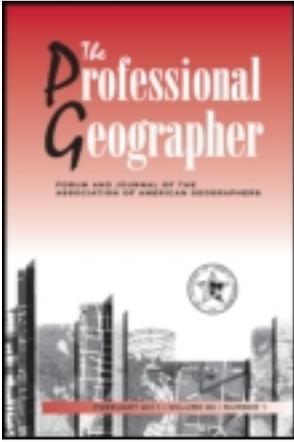


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On: 04 January 2014, At: 06:54

Publisher: Routledge

Informa Ltd Registered in England and Wales Registered Number: 1072954 Registered office: Mortimer House, 37-41 Mortimer Street, London W1T 3JH, UK



The Professional Geographer

Publication details, including instructions for authors and subscription information:

<http://www.tandfonline.com/loi/rtpg20>

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Published online: 11 Feb 2013.

To cite this article: Chris Muellerleile, Kendra Strauss, Ben Spigel & Thomas P. Narins (2014) Economic Geography and the Financial Crisis: Full Steam Ahead?, *The Professional Geographer*, 66:1, 11-17, DOI: [10.1080/00330124.2012.757819](https://doi.org/10.1080/00330124.2012.757819)

To link to this article: <http://dx.doi.org/10.1080/00330124.2012.757819>

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Economic Geography and the Financial Crisis: Full Steam Ahead?*

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This article considers whether the growing theoretical and methodological diversity or pluralistic nature of economic geography contributes to its lack of engagement outside the discipline and academy. Although we are enthusiastic about the vibrancy this pluralism brings, we also speculate that it contributes to the discipline's tendency to fall short of significantly impacting key debates in the social sciences. In particular, we consider the disciplinary challenges to influencing mainstream debates over financialization and the recent financial crisis and the recurring lament that economic geography "misses the boat" by failing to significantly impact key scholarly and policy issues. Specifically, we suggest that methodological and theoretical diversity, local contextualization, and relational analysis, all of which we support as vital to the discipline, make it difficult to isolate a disciplinary core. We conclude that pluralism produces a vibrant discipline with unique explanatory power but that it also has important impacts on the design, execution, and influence of geographers' research outside the discipline. **Key Words:** crisis, economic geography, finance, pluralism.

本文探讨经济地理学中逐渐增长的理论与方法多元性, 是否有助于改善该领域在学术及学科之外缺乏参与的问题。我们虽然热切欢迎多元主义所带来的活力, 但亦同时质疑其是否有助于改善该学科无法显著地影响社会科学中重要辩论的发展倾向。我们特别关照“影响关于金融化与近日金融危机之主流辩论”的学科挑战, 以及重新浮现有关经济地理学无法影响重要学术与政策议题以致“错失良机”之感叹。我们特别主张, 方法与理论的多元化、在地的脉络化、以及关系性的分析——这些我们所支持的学科关键, 皆导致难以分离出单一的学科核心。结论部分我们将主张, 多元主义创造了具有独特解释力的活跃学科, 但同时地理学者的研究在学科之外的设计、执行与影响力具有重要的影响。 **关键词:** 危机, 经济地理学, 金融, 多元主义。

Este artículo trata de establecer si la creciente diversidad teórica y metodológica de la geografía económica, o su naturaleza pluralista, contribuyen a su falta de compromiso por fuera de la disciplina y de la academia. Si bien somos entusiastas acerca de la vitalidad que conlleva ese pluralismo, también especulamos que esto fortalece una tendencia de la disciplina en el sentido de quedarse corta en impactos significativos en los debates claves de las ciencias sociales. Consideramos, en particular, los retos disciplinarios para influir en los debates de actualidad sobre financialización y la crisis financiera reciente, y los recurrentes lamentos de que la geografía económica “pierde el barco” al no impactar significativamente temas cruciales académicos y políticos. Específicamente, sugerimos que la diversidad metodológica y teórica, la contextualización local y el análisis relacional—todos los cuales consideramos vitales para la disciplina—dificultan la clara demarcación de un núcleo disciplinario. Concluimos que el pluralismo puede dotar a una disciplina vibrante con un poder explicativo único, pero al mismo tiempo tener impactos importantes sobre el diseño, ejecución e influencia de la investigación de los geógrafos por fuera de la disciplina. **Palabras clave:** crisis, geografía económica, finanzas, pluralismo.

Although the book was fundamentally geographical, I took the decision at the outset not to use the word “geography” in the title nor even to divulge my identity as a geographer. In some ways, I now feel a little ashamed of having done that. On the other hand, at that time—and, to some extent, this is still the case—most people would

not have taken such a book written by a “mere geographer” very seriously. So it proved.

—Peter Dicken (2004b, 514), reflecting on his book, *Global Shift*, eighteen years after publication of the first edition

*We are grateful to all of the attendees of the 2010 Summer Institute in Economic Geography for cultivating the ideas that resulted in this article. Specifically, we would like to thank Jamie Peck, Trevor Barnes, David Rigby, and John Agnew, and two anonymous reviewers for their thoughtful comments on an earlier draft of this article. The authors, however, are solely responsible for any errors.

It is likely a coincidence that the same year Peter Dicken discussed his hesitancy to identify as a geographer, he also made the case that geographers had “missed the boat” of scholarly debates about the concept of globalization (Dicken 2004a). Dicken’s two observations, coincidence or not, are indicative of a key disciplinary challenge debated at the 2010 Summer Institute for Economic Geography (SIEG)—that between diversity and engagement: diversity, because regardless of our quick development into a friendly community, it was a community incapable of agreeing, or unwilling to agree on a common theoretical, methodological, or topical core to unite us; engagement, because although we passionately debated the strategies, scope, nature, and relevance of our work on the most pressing contemporary issues, we found no common definition of, or strategy for, engagement with them. Ten years after Amin and Thrift’s (2000) oft-cited suggestion of a “turning point” in economic geography, the discipline is more theoretically diverse than ever, but is it also less engaged? Although we generally applaud increasing pluralism, we wonder if economic geographers in their expanding diversity have inadvertently sacrificed some of their capacity to make an impact outside the discipline and the academy. In particular, we are interested in pluralism in geographers’ work on the recent financial crisis. In that context we question, first, what constitutes the pluralism of the discipline and, second, whether geographers should be concerned that this pluralism further denies economic geography the benefits of a disciplinary core.¹

The authors were all drawn to economic geography by what seemed a heady mix of theoretical vibrancy, methodological openness, and clear (if varied) political commitments. Now that we are on the “inside,” we see that this heterogeneity has implications for early-career academics. As discussed at the SIEG, we find this especially important in areas such as positioning within a tight academic job market. We might be more comfortable than others conversing outside our discipline, but we also find it more difficult to identify our scholarly niche and thus our unique contributions qua geographers. This raises key questions about our disciplinary response to, and engagement with, the recent financial crisis and its reverberations. Nevertheless, even though it often seems like a potential solution, we believe that, now more than ever, Amin and Thrift’s (2000) warning holds true: We should not seek legitimacy through rapprochement with orthodox economics, no matter how much we might envy that discipline’s apparent coherence and clout, seemingly undiminished, like the power of investment bankers post-2008. In other words, even though our ultimate motivation is to do “urgent” research (Samers 2001), we suspect that a direct engagement with rational actors, efficient markets, and neo-classical pricing theory would quickly squeeze out what we are most interested in: unevenness, inequality, historical contingency, and alternative futures. In fact, we need not look further

than economic sociology, always a potential disciplinary partner, to see the unfortunate effects of an attempted reconciliation with mainstream economics² (Peck 2005).

In particular, we are reacting to Engelen and Faulconbridge’s (2009) “Dickensian” warning that the financial crisis could be geography’s “next missed boat.” Two years later our qualified response is that economic geographers have responded to the crisis with all hands on deck, but we are nevertheless interested in why there is a recurring lament that economic geographers are insufficiently engaged in key debates. In fact, there is a growing body of geographic work related to the economic and financial crisis that began in 2007 (Aalbers 2009; French, Leyshon, and Thrift 2009; Mann 2009; Castree 2010; Harvey 2010, to name only a few). Other than calls for more geographically minded work (Engelen and Faulconbridge 2009; Lee et al. 2009; Wojcik 2009), however, there is a distinct lack of consensus in economic geographers’ reactions. So although we are generally sympathetic with Barnes and Sheppard’s (2010) promotion of “engaged pluralism” in economic geography, we also want to suggest that there is a trade-off between pluralism and engagement. In other words, we believe that economic geographers’ hesitance to “discipline” each other contributes to the inability to pinpoint just what economic geographers do and, thus, how they enrich larger debates. At a time when there is citational evidence that economic geography might have growing influence outside the discipline (Foster et al. 2007), this is an important consideration.

All of this said, we do not attempt a comprehensive survey of economic geographers’ contributions to discussions of the 2008 financial crisis. Instead, we have chosen three examples of emergent geographic work on finance and crisis that, in their methodologies, help explicate three components of our broader argument about the trade-offs between disciplinary diversity and engagement. The first example, focused on the politics of postcrisis pension provision, considers the remarkable analytical benefits, but also the disciplinary trade-offs, to using a heterodox collection of methods and theoretical frameworks that are common in economic geography. The second example, focused on postcrisis industrial clusters, considers the long-standing challenge to geographers of making the contingency of local geographies relevant to other disciplines and policymakers. The third example, focused on postcrisis economic recovery in Latin America, considers the necessity of relational analysis to economic geography but also the challenge of making the often “messy” explanations relevant to “outsiders.” Considered together, these examples demonstrate that much of the explanatory power of the discipline comes from the same things that contribute to the perception that it fails to engage. Far from simple character flaws, these idiosyncrasies are fundamental to the field’s commitment to broaden economic discourses. In the conclusion we question whether contemporary

economic geography is well suited to produce the sort of pithy responses necessary to engage outside the discipline and academy without jettisoning what we consider some of its most important characteristics.

The Politics of Pension Inequality

The work by economic geographers on pensions and economic decision making is illustrative of the issues, challenges, and opportunities of a pluralistic discipline, especially at a time when reductions in public employee pension contributions have become a key component of postcrisis austerity movements in the United Kingdom and United States. A significant strand of the research agenda of the Centre for Employment, Work and Finance directed by Gordon Clark and Linda McDowell at Oxford University focuses on the evolution of occupational pension systems and institutions and processes of economic decision making of participants, trustees, and investors. Within this broad program some themes have been explicitly—or perhaps “traditionally”—geographical in the sense of exploring the spatial dimensions of differentiation and inequality (Clark and Knox-Hayes 2007; Strauss and Clark 2010). Others have been explicitly theoretical, engaging critically with increasingly influential behavioral economic research on pension decision making (Strauss 2008, 2009; Clark 2010). Perhaps tellingly, however, it is the empirical, mostly quantitative explorations of underresearched issues (e.g., attitudes toward choice and risk and the role of socioeconomic status in decision making in UK defined contribution pensions), often grounded in new primary data sets, that have garnered the most attention in policy circles and outside the field of geography (see, e.g., Clark, Caerlewy-Smith, and Marshall 2006; Clark and Strauss 2008; Clark, Duran-Fernandez, and Strauss 2010). In terms of diversity, the broader pension project—like much in economic geography—draws on other disciplines, engaging geographical concepts and theories in (often critical) conversation. This means that it is not always easy to distinguish a geographical “core” to the research, unless one counts the critique of aspatial models and modes of theorizing. Nevertheless, to those on the “inside,” the ways in which this research is grounded in an understanding of the economic that encompasses the importance of context, and a fundamental concern with economic inequality and social justice, to us reveals its lineage in critical and feminist economic geography.

The question is this: If economic geographers undertake engaged and diverse research projects, why is there a sense that we are prone to “miss the boat”? For example, what have been the contributions to date from geographers in analyzing the unprecedented attack on the postwar model of public-sector pension provision in the United States and United Kingdom? From our perspective there are a number of contributing factors. Early-career academics must focus on publishing papers in peer-reviewed journals if they wish to

be employable in an increasingly competitive and international job market. And, as in other disciplines, the realities of academic publishing mean that many papers will take between twelve and twenty-four months to be published. A paper that first explored some of the implications of the financial crisis for pension funds, for example, was initiated in 2008, accepted for publication in 2009, and finally published in mid-2011 (Strauss 2011). This slowness is intensified by geographers’ commitments to both collect primary data and interrogate a wide range of theoretical frameworks. The latter is important for the longer term strategic process of “an alternative opening up of economic analysis,” and the former is important for “engaging” with economies and economic problems in all their “real-world” complexity (Peck 2005, 161). Together these commitments can divert effort away from ongoing and sustained analysis of secondary data (e.g., government statistics on pension fund deficits, rates of coverage, and pension inequalities). This analysis, however, is what garners the immediate attention of policymakers and is likely to be of help to groups engaged in campaigning on issues related to pension inequality especially postcrisis. Sustaining a research agenda that combines these foci ideally requires sound training in quantitative and qualitative methods, the latter now encompassing participatory, visual, restorative, and ethnographic methods, as well as media training. This is a hugely challenging pedagogical issue.

Thus, the vibrancy, openness, and political commitments that initially attracted us to economic geography have now become challenging goals that we aspire to reproduce and expand in the context of our own academic praxis. The attempted critical engagement with behavioral economics, for example, remains stubbornly one-sided, but is that an indication of the limits to heterodoxy in the mainstream of the economics discipline? Pensions are deeply political, a lens through which to view processes and institutions of the economy and the welfare state, including—or perhaps especially—during times of economic and financial crisis. Orthodox economics and the strong model of economic rationality coconstruct the ideology of individualism that is undermining collective approaches to welfare provision. This is the political logic of its disciplinary—in the double sense—power. Economic geography, to paraphrase John Holloway, should change the world without seizing power.

The Local Context of Crisis

We find similar challenges in economic geography’s ongoing attempts to explain the impacts of crisis on development trajectories in industrial clusters. Economic geographers have enjoyed substantial influence on the development of theories and policies relating to clusters. Classically geographic works like Saxenian (1994) have no doubt contributed to the proliferation of Silicon Valleys, Alleys, Harbors, Islands, and Glens, but the work of management scholar Michael

Porter (1998) remains the most influential across disciplines and outside the academy. Porter is influential because he focuses on policy outcomes—regional economic growth and development—as opposed to geographers who have been “more diffuse in [their] aims” and therefore less useful to policymakers (Martin and Sunley 2003, 9). Because management is a less theoretically diverse discipline than geography, Porter can focus on “practical” effects and spend less time synthesizing his work with numerous theoretical projects. Meanwhile, geographers, often occupied with the fundamental messiness of local clusters and wary of generalization across space and time, find it more difficult to engage in policy debates.

For economic geographers, local context and contingent evolution in the formation and reproduction of clusters take precedence over improving competitive strategies (e.g., Feldman, Francis, and Bercovitz 2005). It is the mingling of institutional, cultural, and organizational influences that creates a specific context from which place-based practices and tacit knowledges emerge (Gertler 1995). There has been some debate regarding whether economic geographers should concentrate on local contextualization or the creation of generalizable models (see Markusen 1996), but generally the trend among economic geographers has been to focus on case studies of specific clusters rather than model building. In the aftermath of a global financial crisis, however, we need to ask how geographers’ specific, contextualized case studies contribute to a greater understanding of how clusters survive and respond to global or national economic downturns. The challenge is to apply what we have learned from our individual case studies to create a broader understanding of how clusters experienced the crisis without surrendering our “more diffuse aims.”

The Kitchener–Waterloo region (an area better known for high-tech companies like RIM, the maker of the Blackberry) provides a useful example of this problem. During a study of industrial innovation in the region carried out during the crisis, it quickly became evident that the present crisis was simply the latest in a series of crises the region had faced over the past decade (Bathelt, Munro, and Spigel 2011). The region’s industries are heavily dependent on both suppliers and customers in the United States and have been hurt by the strengthening Canadian dollar since 2002, which made their products more expensive. The 2008 financial crisis certainly hurt many of the local manufacturers who supplied the auto industry, but other firms diversified their markets in response to the previous rounds of crisis and were successful in weathering the storm.

In other words, regardless of the global character of the financial crisis (French, Leyshon, and Thrift 2009), Kitchener–Waterloo’s experiences are unique to the region. Economic geographers, however, need to make clear that these individual case studies speak to the wider study of clusters and regions. Kitchener–Waterloo illustrates that a cluster’s response to one crisis is, in part, a reflection of pre-

vious crises. Other industrial clusters, such as those throughout the Rust Belt and the English Midlands, are in the same position as Kitchener–Waterloo. Still others, as diverse as high-tech firms in Silicon Valley, small textile shops in northern Italy, and assemblers in Guangzhou, experienced the present crisis differently but still through the context of previous crises.

When addressing the needs of clusters during the crisis, economic geographers need to emphasize the importance of understanding the cluster’s particular history. This does not necessarily imply a strictly evolutionary perspective but rather an understanding that the past influences the future. Storper and Scott (2009) argued that regional economic dynamism lies in the harmony among local resources, institutions, and skills. Economic geographers’ qualitative, contextual research methods and paradigms give us the tools to investigate a cluster’s history and dynamism to identify potential matches between local resources and talents. Cluster researchers, like Porter (2008), who largely lack this perspective, are instead left to argue for large-scale national reregulation to preserve competitiveness, often overlooking clusters’ and regions’ particular needs. Economic geographers might indeed be challenged to engage development policy at the scales that Porter promotes due to our lack of a clear disciplinary core that would allow for a unified, universal response to current events. We argue, however, that geographers are eminently prepared to offer relevant economic development analysis to actually existing localized economies. The challenge is convincing local partners in regional governments and institutions to pay more attention even though we rarely offer the same neatly packaged solutions as our colleagues in economics and management.

Latin America, Crisis, and the Space of Flows

We finally want to point out that most discussion of the 2008 global financial crisis in economic geography and proximate disciplines as well as the mainstream press has been centered on the United States and Europe. But how and where did the crisis spread? The initial crisis caused a widespread economic shock and subsequent slowdown in growth, but distinct global regions have not experienced the crisis in the same way. Realizing that it is a large and diverse region, we briefly consider why Latin America generally experienced a much less severe crisis compared to the United States and Europe. In particular, we suggest that it is only through considering both the grounded and interconnected nature of Latin American economies at the same time that a full conception of that crisis can be grasped. In other words, we suggest that economic geographers’ tendency toward *relational* analysis (Yeung 2005) provides a crucial and unique perspective on financial crisis even though it sometimes results in conclusions that are “messier” than more straightforward methodologies. This is crucial because the only way

one might successfully grasp the complexity of the relationship between interregional financial flows and particular place-based economies is through a diverse and pluralistic discipline capable of juggling analysis of a grounded “real” economy with unorthodox economic ideas such as “the space of flows” (Castells 1996).

In the case of Latin America, reliance on maritime trade and primary commodity exports protected it from the long-term downturn experienced in the United States and Europe. International trade did contract in the wake of the crisis, and this contributed to a significant economic slowdown in the region, but it turned out to be short-lived. Latin America’s previous unfortunate experience with financial crisis (Jara, Morena, and Tovar 2009; World Bank 2010), coupled with expanding partnerships with China, Russia, Brazil, and other emerging markets, meant that it was also one of the first regions to return to robust economic growth after the crisis. The rise of maritime containerization, much like the rise of financial markets themselves, represents just such a shift in contemporary society toward a world of flows facilitated by information technologies (Taylor 2004). It is precisely the noncentered nature of maritime shipping networks that has contributed to the uneven distribution of gains and losses across the global port network as a result of the global financial crisis (Eyerdam 2010). In fact, some shipping companies conducting trade with ports in Latin America have been “ordering more capacity than is reasonable and ignoring the (generally negative) market signals that began in late 2007” (United Nations Economic Commission for Latin America and the Caribbean 2009, 3). In spite of the negative impact that the financial crisis has had on the shipping industry overall, sea lanes between Latin America and emerging markets in Russia and China, for instance, continued to flourish (Eyerdam 2010).

Furthermore, any analysis of the financial crisis in Latin American states, especially those in South America, must also consider the macroeconomic policy approach undertaken in the wake of the 1997–1998 Asian financial crisis and the 2001–2002 Argentinean currency crisis. Among other things, intense capital flight encouraged many regional governments to more closely monitor and regulate foreign currency flows, asset price bubbles, and excessive credit expansion, all of which “arguably moderated the risk of a boom-bust cycle” (Gallego et al. 2010, 12). In addition, Latin American container shipping ports are still commonly described in relation to their host countries’ economies (Ellis 2009). Eyerdam (2010) observed that the Latin American port sector was down 6.8 percent as compared with 9 percent decrease in trade volume for the shipping industry overall in 2009, which suggests that one cannot look exclusively at shipping networks but must also consider the grounded economies, in this case based on primary commodity exports, where those networks are embedded.

As economic geographers, we are ideally situated to analyze the distinct placed-based nature of different institutions existing as widely dispersed nodes within

a broad network of world commerce. It is the confluence of globe-spanning trade networks with the placed-based nature of economic trading hubs (such as seaports) that we see as a key opportunity for economic geographers to make important contributions. This type of global economic organization—a mix of economic flows across large distances and fixed territorial economies—leads us to better appreciate the realities and relations of economic organization and, as such, reactions to crisis. As Agnew (2005) stated, “It is not a question of either/or but of how one relates to the other” (166). Considering so many variables and relations between them, however, does not make for neatly packaged policy prescriptions. The challenge for economic geographers who strive to make the discipline relevant to a broader and nonacademic audience is how to translate relational and network analyses into language that policymakers and other disciplines find useful.

Conclusion: Rocking the Boat

If economic geography is to play a part in challenging these entrenched orthodoxies [neoclassical economics and neoliberal politics], then it will need to find and make common causes, even as it remains a pluralist project.

—Peck (2005, 166)

If the SIEG proved anything to us, it is that economic geography is a vibrant, exciting, and creative discipline at the cutting edge of social theory and rethinking “the economic.” Not unlike other instances of disciplinary contemplation, though, we also sensed a bit of frustration with the seeming infrequency that good geographic work is “impactful” with broader audiences. This takes on even more urgency at a time when tenure-track jobs continue to be sparse, anti-intellectualism is rampant (at least in the United States), and economic recession and austerity movements continually shrink higher education budgets. It seems that now more than ever we should consider economic geography’s relevance.

This is the context in which we authored this article, and our main motivation was to explore both the up and downside to expanding pluralism. To state it simply, our conclusion is that there are trade-offs to the expanding diversity of the discipline. Most notably, it is increasingly difficult to pinpoint the discipline’s contributions to contemporary debates. The “missing the boat” accusation is just one of many related critiques about relevance. Although we certainly agree, in this particular case, that more geographers should find common causes in researching finance, financialization, and financial crisis, we do not think that the accusation is a fair one. If economic geography were less diverse and more “disciplined,” it might be possible to more quickly and succinctly engage issues like the causes and consequences of the financial crisis. We suspect, however, that it would also be more difficult to square our political and intellectual commitments.

After all, the challenges of theoretical and methodological diversity in economic geography should not be reduced to a choice between oversimplifying our explanations or giving up on potential engagements. Rather, it should be about expanding intellectual conversation, questioning dead-end assumptions, and engaging a wide range of contemporary problems related to finance and crisis. Perhaps a better question is what boat we want to be on. If “catching the boat” requires quick, definitive answers to complex sociospatial issues, answers that all too often leave crisis-causing orthodoxies untrammelled, we might embark, but only on a collision course with neoclassical orthodoxy that we suspect will leave economic geography sunk. On the other hand, we believe that using innovative methodologies to help drive forward debates about the type, role, and accountability of financial institutions as well as the part that finance capital plays in processes of globalization, deindustrialization, and economic restructuring are worthy disciplinary goals, even if they take decades to articulate. The speed might be slower, the launches less dramatic, and the seas rougher than some, especially early career geographers, prefer, but we think that the benefits justify the risks. ■

Notes

¹ By *disciplinary core* we refer to a theoretical and methodological canon, something that we believe economic geography lacks relative to other disciplines, especially other social science disciplines such as economics and sociology. We realize, however, that although economic geography might appear from within to be loosely constituted, heterogeneous, and plural, the external view is of a relatively closed and exclusive milieu with rigidly policed processes of interpellation. This applies to most academic disciplines when observed from the outside.

² Although we are oversimplifying the argument, Peck suggests that by attempting to engage with neoclassical economics, economic sociology, or at least the “new economic sociology” (NES), was “disciplined” into accepting some of the key neoclassical axioms such as rational actors and efficient markets. This made it difficult, if not impossible, to arrive at significantly different conclusions. You might say, following Amin and Thrift (2000), that NES lay down with the lion and ended up as prey.

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