The Relationship Between Internet Access and Divorce Rate

Todd D. Kendall

Abstract The rise of the internet has affected the market for romantic partners, arguably lowering search costs. It has been claimed anecdotally that this has led to an increase in divorce. However, a more careful examination of theory suggests that, even if search costs have declined with the rise of the internet, this would not necessarily increase divorce propensity. To examine the issue empirically, this paper employs both state- and household-level data on family structure from the Bureau of Labor Statistics Current Population Survey. A comparison of high and low internet penetration states, as well as a micro panel of initially married households with and without internet access, reveals no evidence that the rise of the internet has increased divorce.

Keywords Divorce · Internet · Search costs

Introduction

Over the last decade, as home internet access has spread, anecdotal reports of infidelity and divorce associated with the worldwide web have become widespread. Popular accounts of this phenomenon appeared early (e.g., Quittner 1997), and often. For instance, Dedmon (2002) surveyed matrimonial attorneys, finding that 42% of divorce cases in the previous year involved one party meeting a new love interest over the internet. In another survey, Avila-Mileham (2003) found that 28% of married internet users surveyed admitted to engaging in a "cyber affair," and many

of these claimed to have met offline for face-to-face meetings. AshleyMadison.com, a site specifically devoted to matching unfaithful spouses who wish to have affairs, claims 200,000 members in North America. Griscom (2002) argues that the internet is fundamentally changing all aspects of marriage markets. To the extent that divorcees do not fully internalize the negative effects of separation on their spouses, children, and others (Schramm 2006; Couch and Lillard 1997; Hans et al. 2009; Dew 2009), the rise of the internet could then have deleterious effects on society. There is some evidence for a large and increasing role for the internet in the marriage market (e.g., Stevenson and Wolfers 2007 find that 16% of singles use online personals services, while Griscom 2002 finds 20% usage among singles[1]), but the relationship between the internet and divorce behavior has not been fully explored.

This paper presents some of the first empirical estimates based on large-sample U.S. datasets of the effect of the internet on divorce propensity. The analysis proceeds in two parts. The first part considers the relationship between state-level internet access rates and state divorce rates. The second part then analyzes a household-level dataset on over 40,000 marriages to estimate the propensity of divorce as a function of the internet. Both the state- and household-level analyses agree that there is little evidence that internet access is associated with significant increases in the number of divorces. In some specifications, internet access is actually associated with lower divorce propensity.

In contrast with the anecdotal reports described above, a careful approach to this question, grounded in standard

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[1] See also Hitsch et al. (2010), who examine data from a large dating site. All three studies focus on the U.S.; but see Johnson-Hanks (2008), who presents evidence that internet communication has had an effect on marriage markets in developing countries, as well.

theories of the marriage market, shows that the overall effect of the internet on divorce propensity is ambiguous, and so it should be unsurprising that the empirical findings do not support large effects.

In one of the canonical models of marriage behavior, Becker (1973) and Becker et al. (1977) considered the marriage market as a process of repeated search under uncertainty about marital outcomes. In their analyses, agents first choose a reservation value (that is, a minimum acceptable quality level) for a partner, and then repeatedly search among the pool of available partners. Agents choose to marry when a partner of quality exceeding their reservation value is found. The reservation value is lower when searching is more costly, because otherwise, agents would forego the gains from marriage for an unacceptably long period. Thus, Becker (1991) predicts weaker marriages among groups with high search costs, such as those who have a strong taste for marrying a partner of their own race or religion.[2]

Therefore, in the Beckerian model, the cost of searching for romantic partners, both before or after marriage, is a crucial parameter, and indeed, it may be argued that the internet has lowered these costs substantially. Chat rooms such as "Married But Looking" on Yahoo!, classified ad sites such as Craigslist.org, and internet dating sites like Match.com and eHarmony.com allow for quick scans of basic information on many potential matches quickly (there are also several sites, including the aforementioned AshleyMadison.com, specifically designed to match unfaithful spouses). By comparison, pre-internet matching technologies such as personals ads in newspapers or meetings in bars or nightclubs are considerably more costly and usually less informative.[3] In addition, potential partners can now use e-mail and instant messaging to correspond at length before ever meeting in person.[4] The decline in search costs may be especially large for post-marital searchers, since postal mail and face-to-face conversations are typically much less private, and more difficult to conceal from a spouse. At the same time, simply because more options are available does not necessarily imply that search costs have fallen under some psychological theories (Schwartz 2005); however, even if the internet did lead to declines in search

costs, a jump to the conclusion that divorces rates must then rise is inappropriate, even under standard Beckerian theory.

A substantial decline in partner search costs *could* lead to higher levels of divorce for three reasons. First, there is a direct effect on those already married, since they may more easily continue to search after marriage, and if a partner of sufficiently high quality becomes available, the expected benefit from the new match may outweigh the cost of dissolving the old match.[5] South and Lloyd (1995) argue for such an effect in a different context, showing that marriages are more fragile where an abundance of spousal alternatives exist. This appears to be the argument upon which many commentators implicitly rely.

Second, forward-looking married individuals who expect low costs of post-marital search may invest less in marriage-specific "capital," such as children or market/home spousal specialization, which makes divorce more costly. That is, individuals who enter a marriage believing there exists a higher probability of finding a better match later may choose to forego decisions that will make divorce more difficult if and when that opportunity arises.

Thirdly, single individuals who expect low costs of post-marital search may lower their reservation quality level for a marriage partner, knowing that they can continue searching while married (though perhaps at a higher cost than while single). This factor leads to more poor quality marriage matches, and therefore, more divorces.

However, there are also aspects of the internet that may lead to *fewer* divorces. First, as noted by Becker et al. (1977), a decline in search costs should lead to longer searches, and therefore, better ultimate matches. This effect is particularly important for unmarried individuals, who would be matched into higher quality marriages when search costs fall, and thus, be less likely to divorce later, and also more likely to invest in marriage-specific capital, which reinforces lower divorce propensity. Moreover, post-marital search using the internet generally involves more privacy than non-digital search methods. If a post-marital search is conducted primarily for the purposes of finding extramarital affairs, as opposed to finding a higher quality marriage partner, then increased privacy may reduce the likelihood of being caught in an affair, and therefore reduce the likelihood of divorce, if affairs and divorces are net substitutes (Posner 1992).[6]

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[2] This of course does not mean that, among groups who have such strong tastes, those who do marry outside their own race or religion will not have higher levels of marital instability (Jones 2010).

[3] Many internet matching sites require paid subscriptions, as well, though they are generally not very expensive. A subscription to Match.com costs $12.99/month, while Craigslist postings are free, and unlimited messaging on AshleyMadison.com costs $60/month.

[4] Romantic partners may be thought of as "experience goods," in which the value of the match is not fully apparent until after substantial resources have been allocated (see Bei et al. 2004, for a discussion of the value of internet information searches for experience goods).

[5] Alternatively, if one spouse is less satisfied with the marriage than the other, a fall in search costs may simply lead to a greater concentration of within-household bargaining power with the less satisfied spouse. To the extent that such bargaining is costless, this would reduce the importance of this effect of the internet on divorce. See Chiappori and Weiss (2006) for a bargaining model of marriage and divorce.

[6] Avoiding harm to one's spouse is an important factor in extramarital decision-making (Meyering and Epling-McWherter 1985).

Thus, Beckerian theory suggests that the overall effect of the internet on the divorce rate may be ambiguous. Lower search costs for married persons generally lead to more divorces, while lower costs for unmarried persons and greater privacy for married persons lead to fewer divorces. In order to illustrate that the conclusion that the internet causes divorces has been too hastily drawn, I have focused on a single well-known model of marriage; however, the introduction of causal mechanisms associated with alternative theories would imply even deeper uncertainty. This ambiguity justifies an empirical approach, as supplied in this paper.

Method

Overview

My empirical work is driven by the analogous literature on the effects of the internet on job search. Like marriage, employment "matches" between a firm and a worker generally follow a period of search (typically on both sides) among alternatives, with substantial uncertainty about the quality of the match which is not resolved until after the match is made, creating the possibility of subsequent "divorce" (quits or firings). Thus, theoretical models and empirical methods used to analyze job search have been widely applied to study marriage. Following Stevenson (2006), I first estimated a state-level relationship between the share of the state's population with home internet access and the state's divorce rate. While state-level data may involve some limitations in controlling for relevant confounding factors (Foote and Goetz 2005), aggregate data may be the best way to identify relationships in which substantial peer effects may exist, as is likely in the case of divorce.[7]

Continuing to follow the literature on internet job search, particularly Kuhn and Skuterud (2004), I also constructed a sample of over 40,000 married households using four Current Population Survey samples that asked about home internet access. I then matched the households in each survey to subsequent CPS surveys, thus generating a two-period panel, and estimated the conditional instability of households with and without internet access, controlling for a wide variety of household-specific characteristics.

State-Level Approach

Since 1996, the National Center for Health Statistics (NCHS) has published state-level counts of divorces, but has not published any more detail on these divorces, such as the age distribution of divorcees or the number of children involved in divorce. Since the bulk of the increase in home internet adoption in the U.S. has taken place since the mid-1990s, the state-level analysis below is limited to these data. Following Bitler et al. (2004), I focused on the "flow" of divorces—that is, the number of new divorces in a year—instead of the total number of divorced persons in the population, since changes in flows are more easily measured in the short run.

Therefore, I measured the "divorce rate" in state i in year t as:

$$\text{Divorce Rate}_{it} = \frac{\text{Divorces}_{it}}{\text{Married Women}_{it}}$$

where Divorces_{it} is the count of new divorces in state i, year t, from the NCHS data, and $\text{Married Women}_{it}$ is the number of married women in state i, year t, measured by the Annual Demographic Supplement of the March Current Population Survey. Note that the divorce rate is measured here relative to the number of marriages, not the total population, as is commonly calculated. While these estimates of the number of marriages in a state are taken from survey data, and are thus subject to measurement error, calculation of the divorce rate in this way eliminates important biases due to variations in the propensity of single individuals to marry, and the number of young children, across states (see Raley and Bumpass 2003; Bitler et al. 2004; Goldstein 1999 for similar constructions).

Data on internet access is derived from the Current Population Survey's Internet and Computer Usage Supplement, which sampled roughly 50,000 households in four different years: 1998, 2000, 2001, and 2003. The survey asked, "Does anyone in this household connect to the internet from home? (yes or no)."[8] This question does not, obviously, indicate the nature of sites accessed or provide information on which members of the household use the internet, though the latter of these I will examine briefly in the following section.

Table 1 gives summary statistics on the state-level fraction of households with internet access, the divorce rate, and a variety of covariates which will be used in the state-level analysis. In order to illustrate the degree of variation in the share of households with internet access, both between states, and within a state over time, Table 2

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[7] Each divorce creates two newly-single individuals, increasing (marginally) the number of available partners for other still-marrieds. Moreover, each divorce also likely reduces the social stigma associated with divorce, raising the likelihood of divorce among other marriages.

[8] In two of these years, questions were asked about internet usage at work. However, there is not enough data to perform any substantive analysis; moreover, access to chat rooms and other socially-oriented websites at work is generally restricted by employers.

Table 1 State-level summary statistics

	Mean	SD	Obs.
Share of households with internet access	48.18	13.37	184
Share of married households with internet access	52.80	14.27	184
Divorces per 100 married couples	1.91	0.40	184
Poverty rate	11.11	2.93	184
Unemployment rate	4.73	1.10	184
Personal income per capita ($2003)	29,189.55	4,018.27	184
Human capital stock (from Turner et al. 2007)	8.26	0.24	184
Percent population black	13.36	8.14	184
Percent population ethnically Hispanic	10.01	10.10	184
Percent population living in metropolitan areas	78.65	15.38	184
Average number of persons per household	3.32	0.17	184

Note: All values reported are means of annual, state-level observations for the years 1998, 2000, 2001, and 2003, weighted by the number of married women in each state. The human capital variables are calculated using Mincerian earnings equations estimates of the rate of return to schooling and experience (see Turner et al. 2007, for details). All statistics are based on 184 out of a theoretical 204 observations (see note below Table 4 for details)

displays state-level means for each state in the first and last years of the sample. Clearly, the internet expanded much more quickly in some areas than in others. Between 1998 and 2003, the percent of households connecting to the internet more than doubled from 24% to over 64% in North Dakota; by contrast, in New Mexico internet usage only grew from 28 to 47%. The reasons for differential growth rates are varied. Goolsbee (2000) finds that state sales taxes can explain much of the rise in internet commerce, while the results of Goolsbee and Klenow (2002) suggest that peer effects are important in the diffusion of the internet.

For the state-level analysis, I estimated a relationship between internet access and divorce rates of the following form:

$$\ln(\text{divorce rate})_{it} = \beta[\text{internet access}]_{it} + \alpha X_{it} + \gamma_i + \mu_t + \varepsilon_{it} \tag{1}$$

where i indexes U.S. states, and t indexes years. In some specifications, I included X_{it}, a vector of covariates, and γ_i and μ_t, state- and year-fixed effects, respectively. Therefore, in these specifications, the analysis controlled for all variables affecting the divorce rate in a given state throughout the sample period, and all variables affecting the divorce rate nationally in a given year. I employed a weighted least squares technique to estimate Eq. 1, with the weights determined by the number of married women (and thus, the number of marriages) in a state in a given year. I clustered standard errors at the state-level, and used the White correction for heteroscedasticity.[9] As discussed

in the following section, examination of the data clearly shows that Nevada is an outlier in divorces; therefore, in all of the state-level analysis that follows, I excluded Nevada, although the results do not change much when Nevada is included.

Household-Level Approach

As another approach to understanding the relationship between internet access and divorce, I also analyzed the 4 years of the CPS Internet and Computer Usage Supplement at the household level.

The CPS surveys around 50,000 households each month. Each household is surveyed for four consecutive months, followed by an 8 month respite when the household is not surveyed, and then another 4 months of consecutive interviews, after which the household is dropped completely from the sample. The sample is staggered so that, in any given month, roughly 1/8 of the 50,000 households are in each of the 8 survey months.

This longitudinal aspect of the survey allows for the construction of a panel of households which are married during the initial survey months, and of which some become divorced by the last month in sample (see a similar approach as described in Madrian and Lefgren 2000). Therefore, it is possible to estimate conditional averages for the probability of transitioning out of marriage over a period of roughly a year for marriages with and without home internet access.

Of the 50,000 households surveyed during each of the 4 months in which the Internet and Computer Usage Supplement was implemented, roughly 25,000 were in the final 4 months in sample, and so were dropped from the analysis (these households were close to the end of the survey, and

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[9] An alternative Prais-Winsten specification of the error structure, which allows for within-state and temporal correlation across states, with an AR(1) process for within-state correlation delivers essentially identical results as those presented in the following section.

Table 2 Percentage of households with internet access, 1998 and 2003, by state

State	1998	2003	Difference (2003–1998)
North Dakota	24.01	64.52	40.52
Wyoming	28.36	65.89	37.53
Iowa	25.93	63.35	37.42
Nebraska	26.16	63.01	36.85
Minnesota	32.92	69.06	36.14
North Carolina	20.69	56.71	36.02
Wisconsin	28.80	63.79	34.99
South Dakota	26.76	61.50	34.73
West Virginia	21.16	55.83	34.66
Maine	30.86	65.43	34.57
Pennsylvania	29.48	63.35	33.86
New York	27.52	61.23	33.71
Virginia	32.56	66.18	33.62
Kansas	29.78	62.60	32.82
Delaware	28.78	61.37	32.59
Oregon	33.23	65.80	32.56
Connecticut	37.71	70.16	32.44
New Jersey	35.73	68.14	32.41
Ohio	28.47	60.43	31.96
Massachusetts	33.69	65.31	31.63
Oklahoma	23.01	54.57	31.56
Missouri	27.49	58.79	31.30
Georgia	25.79	57.07	31.28
Arkansas	16.83	48.06	31.23
Maryland	36.36	67.52	31.15
California	30.87	61.81	30.94
D.C.	24.36	55.28	30.92
Louisiana	19.19	49.97	30.78
Nevada	28.74	59.32	30.59
Idaho	31.71	62.26	30.55
Kentucky	25.84	56.19	30.36
Montana	24.99	55.32	30.33
Michigan	29.10	59.41	30.30
Tennessee	24.48	54.63	30.15
Rhode Island	33.34	63.39	30.06
Texas	24.78	54.54	29.76
Florida	30.97	60.44	29.47
Hawaii	32.51	61.54	29.03
New Hampshire	43.85	72.87	29.02
Mississippi	14.75	43.34	28.59
Utah	39.25	67.71	28.46
Illinois	29.29	57.20	27.91
Alabama	24.05	51.91	27.86
Colorado	38.60	66.23	27.62
South Carolina	24.11	51.69	27.58
Vermont	37.38	64.77	27.38
Indiana	30.26	57.44	27.18

Table 2 continued

State	1998	2003	Difference (2003–1998)
Arizona	30.92	58.10	27.17
Alaska	47.22	73.30	26.08
Washington	42.36	67.98	25.62
New Mexico	27.53	47.23	19.70

Note: Data are from Current Population Surveys, Computer Usage Supplements

so there is no way to know whether they became divorced a year later). Of the remaining households, between 10,000 and 11,000 in each year met three criteria for inclusion in the analysis below: they responded to the survey questions regarding marital status and the internet, they were "husband/wife primary family" households, and they did not attrit from the sample before their last month-in-sample.[10] Aggregating over the four supplement years, this left 43,552 married couples. Summary statistics on this sample, given in Table 3, show that around 53% of these households had home internet access.

Each of these households was then matched to data from its last month in the sample (that is, the eighth month in which they were surveyed).[11] If, at that time, the household contained one divorced person, and no married adults, then the couple was counted to have experienced a divorce.

This matching method likely undercounts the number of divorces for three reasons. First, if a couple was married in the initial survey, then divorced with one spouse moving out, and if the remaining spouse then remarried before the last month in sample, then the household would appear to have remained married. Second, many couples separate for some time before formally divorcing. Thirdly, and most problematically, a substantial portion of divorcing couples move from their home, and the CPS does not follow individuals after a move.

The sample statistics in Table 3 show that only 0.59% of all initially married households in the surveys apparently became divorced by the last month in sample. This is substantially lower than the 1.91% rate measured in the state level data (see Table 1), most likely for the three reasons discussed above. A way of ameliorating at least

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[10] In order to facilitate cleaner matches across subsequent surveys, I also dropped households in which marital status was imputed, or which included more or less than two married persons, or two married persons and some number of divorced or separated persons. The number of such households is quite small, however.

[11] If the household was in its first month in sample when the internet supplement survey was taken, this would then be 16 months later; if the household was in its second month in sample initially, then the matched survey would be 15 months later; and so forth. I controlled in the analysis below for initial month in sample.

Table 3 Household-level summary statistics

	Mean	SD	Obs.
Household is divorced in year $t + 1$ (×100)	0.59		43,552
Household is divorced or separated in year $t + 1$ (×100)	0.95		43,552
Household has internet access	0.53		43,552
Both spouses white	0.87		43,552
Both spouses ethnically Hispanic	0.08		43,552
Husband age	46.87	13.33	43,552
Wife age	44.52	44.52	43,552
Household income <25 K	0.13		43,552
Household income 25–75 K	0.49		43,552
Household income >75 K	0.27		43,552
Husband college graduate	0.31		43,552
Wife college graduate	0.28		43,552
Husband not in labor force	0.18		43,552
Husband employed	0.81		43,552
Wife not in labor force	0.35		43,552
Husband's usual weekly hours worked	32.54	22.24	43,552
Homeowner	0.86		43,552
No children living in home	0.39		43,552
Number of children living in home	1.32	1.32	43,552
Live in metro area	0.79		43,552

Note: Standard errors presented only for non-discrete variables. All values based on 43,552 married couple households sampled in four CPS Computer and Internet Supplements: 1998, 2000, 2001, and 2003 ("year t"), except for the divorce variables, which are based on resurveys of these households in the following year ("year $t + 1$"). Not all households report income levels, so the means of the three household income variables do not sum to unity

part of the problem is to group together divorced households with separated households (defined similarly to divorced households). If most separated households eventually divorce, then this adjustment is appropriate, although the measured effect may differ slightly due to the temporal lag between separation and divorce (Weinberg and McCarthy 1994). To emphasize that this variable may differ in important ways from the divorce rate used in the state-level analysis, I will henceforth refer to the combined divorce and separation rates as the marital "instability" rate. Table 3 shows that this instability rate is 0.95%, which still suggests a substantial number of divorced households among those that attrit from sample; nevertheless, this adjustment allows for formal analysis of a question that is at least complementary to that addressed in the state-level analysis.

For the household-level analysis, I estimated the following equation:

$$\text{Prob}\big(\text{Unstable}_{i,t+1}\big) = F(\text{internet access}_{it}, X_{it}) \qquad (2)$$

where i indexes households, t indexes the year, and again, X_{it} represents a vector of covariates. The variable $\text{Unstable}_{i,t+1}$ indicates whether household i, which was married in period t, is either divorced or separated in period $t + 1$.

Accurate estimation of Eq. 2 answers the question of whether a married household that has home internet access in year t is more or less likely to become divorced or separated in year $t + 1$, holding constant variables X. As a functional form for F, I selected the Gaussian cumulative

distribution function, and so estimated Eq. 2 using a probit mechanism. I weighted the data using the CPS household sampling weights, and standard errors were clustered at the (state × year) level[12] and corrected for heteroscedasticity.

Comparisons and Limitations of the Two Approaches

Difficulties in measuring divorces in the household-level data, as discussed above, may be contrasted with a number of advantages: first, the dataset is much larger than the state-level analysis; second, a number of relevant covariates, such as the ages of children in each household, cannot be accurately measured in the state-level data. Finally, if it were the case that divorced households had a greater propensity to use the internet, reverse causality might drive some of the state-level results (note however, from Table 1, that internet penetration is higher among married households than the population as a whole). Nevertheless, the household-level data is not subject to such biases.

Both datasets, however, are subject to a number of other empirical concerns. First, if there are unmeasured aspects of a marriage that affect both the likelihood of internet adoption and divorce, the measured relationship between the two variables will be spuriously biased. In various specifications, I controlled for many covariates, but given the limitations of the available data, there may be important

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[12] The results are robust to non-clustered standard errors, or standard errors clustered only at the state level.

selection into home internet access based on unmeasured and omitted variables, as there appears to be for internet job search (Kuhn and Skuterud 2004). Second, if partners to a weak marriage expect a high likelihood of divorce, they may purchase internet access precautionarily in order to search for future matches. None of the analysis that follows can fully distinguish the causal effects of the internet from its precautionary demand.

Before presenting the results below, two important notes of caution are in order. First, since the CPS questionnaire lacks specific data on which sites individuals visit, this limits the precision of the analysis in identifying causal mechanisms. For instance, it is possible that the lower search costs introduced by the internet could lead to more divorces, but that this effect is masked by some other, unrelated, aspect of the internet. Moreover, since the internet is a relatively new innovation, it is possible that long-run effects could differ from the temporal effects I estimate. Nevertheless, if the internet is having substantial effects on divorce propensities, it should be evident in the data.

Results

State-Level Results

Figure 1 displays simple cross-sectional scatterplots of the divorce rate and internet access in each of the four survey years, which offers a means of estimating Eq. 1 visually (albeit in levels instead of natural log, and excluding any covariates). In each year, the simple relationship between these variables is clearly negative. In addition, this figure shows that Nevada seems to be an outlier in the distribution of divorce rates. Therefore, in all state-level analysis following, I excluded Nevada, although this decision does not affect the results appreciably.

Table 4 presents regression results for Eq. 1 using the state-level data. In the first column, I confirm the visual impressions from Fig. 1 by estimating Eq. 1 with only year fixed effects, and no covariates. The inclusion of year fixed effects implies that all factors affecting divorce across the entire country in a particular year are controlled for; thus, the regression essentially uses only variation across states to estimate the coefficients. The estimated coefficient on internet access may be interpreted to mean that a 10 percentage point increase in the share of a state's population with internet access is associated with an 11.6% decline in the divorce rate. T-statistics are presented in parentheses under each coefficient.

This analysis ignores the fact that, for instance, since internet access is costly, wealthier states are more likely to have high internet usage rates. As financial matters constitute a major factor in divorce (Seery et al. 2008; Gudmunson et al. 2007; Dew and Price 2010), it may be important to control for such differences across states. Column 2 does just this, estimating Eq. 1 while controlling for all time-invariant factors about each state with the inclusion of a state-specific constant term ("fixed effect"). The inclusion of state-fixed effects means that the coefficient estimates are essentially derived from variation in internet access levels within a given state over time (after

Fig. 1 Scatterplots of divorce rates and internet access rates, 1998–2003. Note: Share of households with internet access from Current Population Surveys, Computer Usage Supplements
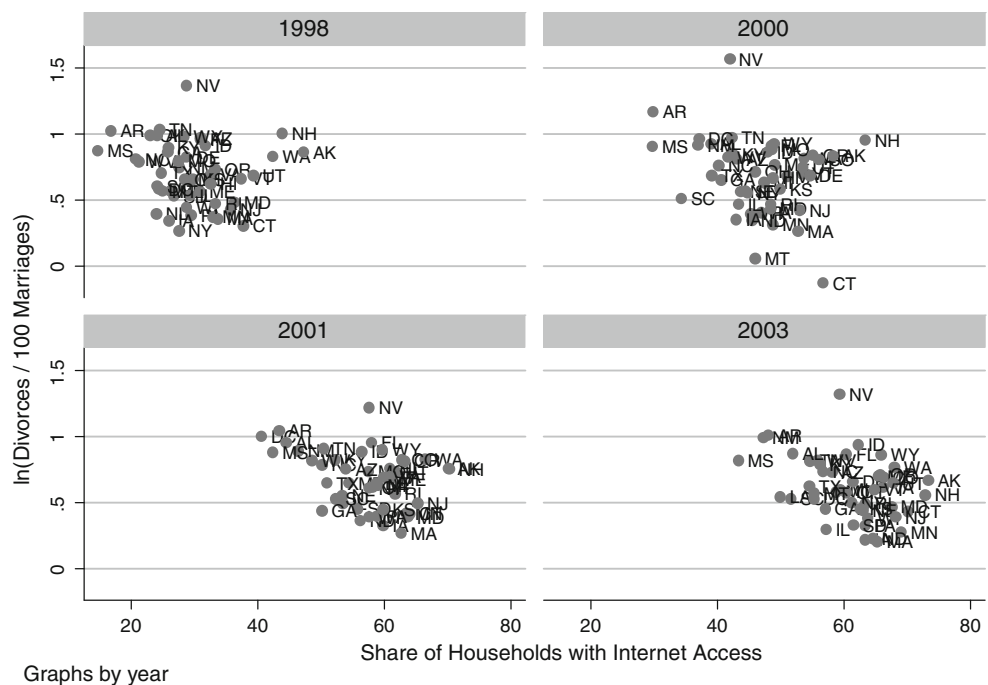
Table 4 Estimates of an empirical model of divorce using state-level data dependent variable: ln(divorces per 100 married couples)

	[1]	[2]	[3]	[4]	[5]
Share of households with internet access (×100)	−1.16* (2.36)	0.63 (1.01)	0.42 (0.83)		
Share of married households with internet access (×100)				0.64 (1.28)	0.20 (0.50)
Ln(real income per capita) [$2003]			−0.25 (0.36)		−0.15 (0.14)
Poverty rate			−0.02 (1.63)		−0.01 (1.51)
Unemployment rate			−0.02 (0.66)		−0.01 (0.33)
Human capital stock			0.18 (1.31)		0.18 (0.89)
% Population Black			−0.09 (1.84)		−0.11 (1.55)
% Population Hispanic			−0.02 (1.72)		−0.01 (1.20)
% Population in metro areas			−0.01 (0.82)		−0.01 (0.66)
Mean household size			0.11 (0.70)		0.04 (0.27)
Constant	0.99** (7.09)	0.53 (1.87)	2.60 (0.37)	0.51* (2.09)	4.14 (0.48)
Age distribution of population?	No	No	Yes	Yes	Yes
State fixed effects?	No	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes
R^2	0.11	0.89	0.91	0.89	0.92

Note: T-statistics in parentheses. Asterisks indicate significance levels: * $p < .05$, ** $p < .01$. The data set is comprised of 184 state-level (including District of Columbia) observations for 1998, 2000, 2001, and 2003. Out of a theoretical possible 204 observations, 20 are unusable due to missing data on the number of divorces. Data from Nevada, an outlier with unusual marriage patterns, is also excluded. In columns [3]–[5], the age distribution of the population is controlled for with the fraction of population in each 5 year age group, from 0–4 to 70–74. Estimation is performed by weighted least squares, with weights determined by the state-level population of married women. Standard errors are clustered at the state level and are robust to heteroskedasticity

differencing out national averages with the year-fixed effects). The results of this analysis indicate that internet access is no longer associated with divorce rates in any statistically significant way. Therefore, a simple bivariate analysis is misleading, and controlling for differences across states appears to be important.

This result, that internet access does not seem to affect divorce rates, conditional on state-specific factors, continues to hold when a number of time-varying demographic and economic factors that might affect divorce are controlled for in column 3, including the state unemployment rate; educational variables such as the human capital stock (from Turner et al. 2007); demographic variables such as the percentages of the population that are black, ethnically Hispanic, and live in metropolitan areas (as calculated from the annual Current Population Survey March Demographic Supplements); the age distribution of the population, measured by the fraction of total residents in each 5-year age group between 15–19 and 50–54; and the average number of individuals per household. Means and standard deviations on these covariates may be found in Table 1.

In general, the inclusion of many control variables, as in column 3, is likely to provide a better statistical test of a given hypothesis, since if these variables were omitted, they could covary with both internet access and divorce, and thus generate spurious correlations. However, it is also possible that the inclusion of these covariates, especially those that may be collinear with internet access such as income, may substantially reduce the signal-to-noise ratio in the regression, and so bias the measured effect of the internet towards zero. Table 5 presents a matrix of correlation coefficients between the relevant variables, indicating no highly collinear variables; however, this does not account for the state- and year-fixed effects, which absorb a substantial amount of variation in the estimation procedure.

Therefore, as a check on this potential problem, I separated out the eight individual covariates from column 3, plus the set of variables measuring the age distribution of the population, leaving only the internet variable and the state and year fixed effects. Then, in the style of Leamer's (1985) "extreme bounds analysis," I ran regressions for every possible specification of Eq. 1 including in the covariate matrix X each subset of these nine controls—thus, I ran $2^9 = 512$ regressions. Such an analysis represents a completely "agnostic" view of whether each variable should be included in the regression. Figure 2 plots the distribution of the 512 coefficients on the internet variable in these regressions. As can be seen, the coefficients vary widely between −0.06 and 0.82, with the median regression producing a coefficient of 0.37. However, not a single regression produced a statistically significant relationship between internet access and divorce (the highest t-statistic was 1.29 on a coefficient of 0.69). Therefore, the evidence

Table 5 Matrix of correlation coefficients between variables used in state-level analyses

	Divorce rate	Internet access	Income per capita	Poverty	Unemployment	Human capital	% Black	% Hispanic	% Metro	Household size
Divorce rate	1.000									
Internet access	−0.236	1.000								
Income per capita	−0.574	0.365	1.000							
Poverty	0.439	−0.531	−0.472	1.000						
Unemployment	0.020	0.316	−0.110	0.158	1.000					
Human capital	−0.392	0.673	0.565	−0.702	0.103	1.000				
% Black	0.079	−0.165	−0.005	0.254	0.065	−0.102	1.000			
% Hispanic	0.105	0.002	0.057	0.396	0.209	−0.322	−0.131	1.000		
% Metro	−0.255	0.188	0.644	−0.196	0.021	0.244	0.146	0.354	1.000	
Household size	−0.206	−0.070	0.073	0.068	0.052	−0.319	−0.229	0.431	0.160	1.000

Note: The data set is comprised of 184 state-level (including District of Columbia) observations for 1998, 2000, 2001, and 2003. Out of a theoretical possible 204 observations, 20 are unusable due to missing data on the number of divorces. Data from Nevada, an outlier with unusual marriage patterns, is also excluded. Figures represent weighted correlation coefficients, with the weights determined by the state-level population of married women
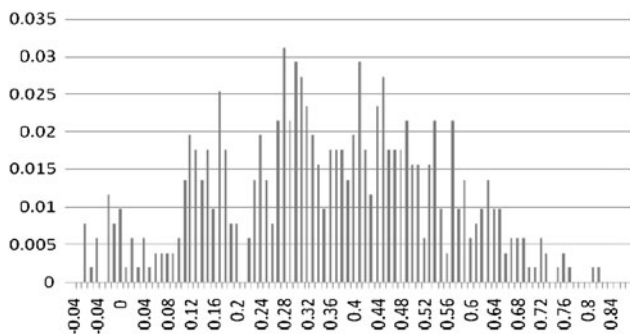

Fig. 2 Extreme bounds distribution of coefficient on internet access variable. *Note*: Histogram of estimates of the effect of internet access rate on ln(divorce rate) from Eq. 1, using all possible subsets of the eight covariates listed in Table 4 and a single set of variables for the age distribution of the population, as described in the paper. This figure therefore represents the results of $2^9 = 512$ regressions. Each histogram bar is 0.01 wide

for a relationship between these variables at the state level should be considered fragile, at best.

Since divorced households might have a different propensity to purchase home internet services, reverse causality might bias the results displayed in the first three columns. As a robustness test, columns 4 and 5 again present estimates of Eq. 1, but instead of the share of all households with internet access, the share for married households only is employed. Again, there is no apparent effect of internet access in married households on the divorce rate.

Household-Level Results

This subsection approaches the relationship between internet access and divorce using household-level panel data. Recall that, as discussed above, divorces are difficult to identify in these data, so I will rely substantially on a broader measure of marital instability, at the cost of reducing comparability with the earlier state-level results.

Table 6 presents estimates of Eq. 2—specifically, marginal effects measured at the means of continuous variables, and discrete effects for indicator variables (such as internet access). All results are multiplied by 100, such that the implied effects are in percentage points. *T*-statistics are presented in parentheses under each coefficient.

In column 1, no covariates (other than year-fixed effects and initial month-in-sample controls) are included. Recall that in the state-level analysis above, the simple bivariate relationship evinced a negative relationship between internet access and divorce. Similarly, it can be seen that in column 1 of Table 6, it appears that married households with internet access are less likely to divorce within the year, although this effect is statistically insignificant here. In column 2, I included a number of other covariates, including some measurements not available at the state level such as the hours worked in the household (see Berry et al. 2008, for evidence that labor force participation is important to family outcomes).[13]

I also included state-level fixed effects in order to control for all omitted variables associated with the household's state of residence, including, for example, state income taxes or state-level social norms. As with the state-level data analyzed previously, including state fixed effects

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[13] In an alternate specifications, I also considered controlling for the husband's and wife's industry and job function (see Young and Wallace 2009 for indications that different types of employment are relevant); the results of that exercise were similar to those presented here.

Table 6 Estimates of an empirical model of divorce using household-level data dependent variable: indicator for divorce at end of sampling period

	[1]	[2]	[3]	[4]	[5]
Internet access at home?	−0.26 (1.49)	0.02 (0.23)	0.05 (0.44)		
Husband frequent internet user				−0.35* (2.12)	−0.18* (1.97)
Wife frequent internet user				0.05 (0.95)	0.04 (0.20)
White couple		−0.15 (0.99)	−0.13 (0.80)		−0.08 (1.66)
Hispanic		0.08 (0.44)	0.06 (0.31)		−0.001 (0.11)
Wife age		−0.02* (2.09)	−0.02 (1.93)		−0.02 (1.34)
Husband age		−0.01 (0.47)	−0.01 (0.53)		0.02 (0.92)
Income <25 K		0.80** (2.83)	0.74* (2.10)		0.75 (1.26)
25 K ≤ income < 75 K		0.23 (1.31)	0.27 (1.65)		0.49 (0.93)
Income ≥75 K		0.26 (1.20)	0.22 (0.97)		0.21 (0.40)
Husband college grad		−0.13 (1.00)	−0.13 (1.11)		−0.06 (0.75)
Wife college grad		−0.11 (0.94)	−0.11 (0.90)		−0.62 (0.89)
Husband NILF		0.63 (1.16)	0.70 (0.92)		0.33 (0.41)
Wife NILF		−0.25* (2.43)	−0.28** (2.79)		−0.89* (2.14)
Husband employed		0.30 (0.89)	0.32 (1.12)		0.70 (0.50)
Husband usual hours worked		0.001 (0.22)	0.001 (0.28)		−0.01 (0.87)
Homeowner		−0.81*** (4.58)	−0.53** (2.73)		−0.34* (2.12)
No children		0.34* (1.96)	0.35 (1.85)		0.20* (2.12)
Total number of children		0.08 (0.73)	0.08 (1.47)		0.42 (0.45)
Metropolitan status		0.09 (0.73)	0.09 (0.76)		0.05 (0.71)
State fixed effects?	No	Yes	Yes	No	Yes
Year fixed effects?	Yes	Yes	Yes	–	–
Initial month-in-sample fixed effects?	Yes	Yes	Yes	Yes	Yes
Observations	43,552	43,552	46,971	4,007	4,007

Note: *T*-statistics in parentheses. Asterisks indicate significance levels: * $p < .05$, ** $p < .01$, *** $p < .001$. Regressions in columns [1] and [2] are based on CPS surveys of 43,552 married couple households. Column [3] estimates are from a multinomial probit and so also include observations on missing/attrited households. Columns [4] and [5] use only data from the 2003 CPS survey. The dependent variable is an indicator taking the value of 1 if the couple became divorced or separated in a resurvey the following year. Standard errors are clustered at the year × state level and are robust to heteroskedasticity

changes the sign of the measured effect, and there is no significant effect of internet access on divorce propensity. In unreported analysis, I also considered other covariates, including more narrowly defined income and education level variables, the number of children in each 3-year age group, and the population of the resident metropolitan area. The findings are robust to inclusion of these variables.

Since attrition from the sample among divorced couples is a concern, column 3 presents estimates of Eq. 2 again, but changes the dependent variable to a categorical indicator that takes different values if the couple remains married, separates/divorces, or attrits. Since there is no natural ordering of these outcomes, I estimated this relationship with a multinomial probit design, although for clarity I displayed only the estimated effects of internet access on separation and divorce. Again, the results show no statistically significant effect of the internet variable.

In the 2003 survey, the CPS asked respondents not only whether they had home internet access, but how frequently

they used it. Specifically, the question asked of each household member, "How often did you usually access the Internet over the last year?" Respondents could answer the question in one of four ways, and for each, the share of married couples in which at least one partner gave each answer is as follows: "at least once a day" (54.72%), "at least once a week, but not every day" (33.71%), "at least once a month but not every week" (6.98%), and "less than once a month" (4.60%).[14] As another test of the relationship between the internet and divorce, I used these data to compare the marital instability of "frequent" internet users—those 54.72% who reported daily access—to infrequent users (all others with home access), again using the empirical model of Eq. 2. Columns 4 and 5 display the results. The estimates in column 4 indicate that married couples with frequent internet users are 0.35% *less* likely to divorce or separate than other internet users, and this effect

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[14] Shares for married couple wives are very similar.

is statistically significant. Controlling for the other covariates, as I did in column 5, reduces the estimated magnitude of this effect, but still evinces a significant effect.

Since this question was only asked in 2003, there is not enough data to reliably extrapolate these results to a general rule, but they are certainly inconsistent with the anecdotal stories of widespread internet-driven divorces.[15] Thus, the general finding from the household-level sample is that there is little evidence that the rise of the internet has had much impact on divorce propensities in the United States.

Discussion and Implications for Future Research

The empirical results presented above show that there is little to no evidence that access to the internet has significantly raised the divorce rate above what it would have been in the absence of this new technology. When examined without any controls, internet access appears to be correlated with lower divorce propensity; however, this appears to be largely an artifact of the fact that higher-income households (which have lower divorce propensity) achieved internet access quicker than other households. Once sensible controls (including measures of income) are included in the analysis, internet access is no longer related to divorce in a statistically significant manner.

The mass-market introduction of the worldwide web in the late 1990s changed the manner by which romantic partners searched for each other. Some commentators have suggested a "dark side" to internet dating—the potential for a decline in post-marital search costs to cause divorces. While such a result is theoretically possible, a decline in search costs also could have affected sorting into marriage by unmarried persons, as well as had a variety of other subtle effects. Thus, empirical evidence is paramount in understanding the effects of the internet.

If the internet were having substantial effects on divorce, these effects would likely be visible in the analysis presented here. The fact that they are not empirically visible suggests that the true effects of the internet on divorce propensity are likely not a cause for alarm at this point. Nevertheless, limitations in available data and methods imply that smaller effects cannot be fully ruled out. Available data on internet usage does not identify specifically which sites are visited by users, nor would such data be simple to collect even with a focused study, given the surreptitious nature of post-marital search. Moreover, in

this study, identification of divorces in the household-level sample is incomplete and the state-level sample suffers from a small sample size due to the limited number of years since widespread adoption of the internet. These facts also imply that the results presented in this paper can only identify short-term effects of internet access, leaving open the possibility of different or opposite long-term effects. Further research will be necessary as more data become available. Nevertheless, speculation about the effects of new technology on marriage markets should be strictly grounded in theory and supported by empirical evidence.

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[15] Moreover, one must take care to distinguish the possibility that intensive use of the internet is associated with variables unobserved in the data, such as running a home-based business (Fitzgerald and Winter 2001).

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