the help and will give you lots of comments on the paper because his name will be on it.

2. Do not expect your first paper to be world-shattering. A lot of eminent people began with a minor piece of work. The amount of information reported in the average scientific paper may be less than you think. Work up to the major journals by publishing one or two short - but competent - papers in less well-recognized journals. You will quickly discover that no matter what the reputation of the journal, all editorial boards defend the quality of their project with jealous pride - and they should!

3. If it is good enough, publish your research proposal as a critical review paper. If it is publishable you've probably chosen the right field to work in.

4. Do not write your thesis as a monograph. Write it as a series of publishable manuscripts, and submit the early enough so that at least one or two chapters of your thesis can be presented as reprints of published articles.

5. Buy and use a copy of Strunk and White's Elements of Style. Read it before you sit down to write your first paper, then read it again at least once a year for the next three or four years. Day's book, How to Write and Publish a Scientific Paper, is also excellent.

6. Get your work reviewed before you submit it to the journal by someone who has the time to criticize your writing as well as your ideas and organization.

Don't Look Down on a Master's Thesis

The only reason not to do a master's is to fulfill the generally false conceit that you're too good for that sort of thing. The master's has a number of advantages.

1. It gives you a natural way of changing schools if you want to. You can use this to broaden your background. Moreover, your ideas on what constitutes an important problem will probably be changing rapidly a this stage of your development. Your knowledge of who is doing what, and where, will be expanding rapidly. If you decide to change universities, this is the best way to do it. You leave behind people satisfied with your performance and in a position to provide well-informed letters of recommendation. You arrive with most of your Ph.D. requirements satisfied.

2. You get much-needed experience in research and writing in a context less threatening than doctoral research. You break yourself in gradually. In research, you learn the size of a soluble problem. People who have done master's work usually have a much easier time with the Ph.D.

3. You get a publication.

4. What's your hurry? If you enter the job market too quickly, you won't be well prepared. Better to go a bit more slowly, build up a substantial background, and present yourself a bit later as a person with more and broader experience.

Postscript

This comment was originally entitled "Cynical aids towards getting a graduate degree, or psychological and practical tools to use in acquiring and maintaining control over your own life." It originated as a handout for the Ecolunch Seminar in the Department of Zoology, University of California, Berkeley, on a Monday in the spring of 1976. Ecolunch was, and is, a Berkeley institution, a forum where graduate students present their work in progress and receive constructive criticism. At the start of the semester, however, no one is ready to talk. This was such a time.

On Friday morning at Museum Coffee, Frank Pitelka, who was in charge of Ecolunch for that semester, asked me to make the presentation on the following Monday. "Asked" is probably a misleading representation of Frank's style that morning. Frank bullied me into it. I had just given a departmental seminar on the Ph.D. work I had done at British Columbia, and did not have much new to say about biology. Frank's style brought out the rebel in me. I agreed on the condition that I had complete freedom to say whatever I wanted to, and that the theme would be advice to graduate students. Frank agreed without apparent qualms. Then I charged upstairs to Ray Huey's office to plot the attack.

I whipped out an outline, Ray responded with a more optimistic and complementary version (see the following Commentary article), and I wrote a draft at white heat that afternoon. We felt like plotters. We were plotters. There were acts of self-definition in the air. On Monday, I recall that I made a pretty aggressive presentation in which, to emphasize how busy faculty members were, I kept looking at my watch. Near the end I glanced at my watch one last time, said I had to rush off to an appointment, left the room suddenly without taking questions, and slammed the door. They waited. I never came back, but Ray took over and presented his alternative view. Ray told me later that Bill Lidicker turned to him and said, "You mean he's not coming back?" I wasn't. Fortunately, they took it well. They were and are a group of real gentlemen.

I mention these things to explain the tone of our pieces. We would not write them that way now, having been professors ourselves for some years. We never intended to publish them, having regarded the presentations as a one-time skit, but our notes were xeroxed and passed around, and eventually they spread around the United States. In the fall of 1986 I got a letter from Pete Morin at Rutgers suggesting that we publish the notes. Its survival for ten years in the graduate student grapevine convinced me that there might actually be a demand for them. I had lost my original, and Pete kindly sent me a copy, which turned to be a nth generation version with marginal notes by a number of different graduate students. On rereading it, I find that I agree with the basic message as much as ever, but that many of the details do not apply outside the context of large American universities.

Ten years later, I have one after-thought.

Publish Regularly, but Not Too Much

The pressure to publish has corroded the quality of journals and the quality of intellectual life. It is far better to have published a few papers of high quality that are widely read, then it is to have published a long string of minor articles that are quickly forgotten. You do have to be realistic. You will need publications to get a post-doc, and you will need more to get a faculty position and then tenure. However, to the extent that you can gather your work together in substantial packages of real quality, you will be doing both yourself and your field a favor.

Most people publish only a few papers that make any difference. Most papers are cited little or not at all. About 10% of the articles published receive 90% of the citations. A paper that is not cited is time and effort wasted. Go for quality, not for quantity. This will take courage and stubbornness, but you won't regret it. If you are publishing one or two carefully considered, substantial papers in good, refereed journals each year, you're doing very well - and you've taken enough time to do the job right.

Acknowledgments

Thanks to Frank Pitelka for providing an opportunity, to Ray Huey for being a co-conspirator and sounding board and for providing a number of the comments presented here, to the various unknown graduate students who kept these ideas in circulation during the last decade, and to Pete Morin for suggesting that we write them for publication.

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